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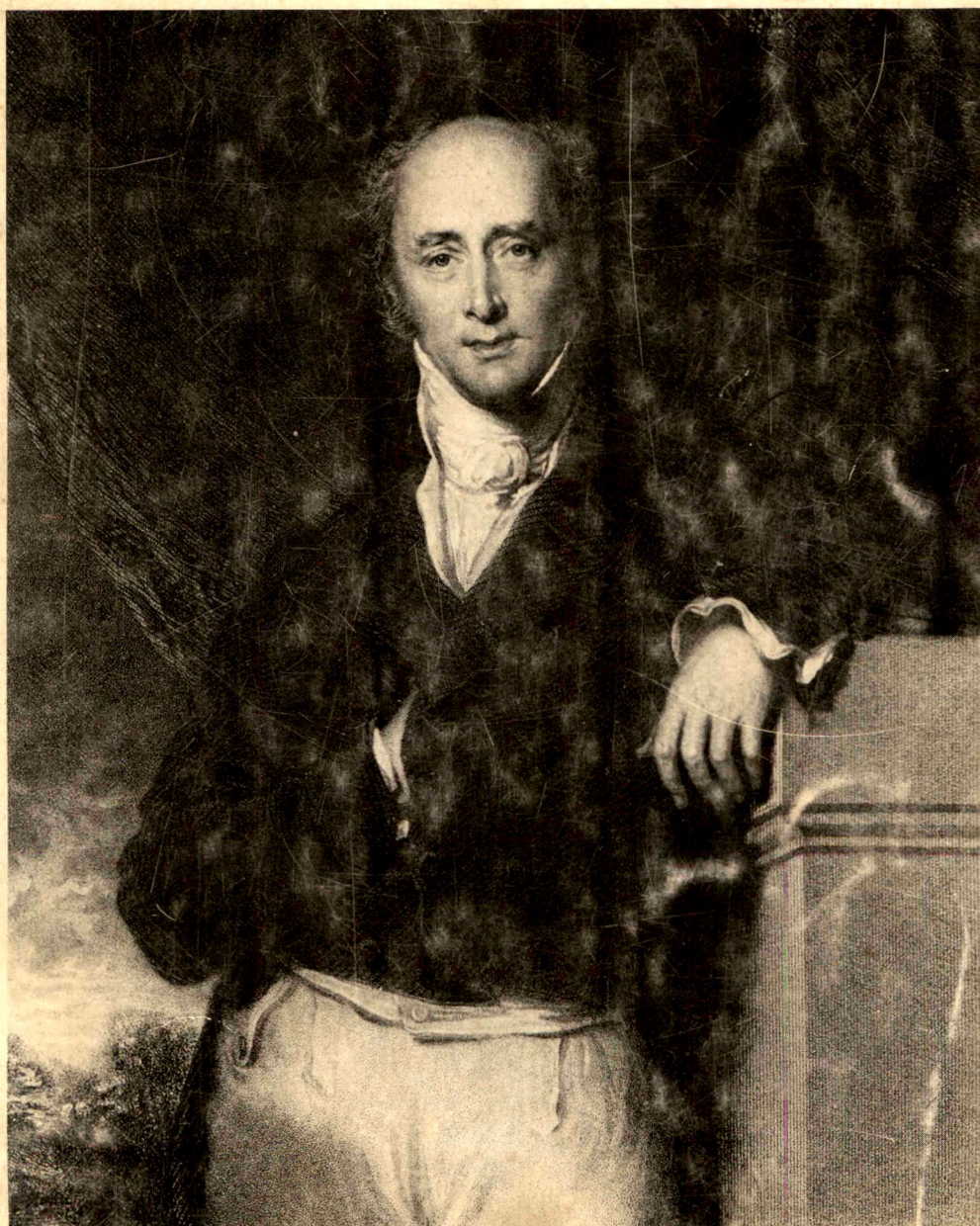
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Ideology and Theory: The Tension between Political and Economic Liberalism in Seventeenth-Century England

JOYCE APPLEBY

DURING THE LAST TWO DECADES of the seventeenth century—ninety years before the *Wealth of Nations* appeared—a number of British writers challenged the central premises of the balance-of-trade theory for economic growth. The currency crisis and the craze over Indian cottons had sharpened a sense of conflicting interests among Englishmen, and these divisive issues called forth a body of writing which attacked the principles underlying the mercantile system. Examining in a new way the operation of the market, Dudley North, Nicholas Barbon, Dalby Thomas, Henry Martyn, Francis Gardner, James Hodges, Henry Layton, John Houghton, and several anonymous pamphleteers produced explanations of economic relations which were far more sophisticated than the prevailing theories, anticipating at many points the premises of Adam Smith's monumental synthesis.

Yet despite this new plateau in economic reasoning, the conceptually flawed balance-of-trade theory, with its built-in corollary that economic regulation was essential to national security, became even more firmly fixed in the public mind in the eighteenth century. The ideological implications of the rejection of these writings have not been explored. Scholars have assumed the science of economics had to wait for the path-breaking geniuses¹ or that the balance-of-trade critics were too exceptional to treat as a significant group.² Charles Wilson recently described Dudley North as a swallow who did not produce a summer.³ The analogy is worth pursuing. Taking a closer look at the birds in hand, it is possible to conclude that it was not the lack of swallows that counted, but the distaste for summers, that, in fact, economic thought ran ahead of social developments, and the analytical insights of the

I am indebted to William Kennedy, Margaret Gay Davies, Richard Steele, and, especially, Andrew Appleby for their careful reading of this article in manuscript.

¹ Eli Heckscher, *Mercantilism* (London, 1935), 1: 104ff; Jacob Viner, *Studies in the Theory of International Trade* (New York, 1937), 90, 117–18. This interpretation is implicit in Bruno Suviranta, *The Theory of the Balance of Trade in England* (Helsingfors, 1923). See also Alexander Gerschenkron, "History of Economic Doctrines and Economic History," *American Economic Review*, 59 (1969), 2; and William Letwin, *The Origins of Scientific Economics* (London, 1963), 144–48.

² J. D. Gould, *Economic Growth in History* (London, 1972), 220–22; Charles Wilson, *England's Apprenticeship, 1603–1763* (New York, 1965), 184.

³ *Ibid.*, 266.

balance-of-trade critics were dismissed because they threatened the fragile social order in England during the country's critical passage into a fully capitalistic society.

The balance-of-trade explanation of how nations grow wealthy had focused attention upon production in such a way as to obscure the dynamics of consumption. Inside England the most noticeable consumers were the very rich and the very poor, and there was little in their patterns of spending to encourage a re-evaluation of consumption. As landowners, the rich could tap agricultural revenues first. Their rent rolls were the principal source of capital, but they spent rather than invested their income.⁴ The very poor were a conspicuous drain upon the economy because there were so many of them, and their subsistence needs were paid for through taxes.⁵ Gregory King's estimate that half the families in England could not pay for their living indicates the dimension of the problem of underemployment.⁶ These realities made plausible the argument that since domestic consumption took from the store of English capital through luxury buying and the maintenance of the poor, markets for English goods should be sought outside the country. In other words, let the social overhead and upper-class vanity of other nations return a profit to England. Such a prescription fit well with the endemic political rivalries of seventeenth-century European states. Blocked from appreciating the role of domestic consumption, economic thinkers slipped easily into the assumption that consumption was a necessary evil, growing—if at all—in response to population growth.

Thomas Mun, Gerald de Malynes, and Edward Misselden had analyzed the influence of demand upon prices in their famous debates over the foreign exchange in the 1620s. In *England's Treasure by Forraign Trade*, Mun also drew attention to the way elasticity of demand influenced foreign consumption, as did Mun's contemporary, Rice Vaughan. But none of these writers of the early seventeenth century or their immediate successors dealt comprehensively with the relation of supply and demand.⁷ As long as domestic trade was considered analogous to taking in each other's washing, there was no way to consider increased spending as beneficial.⁸ Instead total demand appeared

⁴ Sir William Petty referred to transferring wealth through taxation "from the Landed and Lazy, to the Crafty and Industrious," *A Treatise of Taxes & Contributions* (London, 1662), 19. A similar sentiment was expressed by Sir Dalby Thomas in *An Historical Account of the Rise and Growth of the West-India Colonies* (London, 1690) in *The Harleian Miscellany* (London, 1809), 2: 359. He explained that when it is said "people are the wealth of a nation, it is only meant, laborious and industrious people; and not such as are wholly unemployed, as gentry, clergy, lawyers, serving men, and beggars, etc." See also Sir Francis Brewster, *Essays on Trade and Navigation* (London, 1695), 52.

⁵ For contemporary estimates of the Poor Law burden, see [William Carter], *England's Interest Asserted, in the Improvement of its Native Commodities* (London, 1669), 10; [Sir Humphrey Mackworth], *England's Glory* (London, 1694), 24. For a modern estimate, see Wilson, *England's Apprenticeship*, 235.

⁶ King's figures are reproduced and analyzed in Peter Laslett, *The World We Have Lost* (New York, 1973), 36–40.

⁷ Thomas Mun, *England's Treasure by Forraign Trade* (London, 1664), 84–86; Rice Vaughan, *A Discourse of Coin and Coinage* (London, 1675) in John R. McCulloch, ed., *A Select Collection of Scarce and Valuable Tracts on Money* (London, 1856), 82.

⁸ For contemporary assertions that selling to one another is "mere consumption" without enrichment, see Sir Thomas Culpeper, *A Discourse, Shewing the Many Advantages Which Will Accrue to This Kingdom by the Abatement of Usury* (London, 1668), 2–3; Sir William Petty, *Political Arithmetick* (London, 1690), 82ff; John

inelastic. The rich were expected to buy their luxuries, the poor to have enough to subsist. The possibility that at all levels of society consumers might acquire new wants and find new means to enhance their purchasing power which could generate new spending and produce habits capable of destroying all traditional limits to the wealth of nations was unthought of, if not unthinkable.

During these same years that the balance-of-trade theory served as the principal explanation of economic growth, important social sentiments had become embedded in the prevailing ideas of how nations grew wealthy and powerful. Behind the balance-of-trade theory, there lay a model of the national economy which supplied the principal moral support for mercantilistic regulation. Since national wealth was believed to accrue only from the annual net gain from foreign trade, the whole economy could be conceived of as a kind of national joint-stock trading company. In this view, members of society did not interact with each other, but rather participated, one with another, in England's collective enterprise of selling surplus goods abroad. As in a company, the administration was formal. There was little of Adam Smith's awareness of individuals with personal motives working purposively on their own. Rather economic writers approached the problem of promoting national growth much as a factory foreman might view meeting a production quota. Reading through dozens of proposals for promoting English production one is forcibly struck by the absence of concern for the problem of marketing the projected increase in goods. Emphasis fell exclusively upon mobilizing labor and exploiting new resources: lowering interest rates would stimulate land improvements; attracting foreign craftsmen would introduce technical skills; agricultural diversification would relieve dependence upon outside suppliers.⁹ With such a model at the back of their heads, these writers repeatedly elaborated schemes for putting people to work. Houses for the "orderly management of the poor" was a favorite theme. Even more indicative of the national management attitude were the frequent suggestions for a national fishery. Not only would it absorb the labor of weavers' apprentices in off-season, but one writer even suggested that the footmen of the gentry could rise early and employ their idle hours making nets, as could "disbanded soldiers, poor prisoners, widows and orphans, all poor tradesmen, artificers, and labourers, their wives, children, and servants."¹⁰

Cary, *An Essay on the State of England* (Bristol, 1695), preface; [John Pollexfen], *England and East India Inconsistent in Their Manufactures* (London, 1697), 20; *The Profit and Loss of the East-India Trade* (London, [1699]), 8-9; *Certain Considerations Relating to the Royal African Company of England* ([London, 1680], 1; Brewster, *Essays*, 50-52. In the 1690s the word "consumption" loses its pejorative connotation.

⁹ For example, see Culpeper, *A Discourse*, 5; Samuel Fortrey, *Englands Interest and Improvement* (London, 1673) in McCulloch, ed., *A Select Collection of Early English Tracts on Commerce* (London, 1856), 234-36; *Britannia Languens* (London, 1680), in *ibid.*, 298ff; *Angliae Tutamen* (London, 1695), 29. Suviranta, *Theory of the Balance of Trade*, 153-54, errs, I think, in saying that prior to Jacob Vanderlint little attention was paid to the economic value of land.

¹⁰ James Puckle, *England's Path to Wealth and Honour* (London, 1700), in Walter Scott, *A Collection of Scarce and Valuable Tracts* (London, 1814), 11: 380. See also Roger Coke, *A Detention of the Court and State of England* (London, 1694), 2: 494-95; *Reasons for a Limited Exportation of Wooll* (n.p., 1677), 18-20; Richard Haines, *England's Weal & Prosperity Proposed* (London, 1681), 6-7.

This joint-stock enterprise was a powerful image, for it provided symbolic cohesion to a society being atomized by the market. Merchants and industrialists were able to establish their place in the social order in reference to this model, and the laboring poor could find in their disciplined effort an avenue of grace. Effortlessly entwining religion with the social benefits of productivity, Slingsby Bethel railed at popular feastings because they provoked the wrath of God, wasted time, dulled wits, and made men "unfit for action and business, which is [the] chief advancer of any Government."¹¹ Where religion failed to secure the necessary habits, laws were expected to supply the deficiency. William Sheppard urged double indemnity for those who bought wares knowing that they could not pay for them. Those who lived high, he said, should be taxed as long as they continued their excesses.¹² In a similar vein John Scarlett proposed discriminating among defaulters on the basis of the use made of the dissipated funds, those running into debt for riotous living being subject to the "utmost extremity" of the law.¹³ The morality of the market was quietly fused with the morality of the marketman's God.

The balance-of-trade theory explained how increasing exports alone could increase England's wealth and at the same time provided a rationale for organizing labor and legislating market patterns. Associated with it were appeals to patriotism and a justification of existing economic roles. Where the theory failed was in its ability to explain English economic growth. In the closing decades of the seventeenth century, real income, domestic spending, and foreign exports rose together. From John Graunt in 1662 to William Petty in 1682 to Charles Davenant in the next decade the wealth of England drew comment.¹⁴ Every index of economic growth showed an advance—agricultural output, capital investment, imports from the Indies and the New World, and the range and quantity of home manufacturing.¹⁵ Most striking was the abounding evidence of a rise in domestic consumption. What had happened to the store of wealth consumed by the London fire? Contemporaries saw it splendidly replaced before their very eyes. And the rebuilding of London was but the most spectacular testimony to the fact that Englishmen were generally enjoying a higher standard of living. This growth posed questions beyond the explanatory power of mercantilist theory.

In the 1670s some writers, responding to the obvious, if uneven, economic growth, began to speculate upon the dynamic effect of increasing demand. The word "markets" in their pamphlets subtly changed from a reference to the point of sales to the more elusive concept of expandable spending. In the next decade a controversy over East Indian imports grew into a raging debate

¹¹ [Slingsby Bethel], *The Present Interest of England Stated* (London, 1671), 12–13.

¹² William Sheppard, *Englands Balme* (London, 1657), 147, 178.

¹³ John Scarlett, *The Stile of Exchanges* (London, 1682), 321.

¹⁴ K. G. Davies, "Joint-Stock Investment in the Later Seventeenth Century," *Economic History Review*, 2d ser., 4 (1952), 284–85; for contemporary comment, see Petty, *Political Arithmetick*, 96–99; [Charles Davenant], *An Essay on the East-India-Trade* (London, 1696), 8–10; [William Carter], *The Great Loss and Damage to England by the Transportation of Wooll to Forreign Parts* (n.p., 1677), 12.

¹⁵ Wilson, *England's Apprenticeship*, 185; R. M. Hartwell, "Economic Growth in England before the Industrial Revolution," *Journal of Economic History*, 29 (1969), 25; Gould, *Economic Growth*, 156–77.

on domestic consumption. According to traditional writers, the villain of the piece was the East India Company. Not only did the company enjoy a monopoly of the trade to India, but the nature of its trade—exporting bullion in return for imports competitive with English goods—ran athwart the most cherished principles of the balance-of-trade concept. From the point of view of those in the English woolen and silk industries the company's greatest crime was introducing the English public to the light, colorful, cheap fabrics of India. By 1690, the taste for chintz, calico, and muslin had reached epidemic proportions. What had begun as an inconspicuous use of cotton for suit lining had given way to a gaudy display of printed draperies, bedspreads, tapestries, shirts, and dresses.¹⁶ With marketing expertise equal to Macy's, in the twentieth century, the managers of the East India Company had sent English fabric designers to India to direct the Indian craftsmen in reproducing patterns especially admired at home. The impact upon employment in England was strong. The popularity and competitive advantages of imported cottons led to a glut in the home market for woolens and silks. Contemporaries complained that thousands of workers in the two domestic industries were thrown onto the parish for support. The effected producers wanted a flat prohibition on domestic imports of Indian cottons. Their advice to the East India Company was to sell their calicoes abroad where cheap textiles would undermine the native industries of Great Britain's trade rivals.¹⁷

Although it is difficult to learn from contemporary pamphlets whether the poor were important for working up manufactures or manufacturing important for employing the poor, increasingly after 1660 the emphasis fell upon the importance of expanding opportunities for work. Bans on the export of English raw materials and proposals for replacing foreign imports with domestic substitutes were advanced on the ground that they would increase employment. From this point of view, items requiring more labor were socially more useful than those requiring less. Even labor-saving devices were suspect. The woolen and silk manufacturers drew upon this rationale in fighting the East India Company. They stressed the unfairness of searching out places which could undersell English commodities and questioned why manufacturing ought not be promoted "in England [rather] than in India."¹⁸ Charles Davenant was obviously thinking within the traditional theoretical framework when he asserted that cheap imported textiles "freed" more English woolens for foreign export, but other writers recognized that the clothiers could only be convincingly answered by moving outside the balance-of-trade logic altogether.¹⁹

¹⁶ P. J. Thomas, *Mercantilism and the East India Trade* (London, 1963), 30, 51; Suviranta, *Theory of the Balance of Trade*, 7. Both Thomas and Suviranta pointed out the stimulus of the East India trade to economic reasoning in the seventeenth century.

¹⁷ *The Great Necessity and Advantage of Preserving our Own Manufacturies* (London, 1697), 6-10; [Thomas Smith], *England's Danger by Indian Manufactures* (n.p., [1698]), 2-7; [Pollexfen], *England and East India*, 18-20; *Reasons Humbly Offered for the Passing of a Bill* (London, 1697), 7-23; *An Answer to the Most Material Objections* (n.p., [1699]), 1.

¹⁸ *Ibid.*, 2.

¹⁹ Thomas, *Mercantilism and the East India Trade*, 81.

In *Considerations on the East-India Trade*, Henry Martyn made a full frontal attack on the theory of the social utility of high labor costs by examining the differentials in domestic consumption. Conceding that Indian imports "abate the price of English Manufactures," he maintained that this abatement stimulated other segments of the economy. Laborers who bought Indian cottons would have more money available from their wages to spend on those items produced more efficiently by the English. Even if English laborers were thrown out of work, the greater competition for jobs would lower wages and push down the cost of other English products. Driving home his cost-advantage theory, Martyn stressed that any law which forced the English to consume only English goods forced them to pay more for their needs than was necessary. He likened this to denying the benefits of new inventions or the obvious savings from the division of labor or rejecting wheat sent as a gift from God.²⁰ Martyn explored the relation between earning and purchasing power with unprecedented analytical skill. Many of his observations had been anticipated by earlier commentators. For example, Dalby Thomas had extolled the labor-saving ingenuity which the desire to acquire called forth, and John Houghton had disputed Samuel Fortrey's strictures against French imports by pointing out that even foreign luxury items satisfied genuine consumer demands and made people work harder.²¹ These writers legitimized domestic competition because they perceived that England was not a giant workhouse but a giant market whose individual members had differing needs.

In focusing attention on these new market relationships, the pamphlets on Indian imports revealed those areas of conflict between manufacturers and merchants which the predominating concern with foreign trade had so long obscured. Driven no doubt by self-interest, the defenders of the East India Company put forth a justification for "a good buy" which amounted to a defense of domestic consumption. Here the issue became critical to the whole structure of ideas associated with the balance-of-trade theory, because the idea of the English economy as a collective undertaking was being challenged. This line of attack cut deeper than the superficial clash of interests. The actual social atomization which came with the seventeenth-century transition to a market economy had been ameliorated by an imaginative model of economic unity organized around national production and fortified by religion and patriotism. Psychological atomization could be forestalled as long as this image retained its credibility. When individuals began to think of their separate needs and demands as acceptable social considerations, the coherence of the earlier model would disappear. The benefits of the English consumers' having access to cheap East Indian imports depended upon the rejection of the view that society was an interlocking set of producers and distributors and the acceptance of the alternative view that the economy was

²⁰ [Henry Martyn], *Considerations on the East-India Trade* (London, 1701) in McCulloch, ed., *Early English Tracts on Commerce*, 606, 578-86.

²¹ [Thomas], *An Historical Account*, 361-62; [John Houghton], *England's Great Happiness* (London, 1677), 18-20.

an aggregation of self-interested individual producer-consumers. The boldest proponents of Indian imports perceived this difference and advanced a theory of economic growth based upon this perception.

When the maverick spirit of fashion revealed itself in the craze over painted calicoes the potential market power of previously unfelt wants came clearly into view. Here was a revolutionary force. Under the sway of new consuming tastes, people had spent more, and in spending more the elasticity of demand had become apparent. In this elasticity, the defenders of domestic spending discovered the propulsive power of envy, emulation, love of luxury, vanity, and vaulting ambition. On the other hand, as long as demand was viewed as more inelastic than elastic, the static conception of wealth held good. England then could only grow richer by selling a larger share of her surplus abroad, that is, by controlling a larger share of the international market. Once consumption was construed as a constructive activity, the connection could be made between progressive levels of spending or effective demand and a self-sustained momentum for economic growth. Writing in 1690, Nicholas Barbon bubbled over with the new possibilities: "The Wants of the Mind are infinite, Man naturally Aspires, and as his Mind is elevated, his Senses grow more refined, and more capable of Delight; his Desires are enlarged, and his Wants increase with his Wishes, which is for every thing that is rare, can gratifie his Senses, adorn his Body, and promote the Ease, Pleasure, and Pomp of Life."²² From Dudley North came a similar expression: "The main spur to Trade, or rather to Industry and Ingenuity, is the exorbitant Appetites of Men, which they will take pains to gratifie, and so be disposed to work, when nothing else will incline them to it; for did Men content themselves with bare Necessaries, we should have a poor World."²³

Less euphorically, Francis Gardner explained that while frugality was no doubt a commendable thing, "where People grow Rich, they will spend more largely, and it is better they should do so than to slacken their Industry and Diligence in Trade."²⁴ These sentiments even crept into the writings of conventional balance-of-trade writers such as John Cary, who affirmed that the growth of pride and luxury was the principal quickener of trade and extended his analysis down to "our poor in England" who can spend more on clothes and furnishings when they are paid more and hence increase the consumption of the very goods they manufacture.²⁵ An early convert to the power of consumption, John Houghton asserted that "Our High-Living is so far from Prejudicing the Nation that it enriches it." Describing the deadly

²² [Nicholas Barbon], *A Discourse of Trade* (London, 1690), 15.

²³ [Sir Dudley North], *Discourses upon Trade* (London, 1691), 14.

²⁴ [Francis Gardner], *Some Reflections on a Pamphlet* (London, 1696 [1697]), 24, as cited in Richard C. Wiles, "The Theory of Wages in Later English Mercantilism," *Economic History Review*, 2d ser., 21 (1968), 119. Usually identified simply as "Gardner," the author was probably Alderman Francis Gardner of Norwich, who was consulted by the Privy Council on the question of recoinage, according to J. Keith Horsefield, *British Monetary Experiments 1650-1710* (Cambridge, Mass., 1960), 52, and appears among those voting against the recoinage measure in [Thomas Wagstaffe], *An Account of the Proceedings in the House of Commons* (London, 1696), 13.

²⁵ Cary, *An Essay*, 143ff.

sins as economic virtues, Houghton cited pride, finery, vanity, shows, play, luxury, eating, and drinking high as causing "more Wealth to the Kingdom, than loss to private estates."²⁶ "Desire and want increase with riches," Barbon observed, "a Poor Man wants a Pound; a Rich Man an Hundred."²⁷

Not content merely to catalogue the psychological stimulants to demand, these writers drew attention to the specific economic function of each emotion. Foreign imports were justifiable because they dazzled people with their novelty and promoted industry by way of the acquisitive instinct. Analyzing the rationale for banning foreign imports, Barbon explained that it was based on the fallacious idea that if Englishmen could not buy foreign luxuries they would consume domestic goods. This was not true, he said, because it "is not Necessity that causeth the Consumption, Nature may be Satisfied with little; but it is the wants of the Mind, Fashion, and desire of Novelties, and Things scarce, that causeth Trade."²⁸

Dalby Thomas made the same point when he objected to those who wanted England to live on its own without imported luxuries. They were not the source of sin, he said, but "true spurs to virtue, valour, and the elevation of the mind, as well as the just rewards of industry."²⁹ Competition prompted men to invent things to reduce labor costs, Martyn asserted. "If my Neighbour by doing much with little labour, can sell cheap, I must contrive to sell as cheap as he."³⁰ North described envy as a goad to industry and ingenuity even among the lowest order. When the "meaner sort" see people who have become rich they "are spurr'd up to imitate their Industry." Even the man who goes bankrupt emulating his neighbor is a national benefactor, for the public gains from "the extraordinary Application he made, to support his Vanity." Fashion, Barbon said, promotes trade because it "occasions the Expençe of Cloaths, before the Old ones are worn out." Rejecting sumptuary laws, North commended consumption for its stimulus to trade. Nations never thrive more than when "Riches are tost from hand to hand."³¹

Behind these endorsements of early obsolescence and conspicuous consumption lay a new confidence in society's productive powers. Where Adam Smith would use the self-sustaining power of consumption without extolling it, these writers of the 1690s actually praised prodigality. A "Conspiracy of the Rich Men to be Covetous, and not spend, would be as dangerous to a Trading State, as a Forreign War," Barbon proclaimed.³² When John Pollexfen, an unreconstructed balance-of-trade thinker on the Board of Trade, used the old moralistic arguments against luxury consumption, Gardner replied that there was "no other use of Riches, but to purchase" what served "our Necessity and

²⁶ Houghton, *A Collection of Letters* (London, 1681), 60.

²⁷ Nicolas Barbon, *A Discourse Concerning Coining the New Money Lighter* (London, 1696), 3.

²⁸ [Barbon], *A Discourse of Trade*, 72-73.

²⁹ [Thomas], *An Historical Account*, 362.

³⁰ [Martyn], *Considerations on the East-India Trade*, 590.

³¹ [North], *Discourses upon Trade*, 15; [Barbon], *A Discourse of Trade*, 65; [North], *Discourses upon Trade*, 15. See also, *Englands Interest* (London, 1682), 6.

³² [Barbon], *A Discourse of Trade*, 63.

Delight."³³ The dour disapproval of self-indulgence was countered with the happy intimation of a new society of consumer-producers. "The more the merrier," Humphrey Mackworth proclaimed, "like Bees in a Hive, and better Cheer, too."³⁴ Moreover, the writers who promoted domestic consumption stressed the essential reciprocity of international trade. Rather precipitately labeling balance-of-trade notions as dead, the author of the Preface to North's *Discourses Upon Trade* announced that the whole world of trade was but as one nation, concluding from this that "the loss of a Trade with one Nation, is not that only, separately considered, but so much of the Trade of the World rescinded and lost, for all is combined together."³⁵ After asserting that either foreign or domestic consumption was good for the nation, Houghton explained in his *Letters* that import consumption enabled foreign countries to buy of England.³⁶ Henry Martyn's cost-advantage defense of the East India Company also emphasized the mutuality of international commerce.

Accompanying the pamphlet war over Indian imports was a debate over money which pointed up the inadequacy of the mercantilist definition of wealth. According to the balance-of-trade theory, gold and silver alone were wealth, and countries without mines could become wealthy only by a carefully managed foreign trade which brought in more specie than went out. This explanation of wealth undergirt the notion of the sterility of domestic trade and led to an evaluation of all economic activities in terms of their contribution to a net balance of payments. As early as 1650, William Potter had emphasized the commodity exchange that lay at the base of commercial transactions,³⁷ but the writers of the 1690s stressed the utility of money as a means to the goods men desired. Roger Coke put it succinctly: "The wealth of every Nation consists in Goods more than Money, so much therefore as any Nation abounds more in Goods than another, so much richer is that Nation than the other, for Money is of no other use, than as employed in Trade, and the defence of the Nation."³⁸ "To distinguish rightly in these points," Dalby Thomas explained, "we must consider money, as the least part of the wealth of any nation, and think of it only as a scale to weigh one thing against another."³⁹ Carrying the analysis further, Francis Gardner maintained that "some Goods are more acceptable in some Countries, at sometimes, than Money."⁴⁰

The repeated assertions that money was only a means for satisfying one's desire for goods was but a step away from the position that consumption was the logical end of production. As Henry Martyn put it: "The true and

³³ [Gardner], *Some Reflections on a Pamphlet*, 7.

³⁴ [Mackworth], *England's Glory*, 20-23.

³⁵ [North], *Discourses upon Trade*, viii. William Letwin, "The Authorship of Sir Dudley North's 'Discourses on Trade,'" *Economica*, 18 (1951), 35-45, suggests that Roger North wrote the preface to his brother's essay.

³⁶ John Houghton, *A Collection of Letters for the Improvement of Husbandry & Trade* (London, 1681), 52-53.

³⁷ William Potter, *The Key of Wealth* (London, 1650), 2.

³⁸ Coke, *A Detection*, 2:522.

³⁹ [Thomas], *An Historical Account*, 359.

⁴⁰ [Gardner], *Some Reflections on a Pamphlet*, 7.

principal Riches, whether of private Persons, or of whole Nations, are Meat, and Bread, and Cloaths, and Houses, the Conveniences as well as Neccessaries of Life . . . These for their own sakes, Money, because 'twill purchase these, are to be esteemed Riches; so that Bullion is only secondary and dependent, Cloaths and Manufactures are real and principal Riches.⁴¹

Speaking directly to the balance-of-trade maxim that commerce was only beneficial when more goods were exported than imported, Thomas Papillon maintained that this would only be true if gold and silver were the sole stock and riches of the kingdom: "Whereas in truth the Stock and Riches of the Kingdom, cannot properly be confined to Money, nor ought Gold and Silver to be excluded from being Merchandise, to be Traded with, as well as any other sort of Goods."⁴² John Houghton asserted that money in coin was "good for nothing, but potentially is good for everything."⁴³

During the debates which preceded the 1696 recoinage of England's clipped silver, attention passed to the question of whether gold and silver possessed an intrinsic and unique value which made bullion synonymous with wealth, or whether the use of money for exchange purposes made the extrinsic value derived from official minting the more important. In this clash of opinions, John Locke found himself ranged against North, Barbon, James Hodges, Henry Layton, Sir Richard Temple, and William Lowndes. Because Locke's recommendation for recoinage was based in part on the metalist view of wealth, his opponents dug away at this point. Money, Layton said, was as much a measure between items to be bartered and a commodity itself as the "natural, unalterable measure of Commodities," which Locke had made it.⁴⁴ Hodges said Locke's system of coin was built on the common error of considering the estimate of worth to be its intrinsic value rather than its usefulness.⁴⁵ Attacking Locke's belief that trade was valuable only as a means of attracting bullion, Barbon called the notion "altogether a mistake." Gold and silver were "but Commodities; and one sort of Commodity is as good as another, so it be of the same value."⁴⁶

By depriving gold and silver of their unique qualities, the balance-of-trade critics opened the way for appreciating the contribution of domestic trade to economic prosperity. Daniel Defoe developed this position most thoroughly in *Taxes No Charge*. Setting forth an elaborate plan for pumping money into the economy by taxing misers and pleasure spenders, Defoe argued that if the benefit of foreign trade is to bring in commodity for commodity then that can be done at home without exposing people to the hazards of the sea.⁴⁷ Lowndes, Layton, and an A. Vickaris similarly drew attention to the fact that

⁴¹ [Martyn], *Considerations on the East-India Trade*, 558.

⁴² Thomas Papillon, *The East-India-Trade a Most Profitable Trade to the Kingdom* (London, 1696), 4 (originally published anonymously in 1677).

⁴³ Following this logic, Houghton, *A Collection of Letters*, 24-25, recommended bringing in goods rather than money to balance accounts, since money is unable to satisfy any real human needs.

⁴⁴ [Henry Layton], *Observations Concerning Money and Coin* (London, 1697), 12.

⁴⁵ [James Hodges], *The Present State of England as to Coin and Publick Charges* (London, 1697), 135.

⁴⁶ Barbon, *A Discourse Concerning Coining*, 40.

⁴⁷ [Daniel Defoe], *Taxes No Charge* (London, 1690), 17.

money is only uniquely prized as a means of foreign exchange, and that within the internal market the intrinsic value of gold and silver is immaterial.⁴⁸ Carrying the argument still further, Dudley North said that it was absurd for people to say that money was short. If there were a demand for it, it would be manufactured like anything else, since there was free coinage and plenty of gold and silver around in plate.⁴⁹ With increasing sophistication, these writers assessed the economic role of money. In shifting attention away from money as a store of wealth, they moved closer to recognizing the dynamic elements in the economy. The debate over money—often with the same debaters—thus re-enforced the theoretical advances made in the India imports controversy. What was needed next was to examine how the latent consuming capacity of the public at large might become an engine for sustained growth.

The idea of man as a consuming animal with boundless appetites, capable of driving the economy to new levels of prosperity, arrived with the economic literature of the 1690s. By going behind the new tastes to explore the human motives regulating personal spending, some writers discovered both a human dynamic and a market mechanism which undermined the static, specie-oriented mercantilist view. Unlike the number of working days in a person's life, energy and ingenuity organized under the stimulus of desire appeared almost limitless. Since man could satisfy his new wants only by increasing his purchasing power, what desire ultimately produced was an incentive to be more competitive in the market. From such a spring economic activity could function without outside direction. Where earlier writers had recognized the impact of taste and delight upon the market price of items, they never saw the effect of these influences upon total demand. Nor did they move to an appreciation of the role played by domestic consumption in stimulating production and total national growth.⁵⁰ This required a new definition of wealth and a new model of economics as a self-sustaining complex of internal relationships in which foreign trade represented accessibility to desired goods rather than the only source of riches.

The material for building a new economic theory was presented in the 1690s, but these ideas were not worked out in the succeeding decades. Instead the old formulas of the balance-of-trade theory survived with undiminished strength well into the eighteenth century. Fragments from the controversies over Indian imports can be found in the polemics over the French treaty of 1713, in the voluminous economic writings of Daniel Defoe, and in a different vein in Bernard Mandeville's *Fable of the Bees*. Clearly, to conceive of economic growth in terms of goods and services annually produced, purchased, and consumed was difficult for Englishmen long accustomed to the

⁴⁸ [William Lowndes], *A Report Containing an Essay for the Amendment of the Silver Coins* (London, 1695), 81–82; [Layton], *Observations*, 12–14; [A. Vickaris], *An Essay for Regulating of the Coyne* (London, 1696), 22–23.

⁴⁹ [North], *Discourses upon Trade*, postscript. See also [Mackworth], *England's Glory*, 5–6; Sir Richard Temple, *Some Short Remarks upon Mr. Locke's Book* (London, 1696), 4–10; John Cary, *An Essay, on the Coyne and Credit of England* (Bristol, 1696), 5–12.

⁵⁰ See Marian Bowley, "Some Seventeenth Century Contributions to the Theory of Value," *Economica*, 30 (1963), 122–39.

pot-of-gold image of wealth. Even after these ideas had been published, there still remained obstacles—circumstantial as well as ideological—to the idea that society was an aggregation of self-interested individuals tied to one another by the tenuous bonds of envy, exploitation, and competition.

It lies in the nature of historical investigation that developments which could be expected to take place, but do not, are rarely given the same attention as actual events. Yet a break in a development offers important clues to the nature of social change. The lacuna which needs to be explained here is the failure of writers to build upon the insights and arguments of the seventeenth-century critics of the balance-of-trade theory. Between the 1620s when Mun, Malynes, and Misselden probed the mechanism of the exchange and the 1690s when the popularity of Indian cottons and the recoinage became issues, a host of journalists, reformers, merchants, bank promoters, royal officials, London developers, members of Parliament, mathematicians, improving landlords, clothiers, and lawyers published a steady stream of tracts and treatises. Their writings paralleled revolutionary changes in the English economy: the extension of the internal market, the development of the colonial trade, the spectacular growth of London, the striking increase in agricultural productivity, the founding of the Bank of England, and the redistribution of people and production centers. As could be expected from such a motley assortment of would-be experts, the quality of the description and analysis is uneven. There is, nonetheless, an increasing sophistication in the conceptualization of wealth, commerce, and money which challenged the central premises of the mercantilist theory. The failure of others to take up this fruitful line of reasoning is the historical fact which remains unexplored.

The usual treatment of this puzzle is to appeal to the slow spread of new ideas, to minimize the amount and thoroughness of the criticism, or to refer to residual Puritan scruples against spending. Of course new ideas often spread slowly, but the rate and unevenness in intellectual currents is what we would like to understand better. The same is true of the enduring strength of old ideas such as the Puritan legacy in Restoration England. There were many beliefs and values in the complex of Puritan thought. Why did some survive into the Restoration and others not? The waning and waxing of Puritan ideals require investigation. As for the more common scholarly tendency to see the mercantilist critics of the late seventeenth century as brilliant exceptions, the range and number of their publications belie this.

The answer, I believe, is that the values embedded in the alternate theories of economic growth were incompatible with the ideological imperatives of English society. Ideology, I define as a shared and coherent view of the world which provides solidarity by assigning and rationalizing the various roles people fill in society. The moral cohesion provided by common beliefs cannot be dispensed with, particularly in a society where custom has been largely replaced by conscious decisions. In a time of profound social change, such as England was experiencing, the shared explanations which an ideology offers become all the more important because they prepare people to assume new

functions. They also facilitate the integration of new ideas with old ones. The volitional nature of belief means, however, that new explanations must satisfy a range of needs before they will be willingly accepted. The uprooting of much of the English peasantry and the geographic redistribution of workers made social control a critical issue. Ideas which explained and justified that control to both the controllers and the controlled were essential if order was to be maintained through a time of change. The new economic ideas undermined the rationale for lower-class discipline and upper-class direction. The divisiveness of competition among groups within the economy threatened a political structure already strained by clashes with the crown. Mercantilism, unlike the more intellectually impressive ideas of its critics, created morally satisfying roles in the new market society. It prescribed a path of economic development more compatible with social stability. It also deflected awareness of the tensions between economic groups within England. From this perspective, the balance-of-trade theory became economic orthodoxy in the first half of the eighteenth century not because it explained the market to contemporaries—it had ceased to do this by 1680—nor for want of better explanations, but because it offered a rationale for coercing the poor, controlling the direction of growth, and subordinating the competition among groups to the goals of economic nationalism.

Capitalism in the first part of the seventeenth century had proved compatible with traditional social stratification. The lower order became the laboring poor, while merchants, clothiers, bankers, shippers, and processors acquired the gentility formerly reserved for the landed class.⁵¹ Subtle shifts in values had occurred, but the two-tiered world of the propertied and the propertyless had not been undermined. A consumption-oriented model of economic growth, on the other hand, threatened major interests of the ruling class that had coalesced in Restoration England. Dangerous leveling tendencies lurked behind the idea of personal improvement through imitative buying. The notion that the wealth of nations began with stimulating wants rather than organizing production robbed intrusive social legislation of a supporting rationale. Once it was suggested that spending in the home market was more beneficial to the economy than domestic parsimony the social benefits of statutory wage levels could be questioned. If English consumers had a right to a good buy in Indian cottons, as Martyn suggested, why could they not demand commercial policies which would protect them?

It is no accident that the men who advanced these novel opinions about domestic consumption were merchants—either outspoken defenders of the East India Company or like Dudley North, an experienced Turkey merchant, associated with foreign trades which grew with the spread of new tastes. They were not involved, as were manufacturers, in the mobilization of labor. The important variables of their commercial world were markets, prices, shipping costs, and interest rates. The manufacturers whose numbers grew with the

⁵¹ Lawrence Stone, "Social Mobility in England, 1500-1700," *Past and Present*, no. 33 (1966), 52-55.

increase in industrial processing shared the clothiers' concern with employment problems. The lower class loomed larger to them as potential workers than as likely customers. In this regard, the industrial capitalists shared the interest of farmers and landed gentry, who relied upon the availability of day laborers at critical seasons of the year. Unlike the foreign merchants, these members of the ruling class had powerful reasons to maintain the credibility of the balance-of-trade explanation of national wealth. Before patriotism was the last refuge of the scoundrel, it was the valued ally of English manufacturers and landlords, creating a free labor force from the copyholders, tenants, mechanics, and craftsmen of an old order. Moreover, conventional mercantilist formulas supplied a rationale firmly based on national security for the protectionist legislation which came into full force after 1713.⁵² To entertain the idea that "the whole World as to Trade, is but as one Nation or People" was to bring into question not only the entire Navigation System but also the wisdom of England's calculatedly aggressive national posture.

This new challenge to mercantilist thought also bore upon the more subtle problem of social control in a liberal society. Through the course of the seventeenth century, upper-class Englishmen had disentangled themselves from the constraining ties of a corporate society and embraced instead the ethos of liberalism. Slowly the individual's right to be free of inherited social obligations had gained precedence over the older notion of society's primary claim upon its members. While liberal ethics freed property and property owners from traditional social restraints, it also undermined the justification for some people's being invested with permanent authority over others. Instead, all in the society were conceived to be free and individually responsible. In arguing for this personal liberty, however, upper-class liberals had delivered most of the propertyless into the hands of a new master—the market through which they sold their labor and bought their bread. At the same time that the spirit of "possessive individualism"—to use C. B. MacPherson's insightful phrase—shattered institutional responsibility for social survival, developments in the economy separated most workers from their tools or their access to land. Without these, they were forced to sell their labor. Since only through these transactions could people feed themselves, the unseen market replaced the visible and personal authority of the previous era.

Contemporaries recognized this coercive power of the market. In 1641, Henry Robinson had defended higher food prices because the "Husbandman would hereby be brought to a frugall dyet or stirrd up to become more industrious." Likewise, farmers could "discharge a rackrent by multiplying the fruits thereof through industry."⁵³ Evaluating the effects of a poll tax, Petty adduced an added benefit from the fact that it encouraged men with many children to set them "to some profitable employment upon their very first capacity, out of the proceed whereof, to pay each childe his own Poll-

⁵² For an interesting discussion of the repatterning of English trade under the protectionist impulse see Ralph Davis, "English Foreign Trade, 1700-1774," *Economic History Review*, 2d ser., 15 (1962), 294-95.

⁵³ Henry Robinson, *Englands Safety*, in *Trades Encrease* (London, 1641), 7-8.

money.”⁵⁴ Francis Gardner put the low wage argument in a nut shell: “The Poor, if Two Dayes work will maintain them, will not work three: and our Manufactures are never so well wrought as, in a time of dull Trade, when we pay less for Workmanship, and yet the Poor live as well then as in time of greatest Plenty, if they have but a full stroke of Work.”⁵⁵

The efficacy of the market as an implement of control in a technically free economy depended, however, upon whether it was a buyer's or a seller's market. As John Locke commented, mechanics and apprentices lived such a “hand-to-mouth” existence that they were forced to accept food for wages rather than starve.⁵⁶ Another writer feared their excessive freedom and described the poor within a fifty-mile radius of London as idle and surly and willing to work only “if two days pay will keep them a week.” Others suggested an excise tax on food and drink to make the poor work a full week.⁵⁷ Against this background the danger of a consumption formula for economic growth can be assessed better: it threatened the most effective form of class discipline in a liberal society. The capitalistic organization of the economy could force men and women on to the labor market since that was the only place where they could earn the means of subsistence, but the amount of labor they were forced to sell depended upon the wage rate.

The employers' need to control the labor force reveals the incompatibilities between liberalism and capitalism. Liberalism asserted the right of each person to the enjoyment of himself and the fruits of his labor. It did not matter whether laborers were predisposed toward leisure or consuming. But the economic growth which the capitalistic system promoted, required a disciplined and expandable work force. Leisure preferences could be as inhibiting to growth as archaic Poor Laws. Employers did not seek conditions of political and moral freedom for the working class. In fact, as they well perceived, transporting workers to centers of employment was much more efficient than relying upon necessitous, but free, workers to go where the jobs were.⁵⁸ While much has been made of the congruence between freedom and capitalism, it was the freedom of property owners from social obligations which was critical to capitalistic growth in the seventeenth century. Ideas which promoted free choice among the poor were inherently dangerous to the entrepreneurs. The writers who extolled the advantages of domestic spending in the fight over Indian imports implied that human behavior could be shaped by the impulse to consume, but such optimistic reliance upon envy and emulation must have appeared inadequate to the task of controlling laborers described as idle and surly and willing to work only if two days pay would keep them a week. Since

⁵⁴ [Petty], *A Treatise of Taxes*, 43. See also Thomas Manley, *Usury at Six Per Cent. Examined, and Found Unjustly Charged* (London, 1669), 24–25.

⁵⁵ [Gardner], *Some Reflections on a Pamphlet*, 16.

⁵⁶ [John Locke], *Some Considerations of the Consequences of the Lowering of Interest* (London, 1692), 34.

⁵⁷ *The Trade of England Revived; and the Abuses Thereof Rectified* (n.p., 1681), 8. See also E. P. Thompson, “Time, Work-Discipline, and Industrial Capitalism,” *Past and Present*, no. 38 (1967), 56–97; Keith Thomas, “Work and Leisure in Pre-Industrial Society,” *ibid.*, no. 24 (1964), 61–62.

⁵⁸ Sir Josiah Child, *A New Discourse of Trade* (London, 1693), 67; [Petty], *A Treatise of Taxes*, 48–49; Robinson, *Englands Safety*, 44–45.

the old social justification for ordering all economic relations had been destroyed in the fight to make property private and property-owners free, economic theory had to supply the reasons for statutory regulations.⁵⁹ This was the role which the balance-of-trade dogmas played in the eighteenth century.

The critical difference between Adam Smith's theory of economic growth, which was anticipated in the closing years of the seventeenth century, and that of the mercantilists was the constructive value given to consumption. This constructive value in turn rested upon the assumption that all men wished to maximize their market power because that and that alone offered an avenue of self-improvement. Although Smith's sympathies lay strongly with the savers and investors, the propensity to consume provides the linchpin for his whole system.⁶⁰ Externally, it creates the effective demand which calls forth production systems large enough for specialization and division of labor. Internally, the drive to truck and barter—to buy and sell—directs individual energies toward the market and away from other human satisfactions. So axiomatic is it to Smith that self-improvement will be fulfilled through economic activity that he does not even entertain ideas about alternate means to improve oneself, much less alternate goals. One improves through market power, but market power rests upon producing power. The more one produces, the more one can satisfy wants. Since self-improvement, as Smith defined it, has no natural limit, there is no limit to man's endeavors. In Smith's model, people as producers and people as consumers act like an alternating electrical current, throwing a steady flow of impulses into the economy. The flow can be taken for granted because it came from human qualities assumed to be universal.

The acceptance of the idea of universal economic rationality was the key step in the triumph of modern liberalism, because the natural economic laws depended upon natural modes of behavior. Before the laws could be accepted, the description of human nature supporting them had to be credible. Historically, however, before economic rationality became a learned pattern of response, explicit control was necessary to secure working-class discipline. The laboring poor had to be managed. The spendthrift with a feast or famine mentality had to be transformed into the shrewd saver, and the saver had to become an orderly, but compulsive, investor and consumer. Market thinking could be relied upon only after the variety of forces influencing personal preferences in the use of time and wealth had been ruthlessly narrowed to one—the likelihood of gain. This radical reductionism was the essence of

⁵⁹ For Edward Coke's role in endowing economic freedom with a constitutional sanction see David Little, *Religion, Order, and Law* (New York, 1969), 203–17, 243–46; Barbara Malament, "The 'Economic Liberalism' of Sir Edward Coke," *Yale Law Review*, 76 (1967), 1321–58; Donald O. Wagner, "Coke and the Rise of Economic Liberalism," *Economic History Review*, 6 (1935), 30–44.

⁶⁰ "Consumption is the sole end and purpose of all production; and the interest of the producer ought to be attended to, only so far as it may be necessary for promoting that of the consumer. The maxim is so perfectly self-evident, that it would be absurd to attempt to prove it." Adam Smith, *An Inquiry into the Nature and Causes of the Wealth of Nations* (New York, 1937) [originally published in 1776], 625. For Smith's ambivalence on the subject of consumption, compare this quotation with those on *ibid.*, 321–25.

Smith's economic rationality. The economically rational person was the one who subverted all other drives to the economic one of gaining more power in the market.

During the first part of the seventeenth century when invested capital moved England toward a market economy, only the vanguard of entrepreneurs operated as economic rationalists. To them, the rest of the society represented a problem demanding external control and direction. To meet this challenge, moreover, they relied upon a model of economic growth which obscured the possibility that the extension of economic rationalism to the working class was either desirable or possible. Rather the economic irrationality of the poor was assumed; the solution was to train them up to habits of work. When the writers in the closing decades of the seventeenth century proposed unleashing the acquisitive instincts of all classes, they were proposing a route to economic growth fraught with perils. The idea of self-improvement through spending implied genuine social mobility. The assertion that "the meaner sort" could and should emulate their betters suggested that class distinctions were based on little more than purchasing power. The moral implications of growth through popular spending were even more suspect. Unlike the work ethic which called upon powerful longings for self-discipline and purposeful activity, the ethic of consumption rested upon a moral base so shallow as to threaten the whole complex of conventional religious precepts. Calvinism had joined an ancient Christian ascetic impulse to a modern reorganization of work; the psychology of consumption offered nothing more than a calculating hedonism.

The moral anemia of appeals to consume was inextricably tied up with questions of control. Liberalism had posited man's freedom and responsibility. Capitalism required unrelenting personal effort in the market place. The two could meet only if the poor, like the rich, were converted to possessive individualism and economic rationality. Until this transition had been made, class discipline needed the support of economic theories bolstered by religion and patriotism. When capitalism and free choice were joined by Adam Smith, they were compatible because Smith could theorize from a human model in which the drive for economic self-improvement predominated. This conception of man was the antithesis of freedom, for it presumed a compulsive market response. In recommending the democratization of consumption in the 1690s, the proponents of a spending model of economic growth were revealing for the first time the tensions that lay beneath the values and sensibilities associated with the producing and consuming sides of capitalism. Only when economic rationalism had become internalized by the working, as well as the investing, class could liberal economics support the onus of its amorality. The ideology of mercantilism in the meantime blunted the force of new ideas.

The Classical Theory of Deference

J. G. A. Pocock

A DEFERENTIAL SOCIETY IN THE CLASSICAL—that is, eighteenth-century English and American—sense is usually conceived of as consisting of an elite and a nonelite, in which the nonelite regard the elite, without too much resentment, as being of a superior status and culture to their own, and consider elite leadership in political matters to be something normal and natural. Whether elite leadership means simply that leaders must come from the elite, or, in addition, that the leadership given on specific issues by members of the elite is normally to be followed—by no means the same thing—is not perfectly clear. But this summary of deference as an ideal type is intended to suggest that it contains another ambiguity which renders it both convenient and problematic as a tool for the historian. Deference is expected to be spontaneously exhibited rather than enforced. A slave or a serf is flogged into obedience, not deference, and the deferential man is frequently depicted as displaying deference as part of his otherwise free political behavior. He defers to his superiors because he takes their superiority for granted, as part of the order of things. It is often suggested that what makes him do so is the conditioning effect of tradition and that the deferential society is closely akin to another favorite conceptual tool of historical sociologists—the traditional society.

Deference is the product of a conditioned freedom, and those who display it freely accept an inferior, nonelite, or follower role in a society hierarchically structured. Scholars who employ the concept of a deferential society, on either side of the Atlantic and of the year 1800, however, also pay considerable attention to something less subjective, which Castlereagh characteristically termed “persuasion in a tangible shape”: that is, to the notion of an influence, reaching from pure deference toward inducement and even coercion, and including such means of social control as agrarian tenancy and political patronage, which the elite in the deferential society exercised over their inferiors. And it is supposed that finally there occurred—in America during the 1780s or 1820s, in Britain during the 1830s or 1860s—a democratic rebellion

This paper, which serves as an introduction to the two following articles, was part of a symposium on deference presented at the 1974 meetings of the American Historical Association in Chicago. Richard W. Davis' paper, now revised for publication, was also read at that session. David Spring, who was not part of the Chicago program, wrote his paper subsequently to round out this published symposium.

against deference and influence alike, a movement toward a world in which there was no god but equality and Tocqueville was his prophet.

This straw man, if such it be, is woven of a thick and complicated texture, permitting some ambiguities and confusions. It is perhaps time to look at a few ways in which the notion of deference has been and may be applied, not with any specially destructive intentions toward its use as an analytical tool but in the hope of clearing up some of the misunderstandings that may have attended its use. Clearly, the understandings of British Whigs and American Federalists need not be the same as those of their twentieth-century interpreters, but the two views are likely to illuminate one another. In seeking to understand the so-called deferential society, we should start by inquiring what that society thought deference was.

Deference is an oldish word, whereas the term "deferential society" is a twentieth-century neologism. And though political speakers and writers of the eighteenth century knew the word and used it on occasion, they did not employ it as a key concept or as a means of denoting an essential attribute of political society. They were acquainted, however, with what may be considered the essential meaning of the term: the voluntary acceptance of a leadership elite by persons not belonging to that elite, but sufficiently free as political actors to render deference not only a voluntary but also a political act. As clear and simple a description of this effect as any appears in James Harrington's *Oceana*.¹ We are invited to suppose that out of any twenty men engaged in political decision, six will be of superior capacity to the others. Harrington's emphasis falls, however, less on the superior capacity of the six than on the recognition of this capacity by the fourteen. He emphasizes repeatedly that this will be instant, unforced, and infallible. One would say that he presents it as rational but for the difficulty of applying that term to the recognition of a higher rationality by a lower. At all events, there is no need to restrain or compel the fourteen in their identification, recognition, and choice of the six. The six will be there, and the fourteen will find them. The fourteen will acknowledge the superior capacity of the few and accord them the authority of fathers, less in the sense in which Filmer's patriarchs enjoy a God-given *patria potestas* than in that in which Roman senators were called *patres conscripti*. But the fathers owe their authority less to their own superiority than to the acknowledgement—it would be proper to call it election—of their inferiors. Here, surely, is what is meant by deference.

Harrington's fourteen are, no less than the six, active citizens charged with performing political functions. The first and most important of these is the finding and recognition of the six. But since Harrington was a republican rather than a parliamentarian, he did not incur the wrath of Rousseau by implying that the fourteen would then go home and leave the six in plenipotentiary enjoyment of their mandate. A complex distribution of functions ensued between the six and the fourteen, which to Harrington—indiffer-

¹ John Toland, ed., *The Oceana and Other Works of James Harrington* (London, 1771), 44, 236–38.

ent as he was to separation of powers in the eighteenth-century sense—reduced itself to the distinction between “debate” and “result.” It was for the six to conceive and initiate policies, to articulate the differences between them, to argue the cases for and against each one. When the debate of the aristocracy was at an end, however, it was for the democracy—without speech or argument, perhaps by the silent routines of Venetian balloting—to determine which of the policies or courses proposed should actually be adopted. Harrington stresses not only that liberty is at an end when debate and result are lodged in the same hands, but actually that the nonelite fourteen are better fitted to exercise the final determination than the elite six.² The superior capacities which distinguish the six fathers turn out to be capacities for invention, articulation, foresight, and analysis—the virtues of *theoria*. When it comes to actual decision, something else counts. Perhaps it is experience, and the fourteen have more of it because there are more of them. Alternatively, it is honesty, and the fourteen-six relation insures it in both parties. At all events, though the many acknowledge the few to be superior in their capacities, the relation between debate and result is one of equality. Deference, then, is perfectly compatible with equality, so long as the latter is proportionate equality in the Aristotelian sense. Indeed, this sort of equality cannot exist unless qualitative distinctions and inequalities among men are recognized. And if Harrington’s “debate” and “result” be equated with the “speaking aristocracy” and “silent democracy” of which we hear in seventeenth-century New England,³ it becomes apparent that the “silent democracy” need no more lack political will and power than need the “silent majority.”

Harrington was writing in this way because he felt the need to rehabilitate aristocracy in the wake of what he saw as the collapse of feudal oligarchy. When a few owned the land and the many were their tenants, the latter were subject to the power of the former and lacked political capacity. But when this state of affairs collapsed, and the many acquired both propertied independence and political capacity, there necessarily appeared a republic in which the democratic component was of vast importance. It was self-evident, however, to any republican theorist that the people in a commonwealth must be differentiated into an aristocratic and a democratic component; but according to Harrington’s theory of deference, they divide naturally, voluntarily, and spontaneously. There is little need to legislate the special qualifications defining an aristocracy. In the six of superior and recognized capability, we detect the “natural aristocracy” (of so much concern to John Adams), whose progressive collapse from the 1780s to the 1820s provided American political culture with its first prolonged internal crisis. Yet if a theory of deference was coterminous with a theory of natural aristocracy, it was a way of arguing that

² *Ibid.* 44, 236–38, 487.

³ The phrase is Samuel Stone’s. See also Perry Miller, *The New England Mind* (Boston, 1961), 452. James Harrington would have said that if Stone allowed the congregation no “result,” he had no business using the word “democracy.”

no other aristocracy was possible or necessary; and it was a way of welcoming, legitimating, and liberating the newly acquired political capacities of the many. It furnished the grounds, we may suspect, on which Senator Barry Goldwater, centuries later, has felt able to welcome the extension of the franchise to eighteen-year-olds.

The superior capacities of the six will not consist of attributes of personality alone. These talents will also be recognized through outward economic and cultural signs—wealth and birth, leisure and property, liberality and education. Harrington was a gentleman, who thought politics had something in it peculiarly suited to the genius of a gentleman,⁴ and he sought to reassure his class that they need not fear for their leadership status in a yeoman commonwealth. But though he was assuring other gentlemen that the material foundations of their elite status would remain intact, his point was that their property and culture would be recognized by the *demos* as part of the superior natural capacities which the *demos* also recognized. The link between personal capacity and material circumstance—in Harrington's language, between the goods of the mind and the goods of fortune⁵—was provided by the Aristotelian theory of leisure. Property brings leisure, the opportunity to turn the mind away from property and toward the common good of which it is part. Harrington's six will fairly certainly have more property than the fourteen—more leisure and opportunity to develop superior capacity—and the capacity for the limited sort of leadership which is expressed in debate as compared with result. But what keeps it a limited leadership is the fact that the fourteen too possess property, and therefore leisure and the ability to know something about the common good. Their political capacity is not confined to recognizing and choosing the six; retaining the power of result, they retain the capacity of evaluating the policies which the six have the capacity to propose.

We are now in the world of Charles Sydnor's gentleman freeholders, the idealized Virginia of *The Candidates*.⁶ There the yeomen are certainly deferential in the sense that they accept leadership by a natural aristocracy. They recognize extrapersonal characteristics such as birth, wealth, and culture—the members of the gentry seem obliged to emphasize their education and their libraries—as outward and visible signs of the superior personal capacities they are looking for. But the yeomen must first find and evaluate their superiors. They are presumed capable not only of knowing a fake natural aristocrat when they see one, but also of asking sensible and pertinent questions of the genuine article. Deference precludes them from the capacity for leadership, but not from an intelligently critical attitude toward those who possess that capacity. It is wholly compatible with proportionate equality and public virtue. Indeed, if these things presuppose political relations between individuals of diversified capacity, they depend upon deference, and deference

⁴ Toland, *Oceana*, 53.

⁵ *Ibid.*, 36, 41–2.

⁶ Charles S. Sydnor, *Gentlemen Freeholders: Political Practices in Washington's Virginia* (Chapel Hill, 1952).

is no more than the recognition of one capacity by another. It might even be shown by the few toward the many.

Moderns are disposed to think, however, that if one man has more money than another he has power over him, or at any rate power that he does not share with him; and they feel that Harrington's equation of power with property ought to have included this perception. Harrington, however, did not think in this way; he was too exclusively concerned with feudal tenure. He thought the House of Lords had existed because the nobility had once been a feudal baronage. Since that no longer existed, there could no longer be a House of Lords. The political relations between Englishmen could only be those obtaining between classically equal citizens, of which the deference that institutionalized a natural aristocracy was an important part. But several things happened in 1660 that he had failed to foresee. While the House of Lords was restored, feudal authority was not. Yet the House of Lords flourished for a long time thereafter. In considering the obvious social power of the peerage, historians may conclude that the lords had ways of maintaining social power, and of retaining men in dependence, which Harrington had failed to consider. But was that necessarily how the matter appeared to contemporaries?

There was widespread acceptance of that part of the Harringtonian interpretation which stressed that since the landholding classes were no longer divided into barons and their dependent vassals or retainers, England must now be governed by a scheme of civic relations obtaining among its independent proprietors. The latter, according to classical and constitutional theory, must be divided into aristocratic and democratic components, and there now existed a hereditary but no longer feudal upper house to play the aristocratic role. Harrington and other radicals of the Interregnum thought it important to insure that the feudal nobility was not replaced by any other kind of "standing aristocracy,"⁷ whether of hereditary officeholders or of elect saints. The Restoration House of Lords, however, claimed no hereditary monopoly of any significant political function; the lords had instead hereditary titles, hereditary rights of summons to parliament, and lands inherited indeed, but inherited in much the same way as by any other gentleman. Edmund Burke, like most thinkers of the eighteenth century, argued that in a polity of independent proprietors, the lords' hereditary dignity and hereditary role in parliament rendered them independent in a very special degree. This is the point of his famous and not really sycophantic letter to Richmond, in which he says that the hereditary peers are like great trees and the new men—like Burke himself—the none too hardy annuals that bloom in their shelter.⁸ All men are independent, Burke is saying, and the hereditarily independent are not different from the rest of us. They are those animals who are more equal than others.

⁷ For example, see Marchmont Nedham, *The Excellencie of a Free-State* (London, 1656).

⁸ Lucy S. Sutherland, ed., *The Correspondence of Edmund Burke* (Cambridge and Chicago, 1960), II, 377.

The theory of aristocracy in eighteenth-century England rested upon the assertion that a hereditary aristocracy, so long as it did not reduce all others to dependence, was perfectly capable of acting as a natural aristocracy. The House of Lords could be praised for possessing the virtues of such an aristocracy; we read this as late as 1867, in Coventry Patmore's informative if nauseating poem about "the year of the great crime/When the false English nobles and their Jew,/By God demented, slew/The Trust they stood twice pledged to keep from wrong."⁹ Patmore ascribed to the nobility before their apostasy such things as dignity, leisure, *sprezzatura*, the unbought grace of life, and the Aristotelian virtues generally, and indicated that a class endowed with these virtues had incomprehensibly renounced a political leadership which might still have been rewarded with deference had they continued to affirm it. But while Harrington's natural aristocracy received deference within a political process, in Hanoverian England deference operated as often as not outside electoral procedures and even in such a way as to render them unnecessary. If deference was, then, a concept so highly civic as to be quasi-republican, what was its theoretical role in an increasingly oligarchical society, where the political activity of the lesser proprietors tended for at least two generations to be progressively reduced?

There was always an alternative, in both theory and practice, to deference as the voluntary respect which one kind of political capacity paid to another. This alternative was the influence or patronage—possible only in a world of office—which government might exercise over society, or patrons over clients. Most historians would agree that this had a great deal to do with the survival of the peerage in a shape which Harrington had failed to predict, and those who wish to reconcile the "crisis of the aristocracy," culminating in 1640, with its spectacular revival in the century beginning about 1660, seem disposed to stress that members of the nobility growing up under the restored Stuarts were involved in government to the point where they might well have become an official aristocracy rather than a class of parliamentary magnates.¹⁰ But the restoration of the House of Lords was part of the restoration of the parliamentary constitution, and the rhetoric of that restoration stressed the lords' role as a *pouvoir intermédiaire*, a "screen or bank," as the phrase ran in English, pointing toward their role as the hereditary natural aristocracy of the land-owning and propertied nation. After 1688 and again after 1714, the peerage did develop into a class of parliamentary magnates—or a dominant component of that class—but at the same time it becomes less and less necessary to distinguish between the influence which peers exercised by reason of their territorial holdings, the influence which they exerted over parliamentary elections and electorates, and the influence which they possessed because of their activity in government, office, and departments of state. If from one point of view England seemed a vast Country, composed of independent

⁹ Coventry Patmore, *Poems*: vol IV: *The Unknown Eros* (London, 1879), 56.

¹⁰ John R. Western, *Monarchy and Revolution: The English State in the 1680s* (London, 1972).

proprieters of various sorts of freehold, there was another from which it appeared a vast and dispersed Court, in which everyone constantly sought patronage from those in positions to give it.

Here is one of the fundamental ambiguities of our subject, that between "deference" and "influence." No doubt there was a deference proper to the relations between client and patron: the sort of deference which Burke displayed toward Richmond, or Hutchinson toward Hillsborough,¹¹ or Chatham—for that matter—toward George III. But in these three cases (and many others), the client was deeply concerned to maintain his independence. Deference was not merely his means of inducing the patron to grant what he had to offer, but also the means of reminding the patron that it was not his business to reduce the client to dependence in the sense of servility, but to treat him in a way which acknowledged the independence and self-respect of both parties. The technical term for this was "affability." Only a great man could have affable manners. It was a failure of *ton* to ascribe them to one not of superior station, but a patron who failed in affability toward Jonathan Swift or Samuel Johnson was likely to carry scar tissue to remind him that his station had its duties.

There were those who realized that the patron-client relationship, in the form it took when the latter was a man of honor and independence, had much in common with the relationship between lord and vassal, and this doubtless contributed to the idealization of feudal society observable toward the end of the eighteenth century. But once it was admitted—it was sometimes denied—that the lord had been able to oblige and compel the vassal to follow him, then it was no less certain to the Whig mind that eighteenth-century Britain was a postfeudal society, and that deference and influence were equally techniques of social control necessary because society was in such a state. Neither could operate, or would be necessary, except among independent men. But it seemed abundantly clear to theorists that deference could not corrupt men or reduce them to servility, because in the last analysis it was concerned with what Harrington had called "the goods of the mind." Conversely, influence in the sense of patronage could corrupt, because it was concerned with persuasion in a tangible shape, with the material and social rewards which Harrington had ranked among "the goods of fortune." There were thus moral tensions between deference and influence, which it was the business of social morality, in the age of great expectations, to try to overcome; but to go any further in this direction would be to enter that fascinating and difficult territory where the two conceptions interpenetrate.

Given that in the half-century or longer beginning about 1780 there occurred in Britain and America extensive reorganization of both electoral institutions and electoral behavior, which can be illuminated by applying the concept of deference to them, the model constructed here, by extrapolation from Whig political perceptions, suggests that in Britain the continued and

¹¹ Bernard Bailyn, *The Ordeal of Thomas Hutchinson* (Cambridge, Mass., 1974).

massive importance of the peerage was explained and legitimized by the contention that a natural aristocracy, expecting deference, and a hereditary aristocracy, exerting influence, were not mutually exclusive but could, within limits, be identical. In America, however, the relative unimportance of hereditary station and political patronage meant that political elites, where they existed, were, above all in the postindependence era, cast more exclusively in the role of natural aristocracy and compelled to rely upon the expectation of deference to the exclusion of any other means of maintaining their status. Both societies were postfeudal, but one had been directly modified by Whig parliamentarism in ways that the other had not. This explains why American political thought soon became and long remained preoccupied with acclaiming or deploring the failure of natural aristocracy, and the Federalists with the role of aristocrats struggling to understand the failure of the deference due them. Likewise, it dramatizes, if it does not fully explain, the fact that America preceded Britain in experiencing the rise of equality in a revolutionary, Tocquevillian sense in which established elite characteristics were considered irrelevant to claiming political leadership.

Richard W. Davis examines how far Whig parliamentary reform can be explained as an attempt to enlarge deference at the expense of influence, by increasing the independent at the expense of the dependent electorates, and in what sense, if any, the term deference itself is useful as a description of the ways in which independent electorates actually behaved. It would be an ironical conclusion if we found that democratization in Britain was, even initially, a successful experiment in the perpetuation of deference, since it was in America that the rise of democratic machine-politics led to the assumption by influence and patronage of an importance they had never had before and cannot be said to have lost since. In this respect, new democrat was but old Whig writ large, and it would be strange to find that our two societies had exchanged the roles assigned them by the eighteenth-century antithesis. Recent work by several historians of America has drawn attention to the Federalist perception of a democratic assault upon classical education as part of the overthrow of deference and virtue.¹² In Britain, classical education was brilliantly revived by radical Tories like the Arnold family as part of a not unsuccessful attempt to bring a Coleridgean meritocracy into being to redress the balance upset by the decay of the propertied aristocracy. There were utopian dimensions to this experiment, as the younger Arnolds discovered—Matthew by writing about barbarians, philistines, and populace, Thomas junior by emigrating to seek deference as a clerical leader in such unpromising places as New Zealand and Tasmania. But in Britain the creation of a mandarinat proceeded, in however un-utopian a form. Perhaps this inquiry into deference should conclude by asking at what points an advanced education became a positive disadvantage in the pursuit of elective office. And perhaps part of the answer would be that in Britain it has not altogether happened, even yet.

¹² For example, see Linda K. Kerber, *Federalists in Dissent* (Ithaca, 1970).

Walter Bagehot and Deference

DAVID SPRING

UNTIL RECENTLY THE REPUTATION of Walter Bagehot has stood high with students of nineteenth-century England. Because he was a man of many parts, not only historians, but also students of literature, politics, and economics have admired him. Many of them, if asked, would probably have agreed with that master of Victorian studies, G. M. Young, that Bagehot was their favorite Victorian. Bagehot wrote well. He was a maker of memorable phrases, a man also of sympathy, taste, learning, and wit. If, as Young has said, he was not "the greatest man alive and working between 1837 and 1901," so much the better. Bagehot was "a man not too illustrious or too consummate to be companionable, but one, nevertheless, whose ideas took root and are still bearing."¹ In short, Bagehot was easy to like, and to such a man posterity tends to be kind, restraining the critical faculty it exercises so readily upon less likeable and more eminent men.²

Not that critical voices have never been heard. Maynard Keynes early in this century noted that Bagehot did not always reach the level of his reputation. His economics was better psychology than it was economics. He could theorize to a limited degree in familiar fields of observation. But he was sometimes tempted beyond his powers. He could become entangled in "a train of inference," as Keynes noted, that was "too long for him or for which, not truly caring," he had no "steady patience." Similarly, even his gift for psychological insight was limited. It was good when exercised upon "those of a middle position," primarily "business men, financiers, and politicians." It was less good when exercised upon either the high or the low. "Genius and violent feeling on the one hand," wrote Keynes, "and servitude, confinement, and distress upon the other" were beyond him.³

During the last decade, however, voices critical of Bagehot have grown more numerous. For the most part the critics have been political scientists.⁴ But

¹ George M. Young, *Today and Yesterday* (London, 1948), 240.

² See Alan Ryan, "Bagehot's Britain," *Times Literary Supplement*, Dec. 27, 1974, 1453-54.

³ John Maynard Keynes, "The Works of Bagehot," *Economic Journal*, 25 (Sept. 1915), 369-70, 372.

⁴ The criticism by political scientists probably began with Maurice J. C. Vile, *Constitutionalism and the Separation of Powers* (Oxford, 1967). Vile was primarily interested in Bagehot's constitutional theory, which he was the first to attack: "Not only was his claim to originality [on the cabinet] false, but his treatment of the central principles of the Constitution reveals a distorted and unhistorical approach to the subject he

even historians—who as a rule are more cautious, slower to leap on or off bandwagons—have begun to join in the criticism.⁵ “It has long been clear,” observed Olive Anderson, “that we need both a serious historical appraisal of Bagehot’s ideas and a serious attempt to explain Bagehot’s sustained posthumous renown.”⁶

Of course, I intend nothing so ambitious in this short essay. Rather, I intend to analyze his social and political thought to the degree necessary to illuminate his concept of deference. To be sure, deference was a key concept for Bagehot. More than anyone else Bagehot brought this idea into current intellectual vogue. Geoffrey Best recently called Bagehot the Darwin of deference.⁷ And more than anything else in his writing, deference has caught the interest of historians. I suspect, however, that they have not read him as carefully as they might have. It may clear the air therefore to re-examine Bagehot’s social and political thought to see what he meant by deference and to see if his ideas of deference are trustworthy guides to nineteenth-century English political life.

Bagehot dealt with deference most explicitly in *The English Constitution*, and historians have usually derived their ideas of it from this book. A better place to begin, however, is *Physics and Politics*, a much less glittering and quotable book, but one containing the underlying theory on which *The English Constitution* stands. In *Physics and Politics* Bagehot, the Victorian social evolutionist, described the long development of society and politics as did Maine and Spencer in terms of polar opposites. This is a characteristic strategy of Bagehot in all his writings—to deal with reality as a dualism. This strategy partly accounts for the seductive quality of his work. “Bagehot’s attraction,” V. S. Pritchett wrote, “is that he speaks in our accent; he has our drastic habit of mind, our analytical approach.” Nothing is more drastic than Bagehot’s fondness, as Pritchett put it, “for clearing the ground by cutting it in two and then contrasting the two parts.”⁸ There are thus two kinds of novel, two kinds of men, two kinds of cosmology, two kinds of education, and—most relevant for our purposes—two kinds of political society. This social and political duality Bagehot summed up as tradition versus reason, or the “stationary” versus the “progressive” forms of civilization. A stationary state, he believed,

claimed to lay bare for the first time” (p. 214). After Vile, came attacks by Dennis Kavanagh, “The Deferential English: A Comparative Critique,” *Government and Opposition*, 6 (Summer 1971), 333–360; and “An American Science of British Politics,” *Political Studies*, 22 (Sept. 1974), 251–70. Most recent, and perhaps most notable of all, is Samuel Beer, “Tradition and Nationality: A Classic Revisited,” *American Political Science Review*, 68 (Sept. 1974), 1292–95. Unfortunately, I have not been able to use for this essay the recent British Academy lecture by K. C. Wheare on Walter Bagehot.

⁵ Among historians should be included James P. Cornford, who is also a political scientist. See his review of Bob Jessop, *Traditionalism, Conservatism, and British Political Culture* in *Times Higher Education Supplement*, Oct. 4, 1974, 15; and of Robert T. McKenzie and Allan Silver, *Angels in Marble in History*, 58 (Feb. 1973), 127–9. See also Richard W. Davis, “The Whigs and the Idea of Electoral Deference: Some Further Thoughts on the Great Reform Act,” *Durham University Journal*, 67 (Dec. 1974).

⁶ See Olive Anderson’s review of Charles H. Sisson, *The Case of Walter Bagehot in History*, 58 (June 1973),

312.

⁷ Geoffrey Best, *Mid-Victorian Britain 1851–1875* (London, 1971), 236.

⁸ Victor S. Pritchett, “Books in General,” *New Statesman and Nation*, 28 (July 29, 1944), 74.

"is by far the most frequent condition of man, as far as history describes that condition; the progressive state is only a rare and an occasional exception."⁹

Bagehot considered at length the modes of coherence, the organizing principles of these opposing societies. Stability and order preoccupied him. "How to get the obedience of men," he wrote, "is the hard problem."¹⁰ While in his twenties, he observed in his *Letters on the French Coup d'État of 1851*: "The first duty of society is the preservation of society. . . . Legislative Assemblies, leading articles, essay eloquence—such are good—very good—useful—very useful. Yet they can be done without. . . . Not so with all things. The selling of figs, the cobbling of shoes, the manufacturing of nails—these are the essence of life."¹¹ Not many young men of Bagehot's social background talked this way. That Bagehot did so may well have been rooted in personal circumstance. He had grown up in close quarters with the insanity of his mother and half-brother. The recurrent threat of family chaos was an early and terrifying reality, which in all likelihood informed his later experience of society and politics.

Bagehot found the stability and order of traditional society to stem from the human propensity to imitate. This he characterized as both instinctive and unconscious, as something not rooted in the intellect. "At first," he maintained, "a sort of 'chance predominance' made a model, and then invincible attraction, the necessity which rules all but the strongest men to imitate what is before their eyes, and to be what they are expected to be, moulded men by that model."¹² Thus man took the first steps in the general endeavor of "nation making." Thus came to being the "cake of custom," that voluntary, automatic self-discipline, that "hereditary drill," by which the mass of a nation deferred to and followed its leaders.¹³

Bagehot took much satisfaction in enunciating the mimetic principle, which he found at work in Victorian no less than in primitive society. Time after time he gave it emphatic expression. On one occasion he declared he believed "unconscious imitation to be the principal force in the making of national characters."¹⁴ On another occasion he wrote that "the propensity of man to imitate what is before him is one of the strongest parts of his nature";¹⁵ and on still another, that "this unconscious imitation and encouragement of appreciated character . . . is the main force which moulds and fashions men in society as we now see it."¹⁶ Was this the Darwinian social scientist speaking, pleased with his anthropological discovery? Or was it the frightened Victorian gentleman seeking assurance that society really was stable and orderly? Probably both.

Despite his fears of social disorder, Bagehot was ambivalent about tradi-

⁹ Mrs. Russell Barrington, ed., *The Works and Life of Walter Bagehot* (London, 1915), 8: 137.

¹⁰ *Ibid.*, 8: 17.

¹¹ *Ibid.*, 1: 84, 137.

¹² *Ibid.*, 8: 24.

¹³ *Ibid.*, 8: 18.

¹⁴ *Ibid.*, 8: 25.

¹⁵ *Ibid.*, 8: 60.

¹⁶ *Ibid.*, 8: 63.

tional society. Its dreary uniformity, its childlike men and women, above all its lack of freedom distressed the Victorian liberal in Bagehot. He sometimes spoke of traditional society, indeed, as if it were valuable mainly as a basis upon which a progressive one might be built. "The ages of monotony had their use," he wrote, "for they trained men for ages when they need not be monotonous."¹⁷

The unmonotonous age—the age that came so rarely—was one of variety, progress, and, above all, reason. Its origins were more obscure than those of traditional society. But somehow the society of mere order in some few places did the hard thing: it got out of "a fixed law," broke the "cake of custom." This began "in states where the government was to a great and a growing extent a government by discussion, and where the subjects of that discussion were in some degree abstract, or, as we should say, matters of principle."¹⁸ The habit of discussion, once established, brought in its train the cherished goods of a progressive society—freedom, choice, tolerance, science, and invention.

The age of free discussion prompted an even more obvious display of Bagehot's ambivalence. On the one hand, he wrote glowingly of its virtues. Liberty was "the strengthening and developing power—the light and heat of political nature";¹⁹ and in England, he declared proudly, there was "on the whole probably a freer discussion of a greater number of subjects than ever was before in the world."²⁰ On the other hand, Bagehot also entertained a quite different vision of liberty, a fearful vision that it undermined the basis of order: "As soon as discussion begins," he wrote, "the savage propensities of men break forth; even in modern communities, where those propensities, too, have been weakened by ages of culture, and repressed by ages of obedience, as soon as a vital topic for discussion is well started the keenest and most violent passions break forth. Easily destroyed as are early free states by forces from without, they are even more liable to destruction by forces from within."²¹

This was an odd view of man and society, at once humane and inhumane, bold and timid. It was dualistic through and through. The good society needed both tradition and reason, custom and discussion. Without the one there was disorder, without the other sterility. But the two principles, as Bagehot understood them, were at bottom incompatible. Order forbade liberty and liberty order; "discussion and custom," he argued, "cannot be thus combined; their 'method,' as modern philosophers would say, is antagonistic."²² This may explain why in *The English Constitution*—Bagehot's famous account of politics and society in the age of Palmerston—he brought them together as co-existent but wholly separate parts of the English state, their separation presumably guaranteeing that they not destroy one another, and

¹⁷ *Ibid.*, 8: 20.

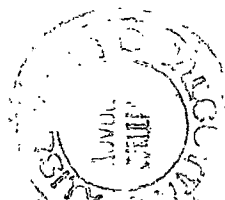
¹⁸ *Ibid.*, 8: 102.

¹⁹ *Ibid.*, 8: 143-4.

²⁰ *Ibid.*, 8: 106.

²¹ *Ibid.*, 8: 117.

²² *Ibid.*, 8: 112.



their coexistence within the same constitutional framework assuring that they somehow support each other. But why, apart from the imperatives of Bagehot's anxious fantasies, need order and liberty, custom and discussion, be wholly separate entities? Bagehot never attempted to answer this question. *The English Constitution*, by far the most memorable of his writings, is thus a curious document. Contradiction and unreality mar its brilliance.

The English Constitution, wherein Bagehot explicitly deals with deference, stands on the same duality as *Physics and Politics*. What Bagehot in one work called tradition and reason in the other he called respectively the dignified and efficient parts of the constitution. This famous dichotomy, to use Bagehot's phrase, was a scheme of "double government."²³ In such a government, he argued, "there are two parts . . . : first, those which excite and preserve the reverence of the population—the *dignified* parts, if I may so call them; and next, the *efficient* parts—those by which it, in fact, works and rules . . . ; every constitution must first gain authority, and then use authority."²⁴ Although Bagehot hastened to add parenthetically that the dignified and efficient parts are "not indeed separable with microscopic accuracy,"²⁵ and although he sometimes let slip a perfunctory reminder that constitutional bodies like the House of Commons or the House of Lords had both parts in them, it was the idea of separation that he constantly reiterated. Certainly this is the idea that his readers come away with.

The efficient parts of the constitution were the House of Commons and the cabinet (the latter incidentally a body Bagehot claimed he had discovered);²⁶ the dignified parts were the monarchy and the House of Lords. The business of the dignified parts of the constitution was to provide those "theatrical elements" in political life that inspired obedience, or deference, in the mass of English society. "The office of an order of nobility," Bagehot explained, "is to impose on the common people . . . , to impose on their quiescent imaginations what would not otherwise be there."²⁷ The conduct of efficient business was for the most part beyond the nobility. "The qualities which fit a man for marked eminence, in a deliberative assembly, are not hereditary and are not coupled with great estates."²⁸ "It is as great a difficulty to learn business in a palace," he said, "as it is to learn agriculture in a park."²⁹

Thus, the landed elite and the royal family functioned chiefly as prompters of deference—to such a degree, indeed, that England was (to use a phrase coined by Bagehot) a "deferential community."³⁰ Characteristically, Bagehot's deferential community contained "crowds of people scarcely more civilized than the majority of two thousand years ago"³¹—or, as he put it even

²³ Walter Bagehot, *The English Constitution* (World's Classics edition, London, 1928), 255.

²⁴ *Ibid.*, 3-4.

²⁵ *Ibid.*, 3.

²⁶ For Bagehot's false claim to originality, see Vile, *Constitutionalism and the Separation of Powers*, 213-219.

²⁷ Bagehot, *English Constitution*, 79.

²⁸ *Ibid.*, 84.

²⁹ *Ibid.*, 106.

³⁰ *Ibid.*, 239.

³¹ *Ibid.*, 5-6.

more brutally, a deferential community was one "in which primitive barbarism lay as a recognized basis to acquired civilization."³² Having, as he added, "whole classes unable to comprehend the idea of a constitution—unable to feel the least attachment to impersonal laws," such a community was fortunate in being able to provide at one and the same time both "a comprehensible element for the vacant many" and "complex laws and notions for the inquiring few."³³ The latter flourished because the efficient part of the constitution—that government by discussion without which, according to Bagehot, "no State can be first-rate"³⁴—found shelter from the depredations of the vacant many (whom Bagehot feared to enfranchise) behind the façade of a theatrical, deference-generating display.

Bagehot's idea of deference was something other than the Harringtonian theory described by J. G. A. Pocock. To be sure it was a deference that a non-elite spontaneously offered to an elite. The members of the non-elite were not virtuous republicans, however, but that new nineteenth-century collectivity—vast, mysterious, intimidating—known as the masses, who on the whole were devoid of property and education. Accordingly, although Bagehot's deference was freely given, there was little of the intellect in it, at least in the form which Bagehot most often, if not invariably, put forward. His deference was much like the mimesis of traditional society in his *Physics and Politics*: "As yet the few rule by their hold, not over the reason of the multitude, but over their imaginations and their habits";³⁵ or again, "the mass of the English people . . . defer to what we may call the *theatrical show* of society. . . . What impresses men is not mind, but the result of mind."³⁶ Deference so conceived, not surprisingly, got Bagehot into difficulties.

How well did Bagehot's leading ideas on deference fit the facts of nineteenth-century English political life? Samuel Beer, not mincing words, contended that Bagehot was blind "to elementary facts of political behavior and of British political experience."³⁷ Bagehot's principal idea, the separation of the traditional and the progressive, the dignified and the efficient, was simply not to be found in practice. The landed elite, said Beer, was at once traditional and progressive. For Bagehot, however, the landed elite seemed little more than an antiquated adornment to the working machinery of government, not unlike those "solely-ornamental wheels introduced into the clocks of the Middle Ages, which tell the then age of the moon or the supreme constellation;—which make little men or birds come out and in theatrically."³⁸ This was hardly an apt figure for one of the most effective ruling elites in world history. Admittedly, like all elites, it had its share of mediocrity and inertia, but on the whole England's was vigorous and businesslike,

³² *Ibid.*, 34.

³³ *Ibid.*

³⁴ *Ibid.*, 311.

³⁵ *Ibid.*, 239.

³⁶ *Ibid.*, 236–7.

³⁷ Beer, "Tradition and Nationality," 1292. My debt to Samuel Beer is large. He was the first to see the close relation between *The English Constitution* and *Physics and Politics*.

³⁸ Bagehot, *English Constitution*, 186.

leaving its mark on all the major departments of English life. What Bagehot overlooked was its obvious propensity for leadership.

If we accept Bagehot's notion of the landed elite as a maker of theatrical display and of little else, then his non-elite becomes merely the passive victim of a giant confidence trick. But few in his own day, the day of Reform Bills and Corn Law agitation, saw class relations in this way. Thus Matthew Arnold, no less intelligent than Bagehot, found the common people "deferential"—his word—but it was a deference that responded to spirited and humane leadership and emanated from ordinary men who were themselves vigorous, high spirited, and full of "a self-reliance which disposed each man to act individually and independently."³⁹ In short, Arnold's was the deference of people who knew where they were being led. They were capable of throwing off their deference, as Arnold thought ordinary Englishmen were doing by 1861, if their customary leaders should no longer provide adequate leadership.

Arnold stood on firmer ground than Bagehot. The idea of deference that Bagehot commonly put forward left little room for the possibility that the lower classes could make a rational and independent choice, little room for the influence of public opinion. Is it too much to say that more went on in Victorian political life than instinctive deference, than the mere registering of the finest brute voters in the constituencies in support of the finest brute votes in the House of Commons?⁴⁰ Bagehot himself was too good an observer to rest content with so misleading an account. On occasion he saw things differently. This Bagehot is not as visible as the first. But neither is he hard to find.

Indeed Bagehot has much more to say on public opinion than might be expected from the Darwin of deference. After all, he was also the eulogist of cabinet government, which made for systematic parliamentary discussion, for the "great scene of debate, the great engine of popular instruction and political controversy . . . , the best means yet known for arousing, enlivening, and teaching a people."⁴¹ Presumably, this instruction caught on, for Bagehot next wrote of "the opinion out of doors, the secret pervading disposition of society" that had a "great influence" on how members of Parliament voted. "The nation feels," he wrote, "that its judgment is important, and it strives to judge."⁴² Perhaps, however, Bagehot had in mind some narrow and superior part of the nation, not its wide and inferior part. But later in *The English Constitution*, the reader comes across the phrase "the ordinary intelligent many"⁴³ (having earlier encountered the "vacant many"), and still later he finds the statement that a "great many ideas, a great many feelings, have gathered among the town artisans—a peculiar intellectual life has sprung up among them."⁴⁴

³⁹ P. J. Keating, ed., *Matthew Arnold: Selected Prose* (Harmondsworth, 1970), 108.

⁴⁰ Bagehot, *English Constitution*, 125. "A cynical politician is said to have watched the long row of county members, so fresh and respectable-looking and muttered, 'By Jove, they are the finest brute votes in Europe!'"

⁴¹ *Ibid.*, 17.

⁴² *Ibid.*, 18-19.

⁴³ *Ibid.*, 73.

⁴⁴ *Ibid.*, 153-4.

Not surprisingly, the Bagehot of "public opinion" created problems for the Bagehot of "deference." In one of his last essays, "Lord Althorp and the Reform Act of 1832," a new kind of deference appeared that Bagehot called "intellectual deference."⁴⁵ Presumably, this was a deference rather like Arnold's, grounded less on instinct than on intellect. According to Bagehot, "Intellectual deference used to be paid to members of Parliament," but the reformers of 1832 "destroyed intellectual constituencies in great numbers without creating any new ones."⁴⁶ Despite its apparent death in 1832, intellectual deference was still alive for Bagehot in the 1870s. In his introduction to the second edition of *The English Constitution* he grudgingly conceded that English voters, however deferential, exercised their intelligence to the extent of choosing between two rich candidates, although he belittled this choice as merely deciding on an issue "selected by the higher classes."⁴⁷ A few pages later, however, he confessed that "in excited states of the public mind,"⁴⁸ the higher classes were not free to select the issue. Then, a few pages further, the Bagehot of "public opinion" would seem to have virtually routed the Bagehot of "deference." Whereas in the first edition of *The English Constitution*, Bagehot had written amusingly on the fondness of the English voter for baronets and earls ("a manufacturer's son has no chance" against them),⁴⁹ he now wrote that Englishmen individually liked a lord, but when they acted in concert—as in elections—they easily found themselves committed to "anti-aristocratic sentiments . . . , and their collective action may be bitterly hostile to rank."⁵⁰

Clearly there are dangers in using Bagehot as a guide to English political life in the nineteenth century. If there is a Bagehot of "deference," there is also a Bagehot of "public opinion," and equally disconcerting, the Bagehot of "deference" does not speak with one voice. Whatever the voice, however, it is an immensely seductive one, consciously employed by a brilliant journalist who, as he said, coveted "power, influence over people's wills, faculties, and conduct more in proportion than I can quite defend."⁵¹ These he gained, both in his own day and ours, but it may be time to resist his blandishments. Historians might begin by using the word "deference" with greater care—at least until something more comes of recent inquiries into its meaning,⁵² and something more is done to investigate the behavior of Victorian voters.

⁴⁵ Barrington, *Works*, 7: 69.

⁴⁶ *Ibid.*, 69–70.

⁴⁷ Bagehot, *English Constitution*, 265.

⁴⁸ *Ibid.*, 270.

⁴⁹ *Ibid.*, 148.

⁵⁰ *Ibid.*, 278.

⁵¹ Quoted in Norman St. John-Stevan, *Walter Bagehot* (London, 1959) p. 25.

⁵² Among the sociologists, the work of Howard Newby is notable. See "The Deferential Dialectic," *Comparative Studies in Society and History*, 17 (April 1975), 139–164. Newby's sociology of deference is a distinct improvement on Edward Shils, "Deference," in J. A. Jackson, ed., *Social Stratification* (Cambridge, 1960), 104–132. Among political scientists, the work of Bob Jessop is notable. See his *Traditionalism, Conservatism, and British Political Culture* (London, 1974), especially chapters 2 and 7.

Deference and Aristocracy in the Time of the Great Reform Act

RICHARD W. DAVIS

A THEORY OF DEFERENCE has recently played a significant role in modifying older explanations of the intention and effects of the Great Reform Act. The historian chiefly responsible for the new interpretation is D. C. Moore, who insists on "the electoral importance of what might be called the 'deference community,' the community of men who lived in close contact with one another, who had the same occupation or were connected by the same 'interest,' and—most important of all—who recognized the same individual, or individuals, as their social, economic, and ideological leader or leaders." These deference communities, Moore contends, were all-important and all-pervasive. Every elector was a member of one or another community, and the great bulk of the electorate tamely followed the preferences of the relatively few leaders. Moore goes so far as to assert: "For conceptual purposes, of course, deferential behaviour should be distinguished from behaviour motivated by economic, social, or religious interest. In practice, however, in most constituencies for most of the century this distinction cannot be made."¹

Moore argues that Lord Grey and his colleagues in the reform ministry were aware of this phenomenon and built their measure upon it. They had no intention of increasing the electoral power of the middle classes or of giving the individual elector a real voice. Rather, they sought to strengthen the influence of the leaders in the several deference communities, most particularly the influence of the larger landowners, the class that had always dominated English politics. The Reform Act was intended, then, not as a concession, but as a cure.²

John Cannon and Michael Brock have argued convincingly, however, for

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¹ D. C. Moore, "Social Structure, Political Structure, and Public Opinion in Mid-Victorian England," in Robert Robson, ed., *Ideas and Institutions of Victorian Britain* (New York, 1967), 36, 47.

² D. C. Moore, "The Other Face of Reform," *Victorian Studies*, 5 (1961), 7-34; D. C. Moore, "Concession or Cure: The Sociological Premises of the First Reform Act," *The Historical Journal*, 9 (1969), 39-59. Moore's arguments will doubtless be developed further in his forthcoming book. In the meantime, the author must acknowledge a private correspondence that has been most helpful in clarifying our differences, even if it has not removed them.

the traditional view of the Reform Act as a concession. But not altogether consistently, they continue to accept Moore's notion of electoral deference, or something very like it. Cannon accepts the notion as applied to counties and smaller boroughs, and he agrees—quite rightly—with Moore in stressing the continuing weight of such constituencies in the electoral system.³ And Brock, in attempting to explain away the apparent implications of the Whig emphasis on a propertied and independent electorate, writes that the Whig view “by which these apparently conflicting principles were reconciled was that the right kind of deference flourished best among men of some property and education.” “Such men,” he says, “deferred to the local magistrate in the only way that was still entirely unobjectionable: they accepted his advice and leadership in politics, not because they had no alternative, but because they knew enough to recognize his superior knowledge. The borough electorate of £10 householders could thus be held to combine all the virtues, the voters being at once propertied, deferential, and independent.”⁴

The theory of deference advanced in the last few years as an explanation of the Great Reform Act, whether right or wrong, is new. It obviously bears little relation to the classical theory discussed by J. G. A. Pocock, for that left the many free to choose between the men and the measures of the few. As David Spring makes clear, Walter Bagehot, who did so much to popularize the notion of deference in the nineteenth century, with an influence lasting to the present day, was in fact confused—and hence confusing—on the question. His writings really reflect little interest in, or understanding of, the electoral process. But Bagehot was at least occasionally drawn toward the concept of what he called “intellectual deference” in which the electorate exercised a rational and independent choice in selecting between their upper-class leaders. No one would doubt that England had a deferential society in the sense that the mass of the electors voted for their betters, but this is not at all the same thing as voting as their betters told them to. Not until recently has it been argued that deference implies the obedient casting of a vote according to the preference of a social superior.

Not only is modern theory largely without precedent, but also it contradicts the privately and publicly expressed opinions of the act's framers. Contrary to Moore's contention that they did not distinguish between influence and public opinion, they made a very clear distinction. Writing to Lord Palmerston in October 1831, Grey identified what he believed comprised public opinion and distinguished it from the most prevalent form of influence when he spoke of “the middle classes, who form the real and efficient mass of public opinion, and without whom the power of the gentry is nothing. . . .”⁵ He told Maurice Fitzgerald, the Knight of Kerry, that the times required that “a greater influence may be beneficially exerted upon the government by im-

³ John Cannon, *Parliamentary Reform, 1640–1832* (Cambridge, 1973), 246–7, 255–6.

⁴ Michael Brock, *The Great Reform Act* (London, 1973), 143.

⁵ Grey to Palmerston, Oct. 10, 1831, Broadlands Manuscripts GC/GR/2042/1–3. The author is grateful for the permission of the Trustees of the Broadlands Archives to make use of the papers at the National Register of Archives.

provements in the representative system. . . .”⁶ Advising his son, Lord Howick, on a speech to his new Northumberland constituents in May 1831, Grey said that it was the aim of the reform bill “to give the People an efficient voice in the conduct of their own affairs, and to provide the best security both for the Crown and the other Branch of the Legislature, by connecting their interests with those of the Publick.”⁷ Lord Melbourne took up a similar theme in October, extolling to the king’s secretary, Sir Herbert Taylor, “the advantages derived from popular institutions . . . , which mainly consist in the contentment, acquiescence, and satisfaction derived from the consciousness felt by the people that they either watch over their own interests, or have each of them an equal voice in nominating those whom they trust with that duty.”⁸ And Lord Lansdowne, like Melbourne one of the more cautious reformers in the cabinet, never contemplated a plan that would not satisfy the middle classes and, as he wrote to Grey in January 1831, “give persons of small property a decidedly larger share of power.”⁹ These statements do not square with the intentions, ascribed by recent historians to the ministers, of a merely passive role for the mass of the electorate.

There is ample evidence that Grey and his colleagues did not subscribe to such simple and all-embracing explanations of electoral behavior as those suggested in the modern theory of a deferential electorate. Writing to Taylor in November 1831 Grey argued that “by giving a second representative to the great towns you will ensure the return of one of the master manufacturers and people of property in the towns; whereas if there was only one, there might be an apprehension that he would be returned by the class of operatives. . . .”¹⁰ Clearly Grey was apprehensive that the operatives might not behave deferentially, but rather vote according to their own inclinations. To encourage compromise the more usual situation of two members and two votes for each elector was to be introduced.

This device was based on experience, and not only or even primarily on experience with large urban electorates. As Lord Althorp remarked during the debates of the summer, every gentleman who had ever been engaged in a county contest knew that “there were always private and personal considerations which induced electors to give their second vote in a different manner to that in which they had given their first. . . .”¹¹ There would be no place in the historians’ deferential electorate for “private and personal considerations.” The framers of the Reform Act made allowance for such considerations because they knew they had to.

⁶ Grey to the Knight of Kerry, n.d., Grey Papers (Department of Palaeography and Diplomatic, University of Durham).

⁷ Grey to Howick, May 6, 1831, *ibid.*

⁸ Lloyd C. Sanders, ed., *Lord Melbourne’s Papers* (London, 1971), 136.

⁹ Lansdowne to Grey, Jan. 18, 1831, Grey Papers.

¹⁰ Grey to Taylor, Nov. 30, 1831, RA Add 15/1618 (Royal Archives, Windsor Castle). See also Henry, Earl Grey, ed., *The Correspondence of the late Earl Grey with H. M. King William IV* (London, 1867), I, 454. The author was able to make use of the material in the Royal Archives by gracious permission of Her Majesty Queen Elizabeth II.

¹¹ *Hansard’s Parliamentary Debates*, 3rd series, V, 1371.

The framers of the act also knew, of course, that electors would continue to be exposed to various pressures and also to what might be called coercive, as opposed to moral or intellectual, influence. But they clearly intended to minimize the effects of such pressure and influence. Furthermore, the intent to create a deferential electorate, in the sense in which that term has been recently used, cannot realistically be attributed to men such as Grey. They could hardly have expected to create an electorate that merely reflected the opinions of its social superiors, or if they did, they went about it in a very strange way.

The most significant evidence of the ministers' intention was their first plan, drawn up in December 1830 by a committee chaired by Lord Durham and composed of Lord John Russell, Sir James Graham, and Lord Duncannon, and accepted with some modifications by the cabinet in January 1831. The £20 franchise for boroughs was the most significant element in the proposal. The usual explanation of this high franchise is that it was a concession extorted from Durham in return for an agreement by his colleagues on the committee to support the ballot. But all the explanations at the time have just the reverse emphasis. Durham himself suggested to Brougham that "if you conceded the Ballot in the first instance you might easily obtain (so great is the desire for the Ballot) a much higher qualification, as the price of it, which would assure you an independent and excellent constituency."¹²

Grey told Taylor that it was precisely this argument that had been used in the cabinet. The ballot, Grey said, "was not proposed even by the framers of the report, as one to which they were themselves partial, but as a concession which would greatly facilitate the raising of the elective franchise, in Cities and Boroughs. . . ." Grey went on to explain that he, Brougham, and Lansdowne among others had argued strongly against the ballot. It had been rejected, though, he said, some still thought it a necessary concession to secure the acceptance of the higher franchise, which all apparently desired and which was retained.¹³ Grey also intimated to the king that he would have been willing to accept the ballot had it been the only way to get the high franchise.¹⁴ Finally, there is Russell's testimony as to the framers' intentions and priorities. Writing to Durham on February 13, 1831, he said: "When we agreed to make ballot a part of the plan to be proposed to Lord Grey, we did so on the ground that such a concession to popular feeling would enable us to vest the elective franchise in a body above corruption, and deeply concerned, by reason of their property, in preserving our mixed Constitution."¹⁵ Russell wrote to Durham in great consternation. He had just discovered that a £20 franchise would create minute electorates in many smaller boroughs and, he said, make the government a laughing stock. The ministers then decided to revert to the £10 franchise, which had been the previous proposal before the committee.

¹² Durham to Brougham, Dec. 30, 1830, Brougham Papers (University College, London).

¹³ Grey to Taylor, Feb. 8, 1831, RA Add 15/1288. See also *Corresp.* I, 114.

¹⁴ Grey to William IV, Feb. 5, 1831, RA Add 15/1283. See also *Corresp.* I, 106.

¹⁵ Russell to Durham, Feb. 13, 1831, Grey Papers.

The Whigs' mastery of detail left something to be desired, and too much cannot be made of the facts and figures they threw about with such reckless abandon. But some idea of what they envisioned for most boroughs from the higher franchise may be gained from Russell's remark to Grey that householders of £7 to £8 in small boroughs were "equal to £20 in large towns."¹⁶ The ministers desired and intended to create a very select franchise in the majority of boroughs. It is hardly likely that they would have expected the resulting electorate to be deferential. Rather, there would have been every reason to anticipate, in Durham's words, "an independent and excellent constituency"; or, as Russell said, to believe that the franchise would be vested "in a body above corruption, and deeply concerned, by reason of their property, in preserving our mixed Constitution."

Talk of a mixed constitution and independent electors suggests an explanation of the Reform Act that has never had sufficient attention. We have been accustomed to regard the framers of the act as proto-democrats, as pragmatic conservatives, or more recently as possessed of a distinctive sociological theory. But Russell, speaking of the first plan, suggested a different interpretation. The plan contained, he said, "hardly anything new. Lord Grey and other reformers had repeatedly since the year 1780 drawn the great outlines of Parliamentary Reform."¹⁷ Historians have been too inclined to see the debate in the 1830s as isolated and arising almost solely out of the conditions and necessities of that period, largely ignoring the eighteenth-century roots of the question. But, as Russell, who was steeped in the thought of the earlier period, reminds us, Grey was an eighteenth-century reformer. His ideas had been formed before what is conventionally dubbed the Industrial Revolution and the social and economic changes that accompanied it had begun to make much impact on men's minds, and before they talked much about the middle classes. Though Grey's notions had been modified and adapted to new conditions and issues, basically they had changed very little.

The usual contrast drawn between a radical young Grey and a conservative old one is misleading. Undoubtedly, much of what he had advocated in his previous reform proposals of 1797, he contended against as dangerous radicalism in the 1830s. Household suffrage, roughly equal constituencies, triennial parliaments, and single-member constituencies are outstanding examples. But these were specifics, the means to implement the ends. And in the earlier period, as in the later, Whig reformers tended to be much sounder on principles than they were on details. If one turns one's attention to the arguments and principles of the 1797 proposals, one will find little that would have been out of place in the 1830s.

No more in the earlier period than in the later did the Whig advocates of reform believe that it would tend toward democracy. Grey assured the House of Commons in 1797 that "he wished to alter no part of the constitution." "It was his desire," he said, "that it should remain as it had been established,

¹⁶ Russell to Grey, May 23, 1831, *ibid.*

¹⁷ Russell to Durham, Oct. 19, 1834, Russell Papers, British Museum Add Ms 38080 ff 73-8.

composed of King, Lords, and Commons." Not only did he wish to maintain a mixed and balanced constitution, he also foresaw no fundamental change in the social composition of the House of Commons. Under his plan, the landowner "would find his property suitably represented; the merchant would find support in the householders; and men of respectability and talents in the different professions, would find a fair door open for getting into parliament. The only persons whom he would wish to exclude from that House, were men who were neither possessed of landed property, nor engaged in commercial enterprise, nor professors of any particular science; but men who, without property, without industry, and without talents, obtained seats in the House of Commons by the influence of great men, for the purpose . . . , not of consulting for the good of the people, but of promoting their own interests."¹⁸ This was Grey's stand in 1797.

It is instructive to set against this Grey's advice to Howick in 1831. The reform bill, he said, was of a "conservative nature." It would give "the People an efficient voice in the conduct of their own affairs" and "provide the best security both for the Crown and the other Branch of the Legislature by connecting their interests with those of the Public." The bill, said Grey, would take nothing from the higher orders that they ought to have. The influence of "high station and of property" would remain if those who enjoyed those advantages showed "by their conduct a claim to the confidence and good opinion of their fellow subjects." But the wealthy and the powerful would "no longer have the power of nominating members, either for money, which in the case of the freeholder would be called a bribe, or for personal advantages to Members or their families." Nor would they "retain the power of dictating to the electors." These not only formed "no part of the constitution," but were in "direct opposition to its fundamental principles," which established "the right of *representation* and not of *nomination*. . . ."¹⁹

Both arguments demonstrate the same concern for a balanced constitution and a common desire to rid the constitution of its corruptions and give the electorate a positive rôle. Fundamental to both arguments is a belief that the electorate will recognize its natural leaders, and that they will be worthy of being so recognized. Indeed the basic arguments in the two periods are indistinguishable. The old "Friend of the People" had not put away the ideas of his youth.

The notion of the independent electorate had a similar source in the earlier debates, though in 1797 it was Charles James Fox rather than Grey who had enlarged upon the idea and defined "the most perfect system" as that "which shall include the greatest number of independent electors, and exclude the greatest number of those who are necessarily by their condition dependent."²⁰ As it was reckoned that the householder franchise which Grey proposed in 1797 would produce only 600,000 electors as opposed to 3,000,000 under

¹⁸ William Cobbett, *Parliamentary History*, 33 (London, 1818), 650.

¹⁹ Grey to Howick, May 6, 1831, Grey Papers.

²⁰ Cobbett, *Parliamentary History*, 33, 727.

universal suffrage, clearly the earlier franchise (as well as the later) was intended to be highly selective.²¹

For the framers of the Reform Act, it was not difficult to identify the middle classes with the independent electorate which was the ideal of eighteenth-century reformers. The Whigs endowed both with the same virtues of independence, excellence, and intelligence. Not that they had a precise definition of middle class in mind. Indeed, theirs was hardly a definition at all, referring pretty literally to all those who lay in the middle sections of society, between the greater landowning classes on the one hand, and the unskilled manual laborers on the other. Certainly the middle classes were not associated in their minds exclusively with urban and industrial areas. That association is a later one, and it has long confused discussions of the Great Reform Act. What Grey once called "the middle and more influencing classes"²² existed in Buckinghamshire as well as Birmingham, in Wiltshire as well as Westminster; and everywhere their property, intelligence, and independence made them worthy candidates for the franchise, and for a greater power in the political system. Whether the manufacturing or mercantile interest should predominate over the landed interest was seen by everyone as a crucial question—but it was a different question. The Whigs never dismissed rural and agricultural England as subservient. That is a later notion.

The Whigs intended to introduce a genuine political life throughout the system. Because they did not see the two intentions as incompatible, they also contended that they and their kind should continue to lead. Arguing for the enfranchisement of Manchester and Birmingham in 1821, Russell clarified not only the Whig attitude on the benefits of extending the representation, but also on the roles and relationships of candidates and their constituents:²³

In neither of these rich and populous communities were there individuals to whom, from their rank or official station, the people were accustomed to look for the tone and colour of their political opinions. In towns that were represented, however violent the politics of those towns might be, there were certain persons candidates for seats in that House, who acted under the control of public opinion, and who gave a consistent colour to the opinions of the body whom they wished to represent. A popular election, besides giving vent to discontent, embodied the vague wishes of hostile parties, and forced all to seek some object which had at least a plausible and legal appearance. It was to the want of any such political centre in these towns, that he was inclined to attribute some of these unfortunate occurrences which had taken place amongst them since the war. There was no authority to which they could conform, or from which they could derive instruction.

Candidates and electors were seen as mutually influencing. Candidates would educate and moderate, give tone and color, but always within limits set by public opinion. It was a genuine political process the Whigs had in mind. They did not confuse political leadership with ideological control of the electorate.

²¹ *Ibid.*

²² Grey to Taylor, Jan. 18, 1832, RA Add 15/1687. See also *Corresp.* II, 137.

²³ *Hansard's Parliamentary Debates*, New Series, V, 615. The author is grateful to Harold Ellis for drawing this quotation to his attention.

The strong Whig aversion to the ballot, probably shared by all the ministers save for Durham and Althorp, may seem to argue against such a view. Yet the Whig dislike of the ballot and preference for open voting must be viewed in the context of the whole elaborate system of checks and balances in which eighteenth-century, and thus Whig, thinking delighted. William IV, in remarks which Grey said had the sympathy and concurrence of his ministers, objected to the ballot because he said it would be "a protection to concealment, would abolish the influence of fear and shame, and would be inconsistent with the manly spirit and the free avowal of opinion which distinguish the people of England."²⁴ Russell believed that it would lead to "bribery and profligacy."²⁵ These remarks do not necessarily suggest anything sinister. So long as the vote was seen not as a right, but as a public trust, open voting was viewed as another means of ensuring that an elector would act with deliberation and responsibility; secret voting, as Russell suggests, was disliked because most Whigs believed that it would lead to corrupt and self-interested behavior. But Whig aversion to the ballot must be seen in context, and their evident concern for electoral independence set against it. Their dislike of the enfranchisement of tenants-at-will in the counties by the Chandos Clause was genuine, their aversion to coercion by landlords real.²⁶ Yet no more in the elector than in the Member of Parliament did they wish to encourage irresponsibility. As the M.P. had often to weigh other loyalties and convictions against the displeasure of his constituents, so the elector should be exposed to a play of influences. It was the strong, however, not the weak, who should feel its pressures.

Whig ideology gave a large role to the people, by which they meant men of independent opinions, the middle classes. Whig ideology and Whig experience also gave the leading role to the aristocracy. They did not believe that the entrenched influence of the unreformed system was necessary to preserve that leading role to the aristocracy. Of course they recognized the influence of property. Of course they hoped that their own dependents would defer to their wishes. But in the final analysis the Whigs put their faith elsewhere. As Lord Holland said before the first elections under the act, they had "only to find good Candidates and the people will chuse them."²⁷

²⁴ William IV to Grey, Feb. 4, 1831, RA Add 15/1279. See also *Corresp.* I, 97.

²⁵ Russell to Durham, Oct. 19, 1834, Russell Papers, BM Add Ms 38080 ff 73-8.

²⁶ See, for example, Grey to Durham, Aug. 28, 1831, Grey Papers; Melbourne to Brougham, Sep. 20, 1832, Brougham Papers; Russell to Palmerston, Sep. 23, 1832, Broadlands Manuscripts GC/RU/4; Grey to Palmerston, Sep. 21, 1832, *ibid.* GC/GR/2147/2.

²⁷ Holland to Grey, Jun. 11, 1832, Grey Papers.

On Decolonization and Informal Empire

ROBIN W. WINKS

TO DECOLONIZE AN EMPIRE usually entails one of three processes.¹ If the imperial masters believe firmly in the doctrine of preparation, with its notions of trusteeship or stewardship, then they must assume a series of measurable, clearly defined steps through which a colonial society passes until it may be deemed ready for independence, a readiness to be judged in part by the colonizer and in part by the colonized, but seldom in equal measure between them. Since the doctrine of preparation implies clear stages of progress toward the stated goal of resuming those rights held only temporarily in trusteeship by an imperial power, the historian should be able to judge—perhaps only dimly at the time but more clearly with hindsight—where in the scale of readiness the colony stands. To be sure, since the imperial power defined the stages through which a colony must pass, and in large measure was the only effective judge of where amidst those stages the colony stood at any given moment, the imperial nation controlled the process to the end. After all, it and not the colony had designed the ladder to freedom, had defined each rung upon it, and could tell the colonial—by invoking emergency powers, or suspending a constitution here and there—how soon he might contemplate taking the next step. If the colonial seemed unclear as to the lesson he had learned, he might be asked to go through one or more of the steps again. As Tom Mboya remarked: “Efficiency is the last refuge of the imperialist.”²

This first, gradualist form of decolonization carried with it a strong assumption that the appropriate means by which progress toward the top rung on the ladder was best measured lay in the political sphere, ordinarily (in the British Empire) through progressive transfer to the colony of the Westminster model

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¹ It will be obvious to readers knowledgeable in the field of imperial studies that this paper owes a large intellectual debt to the fundamental work of John Gallagher, Eric Stokes, Ronald Robinson, and D. A. Low.

² The author draws here, and elsewhere, upon his own essay, *Failed Federations: Decolonization and the British Empire* (Nottingham, 1971), Cust Foundation Lecture.

of Parliament. One result of this assumption is that those who held to the doctrine of preparation saw decolonization first in political terms and only then in economic, social, or intellectual ones—that is, to one-half of an idealist conception of culture. That there was a catch to the process was clear enough: rioting or corruption (as defined by the West) on the part of the indigenous society would be taken as evidence that the process had been carried out too quickly, justifying slowing or stopping the process entirely.³

The indigenous elites saw the two ironies inherent in this situation soon enough. The first was that many former colonies became independent—that is, received back their full rights after the period of trusteeship—only by accepting independence upon imperial terms. At the very moment of birth the new nation was, in the eyes of many subgroups within it, fatally compromised; its form of government was not autochthonous,⁴ its independence being a gift rather than an assertion, its very nature Europeanized. For where independence comes by evolution, rather than by revolution, as even Canada learned, the nation's definitions of self, its nationalism, would be of a nature often radically different from other nationalisms. The second of the ironies followed from the first, from the necessity on the part of the indigenous leadership to accept formal decolonization according to a theory designed for the colony from outside. Zenobia, in Boris Vian's play *The Empire Builders*, asked the question: "... if there's no staircase any longer, after we've moved up once again," where are we to go?⁵ What is one to do when the ladder of success no longer exists, having been carted away along with other now extraneous carpentry by the master craftsmen as they departed? In fine, nationalist leaders of a colony found that they had one set of goals as they worked toward independence, and that these had to be exchanged almost overnight for another set of goals as those leaders became (assuming that they succeeded in doing so) the inheritors of power. Developmental goals differed from custodial goals, and accordingly so did the tactics for economic or further political disengagement.⁶

Now it must be clear that when a historian looks at the decolonization of a so-called informal empire, the process must seem even less clearly defined than in the case of evolutionary independence from a formal condition of

³ Of a large literature, see in particular Frederick Madden, "Some Origins and Purposes in the Formation of British Colonial Government," in Kenneth Robinson and Madden, eds., *Essays in Imperial Government, Presented to Margery Perham* (Oxford, 1963), 1–22; Robinson, *The Dilemmas of Trusteeship* (London, 1965); Ged Martin, *The Durham Report & British Policy* (Cambridge, 1972); D. B. Swinfen, *Imperial Control of Colonial Legislation, 1813–1865* (Oxford, 1970); Geoffrey Bolton, *Britain's Legacy Overseas* (Oxford, 1973); and Francis G. Hutchins, *The Illusion of Permanence: British Imperialism in India* (Princeton, 1967).

⁴ See Kenneth C. Wheare, *The Constitutional Structure of the Commonwealth* (Oxford, 1960).

⁵ See also Aimé Césaire, *Discourse on Colonialism* (New York, 1972).

⁶ Richard Symonds, *The British and Their Successors* (London, 1966); Brian Crozier, *The Morning After: A Study of Independence* (Oxford, 1963); William P. Kirkman, *Unscrambling an Empire: A Critique of British Colonial Policy, 1956–1966* (London, 1966); William B. Cohen, *Rulers of Empire: The French Colonial Service in Africa* (Stanford, 1971); Jesse H. Proctor, Jr., "The Development of the Idea of Federation of the British Caribbean Territories," *Revista de Historia de América*, XXXIX (June 1955), 61–105; Hugh W. Springer, *Reflections on the Failure of the First West Indian Federation*, Harvard University Center for International Affairs, *Occasional Papers in International Affairs*, no. 4 (July 1962); and Arvin W. Murch, "Political Integration as an Alternative to Independence in the French Antilles (unpubl. paper, Yale University, n.d.).

"tutelage." Since none of the preparatory stages could be applied consciously, consistently, or fully within an informal sphere of influence, it is difficult to say when they ceased to be applied, if at all. While scholars may clearly date significant shifts by the metropole away from segments of an informal empire on the basis of treaties signed, or not signed, protective obligations taken up or dropped, consulate posts opened or closed, these obviously are only the upper rungs of particular ladders and do not reveal the nature of the rungs that preceded. And while some areas embraced within the concept of an informal empire may reflect similarities to formal empire, as when Rui Barbosa in Brazil declared England to have been "the great teacher" of his liberal principles, or Joaquim Nabuco asserted that he was "an English liberal . . . in the Brazilian Parliament," as though he were "working under the orders of Gladstone"⁷—thereby reflecting an acceptance of the notion of stages in the political transfer of Westminster's models to unannexed territories—such areas cannot be constitutionally defined. Thus, while many elements inherent in the first manner of decolonizing a formal empire may be present in an informal empire as well, such decolonization is more evolutionary, less clearly marked upon the statute books, and far more open to historical controversy.

If this first manner of decolonization tends, despite caricature, to coincide reasonably well with the image (usually political) held by the defender of empire, the second manner coincides, also despite caricature, with the image (usually economic) held by the enemy of empire. Of course, the first manner may still admit of the idea that decolonization generally occurred when imperial nations found they no longer were obtaining economic benefit from an area (as Eric Williams argues for the West Indies, for example),⁸ were unable to commit sufficient resources to an area in the face of concerted internal opposition to their presence (the oft-voiced argument with respect to postwar British India, as another example),⁹ or were simply defeated in an outright war by courageous freedom fighters (some could cite Aden) or by the burden of world opinion directed against them once they had crushed such freedom fighters (Kenya comes to mind). That these notions are simplistic makes them no less compelling, for it remains a truism of history that it is what a people believe to be true of the past that is important, not what the good grey historian may say. People are motivated by their beliefs not by the footnotes of scholars. Thus—the New Sentimentalism of Heroic Violence, attributed to Frantz Fanon by those who have not read him carefully, which would see the Boxer Rebellion, the rising in the Bushire highlands, the Mat Salleh Revolt of

⁷ Richard Graham, *Britain and the Onset of Modernization in Brazil, 1850–1914* (Cambridge, 1968); Graham, "Sepoys and Imperialists: Techniques of British Power in Nineteenth-Century Brazil," *Inter-American Economic Affairs*, XXIII (Autumn 1969), 23–37.

⁸ Eric Williams, *Capitalism & Slavery* (Chapel Hill, 1944). This argument has been much contested, more recently and effectively by Roger T. Anstey in "Capitalism and Slavery: A Critique," *Economic History Review*, 2nd ser., XXI (ii/1968), 307–20; Anstey, *The Atlantic Slave Trade and British Abolition, 1760–1810* (Cambridge, 1975); and Howard Temperley, "Eric Williams and Slavery," in his *British Antislavery, 1833–1870* (Columbia, S.C., 1972), 273–76. See also Edith F. Hurwitz, *Politics and the Public Conscience: Slave Emancipation and the Abolitionist Movement in Britain* (London, 1973).

⁹ The view is encapsulated on page 146 of Stanley Wolpert, *India* (Englewood Cliffs, N.J., 1965).

1894–1905, and the recent renewed attack on American periodicals in Canada as efforts at decolonization within still informal empires.¹⁰ That all four of these acts arose from different dynamics does not set aside one generalization: this second scenario of decolonization, whether factually wrong or right, places heavy theoretical emphasis upon economic relations.

But a third process of decolonization may occur in both formal and informal empires. Perhaps it is best (if not adequately) described by Karl Deutsch in his *Nationalism and Social Communication*, in which he suggests six means by which a nation may come to a definition and awareness of itself.¹¹ One of these is by mutual interdependence, that is, by finding a definition that turns, at least in a major part, upon a symbiotic relationship with another nation. Deutsch cites Argentina in its relationship to Britain, an excellent if traditional example, since even the British referred to Argentina as the Sixth Dominion.¹² The arrested development of Argentina may be compared to that of Canada, because of Britain's economic significance to each, as well as the fear of the United States in each. Indeed, one may argue that both Argentina and Canada showed the tendencies of staple dependency, of the inflexible economies thus induced, and of a reluctance to move rapidly on the road to "decolonization" because of the way staple dependency closed off options and because of the resultant fear of roads not taken and superpowers that might lie at the end of those roads.

Another even more apposite—although much less studied—example of the clear fact that annexation or possession was of little significance to the mutual responsiveness between Britain and another people in the nineteenth century arises from a comparison between Uruguay—not annexed and of the informal empire—and New Zealand, annexed, settled, and turned into the market garden of the empire. To look at the nature of the two economies, whether at the railway networks in terms of mileage or investment, or at the impact of Liebig's Extract of Meat Company in Fray Bentos compared with New Zealand's wool and meat shipments after the introduction of the refrigerated vessel "Dunedin" in 1882, or at any of several other appropriate economic indices to dependence, is to find them similar in all aspects save one: the movement of peoples. Put differently, the only matter of significance that differentiates the degree of dependency of New Zealand and Uruguay upon the United Kingdom, especially after the Mills Committee report of 1862, lies not in the economic sphere, and not greatly in the political, but in the social.¹³

¹⁰ On Fanon, see David Caute, *Fanon* (London, 1970); and Irene L. Gendzier, *Frantz Fanon* (New York, 1973). On the Boxer Rebellion, Victor Purcell, *The Boxer Uprising: A Background Study* (Cambridge, 1963); and on Mat Salleh see K. G. Tregonning, *A History of Modern Sabah (North Borneo, 1881–1963)* (2nd ed., Singapore, 1965).

¹¹ Karl Deutsch, *Nationalism and Social Communication* (2nd. ed., Cambridge, Mass., 1966). See also William J. Foltz, "Building the Newest Nations: Short-Run Strategies and Long-Run Problems," in Deutsch and Foltz, eds., *Nation-Building* (New York, 1966), 117–31.

¹² See H. S. Ferns, *Britain and Argentina in the Nineteenth Century* (Oxford, 1960), and Arthur P. Whitaker, *Argentina* (Englewood Cliffs, N.J., 1964).

¹³ This comparison has not been developed, and it is one I hope to pursue. On Uruguay, see Peter Winn, "British Informal Empire in Uruguay, 1806–1914," unpubl. paper read in 1968 at the American Historical Association meeting in New York City. An expanded version of the paper appears in Winn, *El imperio*

Of course, national identities also were shaped by another form of mutual responsiveness in keeping with the Deutschian model. Deutsch suggests that national identities may arise in part from negative responses to possible absorption from another quarter. We cannot understand Canada's national identity without understanding the role the United States played in the creation of an overlapping informal empire, competing with Britain's more formal one;¹⁴ nor can we understand New Zealand without understanding Australia; Australia without understanding that nation's fear of Japan and later of Indonesia; Uruguay without understanding both Brazil and Argentina, as well as the reinforcing images held by each of the other. In an important sense, then, while there may be many quantifiable indicators of the changing status of informal dependency one people had with another, the real search for an understanding of how the one wished to promote "decolonization" from the other lies in intellectual, rather than in social, political, or economic history.

The idea of the "informal empire" has been with us for some time. In 1934 Charles R. Fay, in his book *Imperial Economy and its Place in the Formation of Economic Doctrine, 1600-1932*,¹⁵ used the phrase to describe Britain's expanding commercial and financial power beyond political jurisdictions. Fay was looking at the impact of the Ottawa Agreements upon British interests already present in the Río de la Plata, however, and some who have taken up the phrase appear to have missed the point that one reference was to a conscious act of choice on the part of the British in an area in which their presence was already felt. Britain could decide, Foreign Office, Treasury, and Admiralty willing, not to annex a territory on the simple ground that, if the economic returns from the area appeared reasonable without exerting political control in a direct manner, why take on the administrative expenses, the risk of entanglement in foreign wars, and the inevitable direct and indirect competition with European rivals that a colony induced? In later years John S. Galbraith would add an important proviso, that a situation of turbulence must not arise within the hinterland of such an area or it might lead to a grave temptation to intervene beyond the frontier to reassure stability.¹⁶ Obviously, informal empires were best exercised over relatively stable societies.

Fay's conception of the informal empire could apply best only to the latter half of the nineteenth century and certainly not well to the earlier times to which it has on occasion been applied. For one must wait until first the consuls, and then the Foreign and Colonial offices, showed a predictive capacity that fell parallel to the needs of the merchants and investors of the

informal británico en el Uruguay en el siglo XIX (Montevideo, 1975). On New Zealand, see W. B. Sutch, *The Quest for Security in New Zealand, 1840-1966* (Wellington, 1966). Also important in the context of this point is Geoffrey Blainey, *The Tyranny of Distance* (Melbourne, 1966); H. A. Innis, *The Bias of Communication* (Toronto, 1951); and George Grant, *Technology and Empire: Perspectives on North America* (Toronto, 1969).

¹⁴ Winks, "Canada," in Winks, ed., *The Historiography of the British Empire-Commonwealth* (Durham, 1966).

¹⁵ Charles R. Fay, *Imperial Economy and its Place in the Formation of Economic Doctrine, 1600-1932* (Oxford, 1934).

¹⁶ John S. Galbraith, "The 'Turbulent Frontier' as a Factor in British Expansion," *Comparative Studies in Society and History*, II (January 1960), 150-68.

City of London before the premise Fay posed could, in fact, be fully applied: that a conscious act of choice was made with respect to future economic benefits (benefits not alone to be measured in terms of purely financial gain, of course). To make such an act of choice, the bureaucracy had to have a predictive capability that it could trust; that is, it had to be organized sufficiently well, with sufficient continuity of presumed experts in area-designated offices, and with sufficient liaison between the bureaucratic elements involved to be able to argue a case based upon options which, in themselves, had to be defined upon the basis of predictions. While it could be argued—as Schumpeter, and in a different way, Hobson and Lenin classically did¹⁷—that post-1870 formal expansion was fundamentally irrational, the creation of an informal empire need not be irrational. If a country entered into a trade agreement for the purchase of x-quantity of goods of y-type, if it supplied the money necessary to help mechanize the production of y-type goods in order to assure the dependability of x-quantity of a-quality across z-years, it must be in a position to exercise at least some control over x and y so that a trading pattern, an enlarged port, a railway, or an enemy made at the price of extraterritoriality could be expected to pay or to offset dividends not just next year but over those predicted z-years. Fay's informal empire depended upon the "rise of the expert," and while we cannot date such a moment more than impressionistically, the founding in Britain in 1857 of the National Association for the Promotion of Social Science will do. Informal empire was a modern phenomenon.

Informal empire was modern in at least three other ways as well.¹⁸ If one may attempt a working definition of imperialism, and suggest that at the least it consists of the events and relationships which emerge as the result of the impact of a higher-technology society upon a lower-technology one, then an important variable in any act of imperialism, formal or informal, is the gap between the high- and the low-technology societies. In general, the historian may argue that the gap was greater in the nineteenth century than at any other time, and the impact was therefore the greater; further, that even though the British did not formally annex certain low-technology areas at first, preferring to use forms of indirect rule, or did not annex some of them at all, preferring the rubric of the protectorate or its modifications, they nonetheless genuinely incorporated such low technology areas into what most of us would recognize was a formal enough imperialism. But where the technological gap was less great, as between Britain and Argentina, informal empire was the chosen solution to the problem of influence, precisely because it was influence and not dominance that was sought.

¹⁷ See D. K. Fieldhouse, comp., *The Theory of Capitalist Imperialism* (London, 1967); and Fieldhouse, *Economics and Empire, 1830-1914* (London, 1973). The second shows a well-argued shift from the position taken in the first.

¹⁸ Here we rule out the frequently silly and usually tendentious comparisons—so popular to British imperialists themselves, especially at the turn of the century—between the Roman Empire and the British, or the use of metaphors of informal empire in relation to the world of Plotinus or the land beyond Hadrian's Wall. One should also note here that I am clearly using a functional definition of informal empire that differs from that used by Ronald Robinson. The crux of the difference rests on the question of choices which either were or, by the flow of events, were not open to the high technology nations.

Informal empire also rested upon the possibility for what may be called ricochet imperialism, itself modern. This term—the conventional Oxford construction is “colonial subimperialism”—is employed here to embrace those examples of informal control (even if later extended into formal control) which involved a colony itself acquiring preeminent power in another area, as Queensland sought over Papua from 1884 to 1906 or South Africa would do in a variety of circumstances; and it also embraces forms of control exercised by former colonies once they have become fully independent, when those forms of control may be shown to have arisen from the former colonial experience, again as with Australia in New Guinea, New Zealand in Western Samoa, or South Africa in Zululand, a Bapu-Tatswana, and Namibia. Informal empire was often based upon the intermediation of a technically independent government with people it in turn treated as colonial. One example would be Portugal, which for a time became a British quasi-dependency, the Portuguese empire showing many characteristics of an empire within an empire.¹⁹

The third element of modernity arises from big-power conflict. Lenin surmised that some nations would preserve their independence only because of competition between the powers, an early inversion of Deutsch's theme of mutual responsiveness. Such has been the case, although not invariably, in Latin America, where many scholars would argue that Britain did not intervene directly because of fear of the United States. Of course, this is a persistent theme of Canadian history as well,²⁰ since many Canadians felt that their interests were sold on the block of Anglo-American harmony, whether at the Treaty of Washington in 1871 or at the Alaskan boundary arbitration. Nonetheless, the main point is not blunted: the major powers often found an informal system of control more desirable than formal annexation precisely because of the competition between them; and informal arrangements were attractive in a new age of multiple major powers, a situation which if not politically unique to the nineteenth and early twentieth centuries, was, when coupled with technological capabilities, very nearly so.

At first historians were inclined, when confronted with the apparent contradictions inherent in several formal and informal empires, to take refuge with Walt Whitman who, in *Song of Myself*, said “Do I contradict myself?/ So I contradict myself,/ I am large, I contain multitudes.” But starting in 1953, Ronald Robinson and John Gallagher have maintained that the apparent contradictions of mid- to late-nineteenth-century imperialists were not contradictions at all. They point to the facts that Britain expanded most in the period of 1850 to 1873, the last precisely at the time that Lord Carnarvon and others in the Colonial Office were urging a cessation of expansion; that an empire that did not pay nonetheless continued to grow; and that areas of little

¹⁹ Other papers read at the 1973 AHA meeting examined other areas of informal imperialism. Joseph Tulchin spoke on “A Theoretical Framework for the Study of Informal Empire” and applied it to Argentina, while Joseph J. Malone spoke on “Informal Empire: Case Studies in the Techniques of Control—Arabia (Britain and the Pashaliq of Baghdad).” The commentators were John Cell and Peter Mellini.

²⁰ A classic example is Donald G. Creighton, *The Story of Canada* (Boston, 1960).

economic value were among the most coveted.²¹ Although strongly challenged by Oliver Macdonagh in "The Anti-Imperialism of Free Trade,"²² and while not without over-simplifications, Robinson and Gallagher have become household words among those whose house is Imperial History, and their view of informal empire is now a familiar one. Grossly oversimplified, their argument is that empire—formal and informal—grew initially upon the basis of free-trade principles (as Fay had strongly implied). Other things being equal, this should mean that the rise of protectionism could lead to a lapse in informal empires—but then, on the principle that the observer changes the phenomena observed, it is also perfectly clear that an informal empire once instituted guaranteed that all things would not again be equal.

In short, Britain was first drawn into informal imperial relationships through a syllogism: It found that it had a technology more sophisticated than any other, having entered the industrial revolution first and, although narrowly, having experienced the most important stages of industrialization more completely than others, thereby temporarily intensifying the technological gap between itself and other societies.²³ Further, this was especially so in cotton, which the world needed in its manufactured form, as did the British middle class, creating both a body of expertise with predictive abilities within Britain in relation to cotton textiles²⁴ and a demand for overseas markets. As other nations rose to industrial prominence, however, Britain found itself suffering from having gone that route, burdened with antiquated equipment, outmoded industrial practices, and entrenched methods, skills, and techniques. Not being able to meet the new challenge on direct industrial grounds, Britain turned increasingly to the worlds of finance and trade, preferring informal expansion to formal expansion, where possible, a preference it based upon its greater entrepreneurial and capital resources, especially in four areas: railways, loans, banking, and extractive staple-related industries. The

²¹ On the "Robinson and Gallagher thesis," see their work, *Africa and the Victorians: The Climax of Imperialism in the Dark Continent* (New York, 1961); "The Imperialism of Free Trade," *Economic History Review*, 2nd ser., VI (i/1953); "The Partition of Africa," *New Cambridge Modern History* (Cambridge, 1962), XI, ch. 22; and Robinson, "Non-European Foundations of European Imperialism: Sketch for a Theory of Collaboration," in Róger Owen and Bob Sutcliffe, eds., *Studies in the Theory of Imperialism* (London, 1972), 117–42. I have drawn heavily on the last essay here. On the criticisms and appraisals of Robinson and Gallagher, see in particular three, Eric Stokes, "Late Nineteenth-Century Colonial Expansion and the Attack on the Theory of Economic Imperialism: A Case of Mistaken Identity?," *The Historical Journal*, XII (ii/1969), 285–92; Stokes, "Imperialism and the Scramble for Africa: The New View," *Historical Association of Rhodesia and Nyasaland*; and the forthcoming book by William Roger Louis, *Imperialism: The Robinson and Gallagher Controversy*, which I have read in typescript. See also Eric Stokes' review essay, "Uneconomic Imperialism," *The Historical Journal*, XVIII (ii/1975), 409–16; and *The Theory of Imperialism and the European Partition of Africa*, Center of African Studies, University of Edinburgh, *Proceedings of a Seminar* (1967). For one application of the "collaborator" model, see Anil Seal, *The Emergence of Indian Nationalism* (Cambridge, 1968). Seal and Gallagher are now at work on their own volume on India.

²² Oliver Macdonagh, "The Anti-Imperialism of Free Trade," *The Economic History Review*, 2nd. ser., XIV (iii/1962).

²³ See Eric J. Hobsbawm, *Industry and Empire: An Economic History of Britain since 1750* (London, 1968), and P. L. Payne, *British Entrepreneurship in the Nineteenth Century* (London, 1974).

²⁴ See Peter Harnetty, *Imperialism and Free Trade: Lancashire and India in the Mid-Nineteenth Century* (Vancouver, [1972]).

last reinforced the staple dependency of the colonial economies.²⁵ Later would come the retreat of the 1890s into an entangling formal imperialism.

Thus one need not accept entirely or uncritically the Marxist analysis of why imperialism continued when there were a declining number of colonies to recognize that elements within the Marxist analysis apply especially well to the idea of informal empires and to their elimination, sublimation, or redefinition. For foreign investment on a large scale is of the essence to informal imperialisms, and whether one argues with Hobson that capital export arises from the existence of surplus capital, with the Marx of volume II of *Das Kapital* and of the *Grundrisse*²⁶ that the explanation lies within the falling rate of profit in an industrial society, or even with Harry Magdoff and the New Fundamentalists that the main cause of capital export rests with the very basic monopolistic structure of industry,²⁷ the conjunction between the capital element and the human factor must be recognized, and especially so when the time comes to put an end to a relationship. If, as some writers argue, foreign investment came to be financed largely out of retained earnings and locally raised funds, the historian must then study closely the nature of the local society itself and cannot retreat into the comfort of broad economic theory to explain either the rise or the fall, much less the nature and continuity, of informal imperial systems.

It seems apparent, then, that there is as wide a diversity of types of informal empires as of formal, and we must anticipate a diversity of forms of "decolonization" of the informal structures. The comparison between Uruguay and New Zealand suggests one form; the mutual responsiveness of Britain and Argentina suggests a second; the intricate nature of the Anglo-Canadian-American connection suggests a third; the relationship of expanding settler society to indigenous low-technology cultures, as in South Africa,²⁸ suggests a fourth; the nature of ricochet imperialisms suggests a fifth; the reality of strategic necessities, as seen by the conventional wisdoms of the time and under the impact of the prevailing military hardware—although not discussed here since one may ultimately subsume most strategic concerns under economic ones—suggests a sixth (Corfu, Iceland today, a corner of Abyssinia under the Treaty of Ucciali, and Crete are examples).²⁹ Nor do these variants include the less finite but no less important forms of relationships, those created by the dominance of a language or a literature, or by the declaration

²⁵ On dependency theory, see in particular James D. Cockcroft, André Gunder Frank, and Dale L. Johnson, *Dependence and Underdevelopment: Latin America's Political Economy* (New York, 1972), and Robert I. Rhodes, ed., *Imperialism and Underdevelopment: A Reader* (New York, 1970).

²⁶ David McLellan, ed. and trans., *Der Grundrisse* (New York, 1971). The question of dependency, alienation of land, and the development of "modern" market economies based on pre- or proto-capitalist indigenous societies is complex wherever it arises. For one major work in the area, see A. G. Hopkins, *An Economic History of West Africa* (New York, 1973).

²⁷ Harry Magdoff, *The Age of Imperialism* (New York, 1969). See also Arghiri Emmanuel, *Unequal Exchange: A Study of the Imperialism of Trade* (New York, 1972).

²⁸ See chapters 7 through 9 of Monica Wilson and Leonard Thompson, eds., *The Oxford History of South Africa*, I, *South Africa to 1870* (Oxford, 1969).

²⁹ Raymond Matton, *Corfu* (Athens, 1960); A. H. M. Jones and Elizabeth Monroe, *A History of Ethiopia* (Oxford, 1960); W. F. Monk, *Britain in the Western Mediterranean* (London, 1953).

of responsibility for an entire people regardless of where they go and irrespective of the annexation of the lands from whence they come (which in effect occurred with the establishment of the Western Pacific High Commission in Suva in 1875, for example).³⁰

Despite the presumed element of predictability in informal empires and the assumption of returns at least potentially more open to guarantees in practice, the informal empires did not prove to be any more tractable, predictable, or rewarding than the formal ones. D. C. M. Platt and Peter Smith³¹ have both argued that precisely because the British presence did not have the putative certainty of permanence, or even the illusion of permanence as in India, informal imperial controls were surprisingly weak. Platt asserts—though from an utterly different perspective, to the same logical conclusion as those who seek to refute the New Fundamentalists—that to understand informal empire one must understand the individual investor, financier, trader, contractor, manager, and concessionaire;³² in short, that economic theories of imperialism, and especially of the informal variety, have not given sufficient attention to private individuals as individuals rather than as Economic Men. While monopolists tended to behave in much the same way whether at home or abroad, so that “empire” becomes an umbrella word without real meaning, the railway companies operating from Sheffield behaved in the same way as the British railway monopolies in Latin America. Where a fixed, or severely limited, market existed for a product, an organization developed to exploit opportunities to restrict its production, whether in an informal empire or at home, whether on the basis of expatriate capital or domestic, whether based upon indigenous control of the betel-nut trade or on Hausa, Fulani, or Ngati Poneke leadership. The examples thus far best researched—of the foreign-dominated nitrate combinations of Chile, the international iodine syndicate, and the Borate syndicate, contrasted with the native-dominated organizations for valorization of Brazilian coffee, the promoters of cocoa production in São Thomé or Ghana, or the wine monopolists of Argentina—could, with less detail, be repeated around the globe. The Latin American case suggests that the example of cotton may be generalized upon: it was the nature of the trade or industry which basically determined its need to control a market, whether under foreign or native direction.

³⁰ Deryck Scarr, *Fragments of Empire: A History of the Western Pacific High Commission, 1877–1914* (Canberra, 1968).

³¹ D. C. M. Platt, “British Diplomacy in Latin America Since the Emancipation,” *Inter-American Economic Affairs*, XXI (Winter 1967), 21–41; Platt, *Finance, Trade and Politics in British Foreign Policy, 1815–1914* (Cambridge, 1968); Platt, *The Cinderella Service: British Consuls since 1825* (London, 1971); and Platt, “Economic Imperialism and the Businessman: Britain and Latin America before 1914,” in Owen and Sutcliffe, eds., *Studies in the Theory of Imperialism*, pp. 295–311. As Graham points out elsewhere, while Platt sees himself in opposition to Robinson and Gallagher, some of his evidence can be read in their support. See also Benjamin J. Cohen, *The Question of Imperialism: The Political Economy of Dominance and Dependence* (New York, 1973).

³² Some prefer to refer to *compradors* rather than collaborators, especially in an Asian context. See Norman G. Owen, *Comprador Colonialism: Studies on the Philippines under American Rule*, Michigan Papers on South and Southeast Asia, no. 3 (Ann Arbor, 1971); and Yen-p'ing Hao, *The Comprador in Nineteenth-Century China: Bridge between East and West* (Cambridge, Mass., 1970).

No less important were the variables which helped to determine the manner in which a people might disengage themselves from another. (These variables apply equally well, indeed quite possibly better, to the comparative study of the differing nature of white settler-native peoples relations in Canada, Australia, New Zealand, and southern Africa.)³³ The first variable is the nature of the indigenous society, in all of the multiplicity of variants societies may show. For we must take care not to be unconscious racists, or to embrace simple-minded assumptions that low-technology people are all alike. To accept a single explanation for the rise of collaborators within indigenous societies is to accept the harmful implications lurking behind the anthropology of the Lucien Lévy-Bruhl of *How Natives Think*.³⁴ Obviously each local society creates a collaborator class out of its own culture, and the nature of the collaboration that follows will be based upon quite different forms of imitation, role playing, and (to use Deutsch's phrase) interlocking ranges of transaction.³⁵

The second variable is the nature of the imperial power and its people, for each high-technology nation perceives of the role of technology differently, places different values upon specific technologies, and conditions those who go overseas in the service of each technology differentially. Within a managerial group the nature of interaction with the local culture will vary depending upon such matters as the generation, training, marital status, income, rank, expectations, and goals of those sent out. (Clearly here is one instance where the Cliometricians can come to our aid.)

The third variable is the relationship between the nature of the terrain in which the meeting of cultures takes place, the value of the land in relation to the needs of the two cultures, and the nature of the resources in terms of world demand at specific times.

And the fourth variable is the degree of commitment the imperial power wishes to make to the retention of a territory, a treaty right, a most-favored-nation clause, a railway contract, or a mining lease. Commitments change dependent upon unpredictable urgencies, as when the British sought to control Akaroa (in New Zealand) in the face of mistaken expectations about French intentions, or upon predictable contingencies, as when the British scuttled the Anglo-Japanese alliance in the face of Canadian pressure and American concern. Clearly Britain was often unwilling to commit her strength unreservedly to informal empire.

Having examined this one variable, for example, Platt concludes that limitations on the discretion and authority of foreign entrepreneurs were great, and that because of fear of the United States Britain intervened very seldom in Latin America. His judgment, not yet sustained in all particulars, helps explain the process of decolonization, for as he writes, the "only real safeguard" for the foreign entrepreneur was his "indispensability, and when

³³ The following argument is touched upon in Winks, *The Myth of the American Frontier: Its Relevance to America, Canada and Australia* (Leicester, 1971), and I hope to develop the argument further in an extended essay.

³⁴ Lucien Lévy-Bruhl, *How Natives Think* (London, 1926). First published in French in Paris in 1910.

³⁵ Karl Deutsch, *Political Community at the International Level* (Garden City, N.Y., 1954).

local competitors and substitutes developed he was soon displaced.³⁶ Because there are powerful and effective informal empires today, because thinking Americans are aware of massive pressures by their government and their businessmen overseas, perhaps many apply to the years prior to World War I conditions truly valid only for the more recent period. While any industrial government today presumably assumes a responsibility to promote financial and commercial interests overseas, until the D'Abernon Agreement with Argentina in 1929 the British government insisted that its responsibility was limited to supplying commercial information. Nor in the nineteenth century did Britain seek the vital materials wanted so desperately by industrial powers today, for she had her own fuel, which was coal. Further, Britain was in no position, given the realities of the world, to achieve hegemony, or invariably to refuse disengagement when it was demanded of her.

As we know, where a formal unit of her formal empire was involved, certain assumptions tended to guide British actions. As suggested, Britons sought to export a Westminster model of Parliament and to establish an independent judiciary, a neutral civil service, and (often) the local equivalent of Sandhurst. They believed that "national unity" must be encouraged before decolonization could proceed.³⁷ They reasoned that where small colonies lacked the potential for a "viable economy," they might be tidied up by being pressed into some form of convenient (to the British) federation. And they held that institutions were the best indices of progress. Lacking the power, desire, or purpose to apply measures to achieve these goals within an informal situation, recognizable measures were not easily applied, so that the role of the collaborator groups was more difficult, tenuous, and exploitative than in annexed territories simply because such groups were not to be used as the agents (or tools, if one prefers) of a set of specifically mandated changes.³⁸

Resistance to informal empire was therefore of a different nature than within the areas of the world map painted red. To be sure, the same typology could be observed, of primary resistance, secondary resistance (as the phrases have it), of quasi-millenarian movements, of hostile welfare associations, trade unions, and churchly bodies, and of sophisticated political opposition. Within informal empires, such forms of resistance could never be clear cut. As generations competed for elite leadership—a competition that often turned upon ever larger access to education by each succeeding generation, either within schools and training schemes established by the informal imperial power, or within traditional forms that clearly created no adequate means of dealing with the challenges brought by the interlopers' new technology—those who wished to oppose had first to cooperate. The manipulation of prices,

³⁶ Platt, in Owen and Sutcliffe, eds., *Theory of Imperialism*, pp. 295–311. On Chile, see Harold Blakemore, *British Nitrates and Chilean Politics, 1886–1896: Balmaceda and North* (London, 1974).

³⁷ Bernard Semmel, *Imperialism and Social Reform* (Cambridge, Mass., 1960).

³⁸ The British helped create their own Opposition in Sierra Leone precisely because that colony was not producing an effective Opposition, contrary to the needs of colonial theory. But the British did not need to create oppositions in Central America or Greece or Argentina. Their battles were fought for them in such places.

the transformation of agricultural practices, the creation of marketing agreements, and interference with systems of land tenure, all of which could occur in informal as well as formal situations, drew those who formerly were outside the influence of the local bureaucracy into its circle. One result was that nationalist or antiforeign movements could run downwards through a series of competing elite groups, each able to command a wider circle of support, and at the same time could also be the product of a populist ground swell, catching the middleman truly in the middle. In a formal empire, any great power could take steps to rectify such a situation in its favor; where an imperial power was dealing with a putatively independent country, it could do little save ride out the storm, often unsuccessfully.³⁹

Thus the role of the collaborator (or *comprador*) was critically important within the informal empire. (Collaborator here is meant to have none of the pejorative meaning given to it by its association with *quisling* in World War II, for those who employ the term intend it as an unemotive, descriptive word for a sociological phenomenon.) Where informal empires arose, the relationship between the two elements was initially one stemming from conditions of relative equality. The local leaders, whether of business or politics, wished to have the benefits—as they defined them—of technology and trade. Having grasped the end of the stick, they sought to maintain their monopoly over communication with the power that could provide these benefits. As the European power explored more carefully the benefits that might flow to it from working through such collaborators, it usually found that the initial group was inefficient or otherwise not the most appropriate, leading to an uneconomic relationship which could be little improved upon.

In any event, British businessmen were well aware that to have no options was to have no future, and that options were eliminated in direct proportion to the degree to which they depended upon a single collaborating group. The imperial nation logically enough wished to exert increased authority over the initial collaborating element while trying to create new elements as well; this could be done only with the greatest delicacy, however, for the very stability of the society could rest on the continued preeminence of the initial collaborative group, and above all the businessman wanted stability. His choice often was one of inaction then. Still, the first group of collaborating merchants would protest if they found their interests declining—this happened too in formal colonies, of course, for what else was the Montreal annexation manifesto of 1849?—and would fear that the British would be only too happy to shift their trade, contracts, and loans to a younger, better educated, more technologically alert group of collaborators waiting in the wings. Sometimes the fear was realized. Having seen one group shunted aside, other elites might

³⁹ Eric Stokes, "Traditional Resistance Movements and Afro-Asian Nationalism: Towards a General Theory," unpubl. paper presented at the University of London's Institute of Commonwealth Studies Postgraduate Seminar on October 29, 1970; and Stokes, "Traditional Resistance Movements and Afro-Asian Nationalism: The Context of the 1857 Mutiny Rebellion in India," *Past and Present*, no. 48 (August 1970): 100-18.

collaborate beyond the point of no return rather than take the risk of protesting too much, since they almost always had no support from below that could be counted upon, the populist form of protest to an imperial presence being based upon the tenets of class warfare. As Eric Stokes has noted of, among other areas, unannexed but "protected" parts of the Indian Empire, "the processes of modernity will set up a constant tendency to displace traditional by more modern elites."⁴⁰ Thus arose the recurrent crises of displacement.

In the long run, however, the British generally were not successful in informal situations—as they often were successful in formal ones—in keeping a single group from taking control of the channels of communication. For in a formal colony, those channels were kept open for a great diversity of reasons, while in an informal relationship the channels were maintained largely, although not exclusively, for reasons of trade and finance. This being so, and trade and finance being subtle and shifting matters from which one could not divert young turks by sending them off after some constitutional bauble, one had to stick with the collaborator groups one had, cutting the options down severely and reinforcing a tendency toward inaction. While the presence of an informal empire could draw the potentially dissatisfied, especially those unable to live from landed incomes, into increasingly sophisticated forms of government employment, the imperial nation had no control over the middle and lower echelons of bureaucracy within the local society. Petty bureaucrats in India had reason to be loyal to the system just as poor whites in the pre-Civil War American South were the staunchest supporters of slavery, although they owned no slaves themselves: they did not want anyone to kick that ladder out of place by which they defined their present status and future goals. But petty bureaucrats in Latin America or Eastern Asia had no reason to be loyal to the British, and it was from their ranks that opposition to the entrenched collaborators at the top came. They were likely to feel more quickly than others the effects of dislocations within the world's market structure, and it seems clear that the price inflation of the decade prior to World War I helped focus discontent at the mid-level against the older forms of informal empire. It was then that British engineers in Thailand at work on the railroad, in Constantinople at work on the sewers, and in Argentina at work on the meat-packing plants began to realize that they soon would no longer be needed or wanted, for local counterparts would take over.

Which brings us squarely to that collaborator around whom we have been skirting, and back to the brilliant insights of Ronald Robinson.⁴¹ An unusual aspect of Robinson's work, perhaps because of the ambivalent nature of imperial contacts with Egypt and the Sudan—the areas to which he and his collaborator John Gallagher first applied themselves—is that his model applies no less well to informal than to formal situations. To be sure, detailed applications by his model have, to date, been limited to preindustrial societies, and certain adjustments are called for within the model. Nonetheless,

⁴⁰ *Ibid.*, 103.

⁴¹ See note 21.

working along somewhat parallel lines, as we have seen, several scholars have reached similar conclusions for Latin America and the Far East, without specific reference to or perhaps even full awareness of Robinson's main points.⁴² All have a basic point in common, however: to understand imperialism, whether expressed in formal or informal modes, one must understand the mediating groups in the society being encroached upon, in order to examine how such groups interweave their needs with those of the expansive power, until—as particularly in the case of informal empire—a type of equilibrium between those needs temporarily is achieved. It follows that if we are to understand decolonization in this context, we must put two questions: how does the woven fabric fray? and may the fraying—or, to abandon the metaphor, the *perception* of decolonization—not simply be a consequence of the fact that an equilibrium had never been achieved, the mediating groups having failed rather than succeeded? For either question one must study individuals and groups within the local society, treating them not as economic integers in some broad theory but rather with all the proliferating variety common to a Burkean view of life.

As Robinson argues, it is clear that where the local society is based upon a large subsistence sector, and has only a small export-import sector, real or potential, to its economy, informal empire is not a rational arrangement by which an equilibrium of needs is achieved, and the imperial power may be tempted to intervene directly in order to exercise sufficient power to reshape the balance between the sectors. But intervention need not mean annexation, as it did not in the Argentine or in Uruguay. In both cases the problem for the British was that—partially because of inadequate political unification (for which the British devoutly wished in Central America)—no one group in either nation could expand the import-export sector of the economy rapidly enough to meet Britain's needs. (So much for the easy notion of divide and rule.) Later, however, as both nations moved to greater unification, stability, and sophistication, the *porteños* were able to overbalance the backlands. As a local economy diversifies into local capital formation, its commercial dependence upon the imperial power lessens, unless that capital is channelled into noncompetitive areas or becomes subsidiary to that of the imperial power; thus while informal empire may be built upon simple economic needs, it endures only through the maintenance of a delicate balance of forces on both sides, a balance the imperial power will be tempted to manipulate through the local political system.⁴³ This may produce what another generation will call Uncle Toms, or what some will see as the "bought politician"—who will seem inimicable to his own nation's deeper needs as defined by the rising young turks. For the young turks will always rise, and knowing this, the

⁴² See also the work of Frank J. Moreno, John H. Rowe, and, most particularly, André Gunder Frank, *Capitalism and Underdevelopment in Latin America: Historical Studies of Chile and Brazil* (rev. and enl. ed., New York, 1969).

⁴³ I am indebted to four graduate students at Yale, Gilbert Joseph, David Holm, Florencia Mallon, and Steven Rosenthal, for conversations on Mexico, Thailand, Chile, and Turkey respectively.

imperial power will stand ready to disengage when the game is no longer worth the candle, since matters of national prestige do not, and matters of preemptive annexation seldom need arise.⁴⁴

Put differently, the equilibrium fails when the imperial partner seeks too large a share of profits for the internal partner to justify to those it must keep in domestic equilibrium to itself, or if the imperial partner demands publicly visible, and therefore politically unacceptable, forms of gratification. When this occurs, the terms of collaboration are no longer mutually acceptable, for one side has transgressed against the need of the other to be seen capable of acting independently. This being so, the internal partner can no longer hope to maintain political control at home and one of two things will happen: it will cease collaboration in order to maintain that control in the face of the young turks, or it will fail to cease in time and be overthrown by a different group. Either way, the imperial power must adjust its role, and either way the equivalent of a form of decolonization will occur, even if only temporarily. Since much of this change will not be visible to the casual observer, however, will not be demarked by flag lowerings and raisings, will not involve the dismissal of a lieutenant-governor or, perhaps, even the abrogation of a trade treaty, disengagement will be as subtle and difficult to analyse as was the original acquisition of power.

One additional condition remains to be taken into consideration: the prevailing wisdom on the world balance of power. By this we do not mean the actual balance of power, obviously, but rather what strategists had to say about it in relation to perceived strategic needs, predominant weaponry and technology, and countervailing tensions between other powers. Hence the importance, for example, of a Halford John Mackinder.⁴⁵ (We have already seen the necessity of understanding Britain's disengagement from Japan in relation to Canada and the United States, and we have noted D.C.M. Platt's strictures on the image of an all-powerful and insidiously pervasive British presence in Latin America.) To be sure, the British worked to abolish or break into existing monopolies, to effect by persuasion, compromise, and trade a lowering of tariff barriers, and after the 1860s to modernize the fiscal, legal, and political systems of societies which wished British loans.⁴⁶ They also wished to use these "modernized" systems to assure that the collaborators involved in the export-import sector of the economy, which by definition was regarded as the more modern or progressive, would exercise political control. Yet what they wished was seldom achieved for long, and Britain sought disengagement.⁴⁷

⁴⁴ One hopes that the work of Ronald Wraith and Edgar Simpkins, in *Corruption in Developing Countries* (New York, 1963), and Onofre D. Corpuz, *The Philippines* (Englewood Cliffs, N.J., 1965), has led some scholars to be less glib about the notion of corruption as measured by Gladstonian liberal principles.

⁴⁵ See especially Halford John Mackinder, *Democratic Ideals and Reality: A Study in the Politics of Reconstruction* (London, 1919).

⁴⁶ For early examples of this view see William Woodruff, *Impact of Western Man: A Study of Europe's Role in the World Economy, 1750-1960* (New York, 1967); and I. Robert Sinai, *The Challenge of Modernization: The West's Impact on the Non-Western World* (New York, 1964).

⁴⁷ See D. A. Low, *Lion Rampant: Essays in the Study of British Imperialism* (London, 1973).

To disengage from a formal imperial responsibility would prove difficult, though not invariably so, for since there were specific powers to be handed back, specific stages to be passed, and a clear, if perhaps too misleadingly political, final goal in mind, the imperial power knew where it stood. When the ties were informal, left to the realities of the world market, or to the whims of democratic Parliaments which might or might not choose, for any of a number of reasons, to staunch the flow of immigrants, or skilled artisans, or capital, or rails to another land, under the demands of free trade or fair trade or moral boycott or some other compulsion of the moment, then those ties were far more difficult to disentangle. It is a Mexican proverb, oddly enough, that says that to divorce one's wife is simple; to divorce one's mistress impossible. Perhaps the final irony of informal empire was that precisely because it was informal, it was the most tenacious of all. For what else could follow in the wake of withdrawal under such circumstances, there being no new flag of independence to run up the pole, what else except internal struggle between shifting, bereft collaborators seeking new positions on the rungs of an endless ladder?

Reviews of Books

GENERAL

JACQUES BARZUN. *Clio and the Doctors: Psycho-history, Quanto-history, & History*. Chicago: University of Chicago Press. 1974. Pp. xi, 173. \$7.95.

For more than twenty centuries, historians have been laboring to satisfy the human craving for a comprehensive and intelligibly written past. Their task has not always been easy, for man is distressingly complex, if not perverse, events and forces conflict, plans go awry with unintended results, and records have a habit of being either appallingly sparse or overwhelmingly abundant. Far too often, historical events are the result of conflicting forces which defy repetition or measured ranking, and their impact is subject to continuous debate. As a result, historians have become "scientific" only to the extent that they subordinate their prejudices. Their much-touted "method" consists of little more than thorough research, informed judgment, and a common-sense weighing of the facts. Yet, it is precisely this indeterminacy which gives history its enduring charm, for its purpose is to cultivate the mind, not to convey facts.

Recently, this traditional view of the historian's craft has fallen under heavy attack from dissenters bent on converting history into a behavioral science. The assault is not unpredictable in an age plagued by problems which demand immediate solution, and for that matter, it is not really new. The appeal to psychology and other disciplines dates from Homer and Plutarch, and the modern effort to lure history into the Procrustean bed of analytical models finds its roots in phrenological determinism. Barzun's objection to these "doctors," the psychologists, sociologists, and quantomaniacs who are "attempting to rescue Clio from pitiable maidenhood by artificial insemination," is quite simply that they are anti-historical and represent pseudosciences that confuse vocabulary with method. History is, after all, the study of change produced by the interaction of people and forces over time, and the study of a single factor at the expense of all others distorts more than it enlightens. It is all very well to argue

that Cleopatra was fondling a phallic symbol when she clasped an asp to her bosom, but it tells us nothing and reduces a great historical tragedy to a tasteless act of perversion. Even this might be acceptable if the behaviorists provided a unified theory instead of a sea of conflicting interpretations, but how are we to choose between the findings of an intrepid conclave of the AHA which traced German fascism to Hitler's undescended left testicle, and a recent study which explains the same phenomenon as the by-product of an encephalitis epidemic. The answer, of course, is that one, and probably both, will be consigned to well-deserved oblivion. The most telling indictment, however, is that behavioral models assign man the role of helpless automaton in the grip of overwhelming forces which dictate his past, present, and future, a condition which reduces human will and history to a null entity.

Barzun's defense of his craft is the charming, witty, and perceptive work of a mature and urbane scholar. It says what needs to be said at a critical stage in the development of historical thought and method. While it will undoubtedly draw heavy fire from the advocates of the "New History," it will stand as a classic of its kind when their work is being cited as hackneyed and dated.

ROBERT J. PARKS
University of Wyoming

ERIC VOEGELIN. *Order and History*. Volume 4, *The Ecumenic Age*. Baton Rouge: Louisiana State University Press. 1975. Pp. xvii, 340. \$15.00.

The appearance, eighteen years after volumes two and three, of the fourth volume of Eric Voegelin's *Order and History* is noteworthy in itself. But the fact that in the interval the originally projected pattern of the work has been dramatically altered (or "broken," as the author puts it) heightens the significance of the event. The result is a theoretically more coherent, if much more complex, philosophy of history than Voegelin offered in the earlier books. *The Ecumenic Age* is both a singularly important and exceptionally difficult book.

In the previous volume Voegelin adumbrated a

plan for treating the symbolization of order in concrete societies as a linear development through time, but always with a clear recognition that history is open-ended. His later studies of the empires of the Ancient Near East and other civilizations led him to conclude that the differentiation of the compact cosmological interpretations of man's existence in history through revelation in Israel, philosophy in Ancient Greece, and the Christian experience of salvation are not unique in their mythopoetic expressions of "man's participation in the flux of divine presence that has eschatological direction" (p. 6).

The very organization of the fourth volume reinforces Voegelin's shifting perspective, for it consists of a series of special studies which range over parallels in history without respect to sequence. The constancy of man's experience and the symbolic means used for interpreting that experience engage Voegelin's imagination and bind the disparate studies into a unified theory. The common experience out of which the symbolic interpretations emerge is the consciousness of living in a state of "*Metaxy*" (Plato's term), which is the tension between the finiteness of the world and the intimations of divine presence partially open to man.

The chapter from which the title of this volume is taken concentrates on the period from the rise of the Persian Empire to the fall of Rome—the "axis-time" during which the rise of the great religions of the world (simultaneous spiritual "outbursts") coincided with the development of multi-ethnic geographic empires which drew on the religions for their claims to universality in both the physical and historical senses. The other special studies in the fourth volume, including the origins of historical speculation, St. Paul's understanding of the resurrection, the Chinese ecumene, the concept of universal humanity, etc., all provide empirical evidence to enlarge our understanding of the central thesis of man's quest for the ground of being and the relation of that quest to the problem of order in the history of pragmatic societies.

The Ecumenic Age confirms the view of those who have long held that Voegelin is the only contemporary philosopher of history whose mastery of historical sources equals that of the late Arnold Toynbee, and whose philosophical acumen by far exceeds Toynbee's.

WILLIAM C. HAVARD
Virginia Polytechnic Institute
and State University

M. I. FINLEY. *Democracy Ancient and Modern*. (Mason Welch Gross Lectureship Series.) New Brunswick: Rutgers University Press. 1973. Pp. x, 118. \$6.00.

This is the text of a series of three lectures delivered in New Brunswick, New Jersey in 1972. It has the advantages and disadvantages of its origin: unrelated organization and different kinds of presentations, in which emphasis is not placed upon the careful and systematic sifting of evidence in an effort to reach conclusions but upon simple statements of belief, pleasantly spaced by copious quotations. Nevertheless, there is much here to think about. Shifting back and forth between the ancient and the modern world, the author develops a number of themes. Of these the most interesting is the apparent paradox between political leadership and participatory democracy in Athens. Although this condition would normally be expected to lead to a confrontation ending in stagnation or anarchy, the author shows us how, on the contrary, peaceful coexistence prevailed for a long time. He does not really face up, however, to the problem that Greek democracies failed to produce, for good or ill, a Greek nation or a united country. He spends considerable time defending the Peloponnesian War, at least in the sense that it was undertaken with the consent of the Athenian people and that it benefitted the poor rather than the rich. In these latter points he fails to get the question of class struggle onto center stage, although this reviewer senses it waiting in the wings. Its entry would have been interesting, for no one has ever proved the existence of class struggle in fifth-century Athens. In the third and final section on "Socrates and After" there is a more unified and better-organized treatment of a very important aspect of Greek life, freedom of speech and civil liberty. All in all, the author apparently favors a return of democracy to something like the Athenian model, at least in its spirit and its approach to government.

ROBERT K. SHERK
State University of New York,
Buffalo

DAVID KNIGHT. *Sources for the History of Science, 1660-1914*. (The Sources of History: Studies in the Uses of Historical Evidence.) Ithaca: Cornell University Press. 1975. Pp. 223. \$11.00.

Here is a useful and readable introduction to materials for research in the history of science: journals, books, manuscripts, and physical objects. The emphasis is on chemistry, physics, and natural history in nineteenth-century Britain, but students of other periods, countries, and disciplines will find valuable information. The numerous examples of pitfalls might have been supplemented by more specific examples of how to avoid them. Moreover, the index sets a bad example by not covering the footnotes; this is not a "handbook" in

which one can quickly look up detailed information. But it can be recommended to graduate students and others who want to know how historical knowledge about science is gleaned.

The paperback edition is unfortunately available only in the United Kingdom.

STEPHEN G. BRUSH
University of Maryland,
College Park

PHILIP S. BAGWELL. *The Transport Revolution from 1770*. New York: Barnes and Noble. 1974. Pp. 460. \$17.50.

Comparisons with T. C. Barker and C. I. Savage, "An Economic History of Transport in Britain" (third revised edition, 1974) and H. J. Dyos and D. H. Aldcroft, "British Transport" (1969) are both inevitable and necessary. All three works are comprehensive narratives of transport developments within Britain from the eighteenth to the mid-twentieth century, although Dyos and Aldcroft terminate their study at the commencement of the Second World War. Bagwell's analysis is the most expansive of the three in that a review of coastal shipping is added to thoughtful, detailed, and lively commentaries upon the relative roles of inland waterways, railways, and road transport, with internal air transport being briefly touched upon in the final chapters.

Developments subsequent to the Second World War are fully discussed with particular emphasis upon the influence of transportation policies of the various governments. The author provides a pithy appraisal of a somewhat restricted bibliography, but, more important, provides extensive footnotes to each chapter, with ample reference to numerous primary sources, particularly government papers. Throughout the book, much use is made of statistical material, presented clearly by a plenitude of tables, with references to the less familiar twentieth century rather than the well-worked nineteenth century.

By providing such a comprehensive review of the existing research, Bagwell is at once the master and prisoner of his sources. The weaknesses inherent in his study are those of the discipline itself—it is clear that there are still areas where research must be undertaken. Some facets are not considered in the same degree of detail as are others, although it might be argued that they are of equal significance. The author has done transport history a great service, however, by pinpointing, albeit tacitly, those areas where future research should occur. His eminently readable style and competent handling of a necessarily factual account have provided transport historians with a

particularly valuable addition to this flourishing branch of economic history.

JAMES R. HEPPLER
University of Hull

GEORGE ROSEN. *From Medical Police to Social Medicine: Essays on the History of Health Care*. New York: Science History Publications. 1974. Pp. 327.

This book brings together a group of thirteen essays on the relations of medicine and society published in eight different journals between 1944 and 1973. Two general themes run through the collection. The first is that in any society social, economic, and political conditions and theories influence or even determine the type and prevalence of disease as well as the society's policies, plans, and proposals for health care. Essays examine this theme historically with examples of mental illness and pellagra, and they relate it to the development of proposals for medical care and public health programs during times of different views of political economy. Examples come from Germany, France, England, and the United States. The second theme is that historical examination and analysis are necessary for understanding modern institutions of medicine and the way medical and health care is provided. It supports Rosen's idea that medicine is a social as well as a biological science. As a whole, the essays provide an excellent study of many aspects of health care.

The essays were originally addressed primarily to audiences of health professionals, who presumably are less committed than historians by profession, temperament, or interest to the value of history and less inclined to think in social terms. For them, the thematic view may be more original and significant. For social historians, the essays will provide much good meat and sound historical scholarship on the relations of medicine and society from one whose medical knowledge provides a background that many social historians obviously lack.

Inevitably, there is a certain amount of duplication among the essays, making readers hope that Rosen will in time prepare the comprehensive history of medical care for which he is eminently qualified as physician, sociologist, public health expert, and historian. Meanwhile, it is useful to have these related essays in convenient form.

JOHN B. BLAKE
National Library of Medicine

THOMAS F. GLICK, editor. *The Comparative Reception of Darwinism*. (The Dan Danciger Publication Series.) Austin: University of Texas Press. 1974. Pp. ix, 505. \$15.00.

A collection of seventeen essays by fifteen authors cannot be expected to maintain a standard level of treatment, even when they are the result of a conference sponsored by the American Council of Learned Societies and the University of Texas. With no uniformity of procedure, with no average level of coverage, and without even a standardized definition of what Darwinism is, the book is of limited use to the general historian who wants one worthwhile work on the subject—or even one complete treatment of the country of his choice. The general historian, for example, might turn to M. J. S. Hodge's bibliographical essay on England, which mentions seven publications as, respectively, "unmatched," "remarkable," "masterly," "exemplary," "lucid and penetrating," "incisive," and "important." Hodge mentions eight others with less enthusiasm: "careful analysis," "detailed discussion," "still useful," "also valuable," "enlarge our knowledge" (two articles), "some detail," and "standard." He discusses others at more length; but how is the general historian to guess that I, for example, would argue seriously with most of the publications on the first list and would rely with confidence on most of those on the second list? I am not saying that I am right and Hodge is wrong, but this is a field full of snares for the uninitiated.

The volume is idiosyncratic, however, in the best sense. It is a fascinating display of the various procedures that are used in "reception of" studies and of the puzzlements caused when a major scientific development becomes involved with older, perhaps more powerful, and sometimes antagonistic movements of belief existing in a society—such as evolution, progress, social amelioration. It is easy to condemn modern authors for discussing these other things instead of the specific reactions to Darwin's science, but what is one to do if these things *were* what was discussed most at the time? My feeling is that this book is well suited as the "text" for an advanced seminar on method. By text I mean that the students would tear some of the essays to shreds and press hard even on the most substantial ones. There is not yet a satisfactory methodology for handling this kind of subject. This book could be the stimulus—or irritant—for developing one.

Under these circumstances, a reviewer can only indicate what he likes and not complain too much about what is not done. My choice for first place is William Montgomery's essay on Germany. His treatment of specific scientific reasons why some biologists opposed Darwin is the kind of work the field needs. I admire Robert Stebbins' demonstration of why Frenchmen were content with their own *transformisme* and unable to see that Darwin was either new or important. Frederick Burk-

hardt's account of papers published by the major learned societies of England and Scotland is solid and satisfying, a job that needed to be done. Hodge discusses the scientific-intellectual scene in England fairly widely, while warning that such a description does not explain in detail the reception of Darwinism.

Darwinism in the United States is too big and too confused a subject to be covered in one paper, although Edward Pfeifer tries. His account of the development of neo-Lamarckism is well done. Michele Aldrich, in her bibliographical essay on Darwinism in the United States, summarizes valiantly, draws attention to "carelessness in the American literature about whether a response was triggered by a writing of Darwin . . . or by another thinker" (p. 207), and concludes—if I interpret her correctly—that it is time for someone to clean up the mess.

Russia seems to me to suffer, even with two essays, Alexander Vucinich's "Biological Sciences" and James Allen Rogers' "Social Sciences." Both authors in my opinion have done their best writing elsewhere; for example, Rogers' article in *Isis* in 1974 seems to me simply more interesting than the Russian articles in this volume. The same is true for David Hull in "Darwinism and Historiography"; he shows his subject to better advantage in the introduction to his own anthology, *Darwin and His Critics* (1973).

Ilse Bulhof, on the Netherlands, has a specific thesis. "From a sociological point of view, Darwinism was received by and on behalf of the liberal bourgeoisie" (p. 289). Plausible, but is it demonstrated? How should it be demonstrated? Najm Bezirgan, in a short essay on the Arab countries, is sure that it was "the philosophy of evolution and progress" that was being introduced; but, compared to other Western ideas, such as those of Comte, Mill, and Spencer, Darwin's own ideas were not very important. Harry Paul has an international essay, "Varieties of Catholic Reaction"; it is only in a few of his excellent pages that Italy is discussed in the volume.

Anthony Leeds argues at length that virtually all forms of nineteenth-century evolutionism that affected or still affect the social sciences were non-Darwinian, indeed, were anti-Darwin. He quite approves of this: "The ordering of life characteristic of the human species means that the Darwinian model will not work for sociocultural phenomena" (p. 475). If Leeds is correct as to what modern social scientists do believe, then he is placing them as still in the intellectual climate of the British 1840s. Then, too, Christian and Romantic intellectuals thought that both apparent goal-directed activity and nonutilitarian behavior norms ensured that mankind could never be dealt with in

the same fashion as the rest of nature. Darwin does have answers for these problems, but they are often simply ignored. For example, they do not occur in Leeds' account of what "the Darwinian model" is. This reminds one of a major gap in the volume: there is no satisfactory account of what Darwin's set of presuppositions and theories actually was.

If I rated Montgomery as first because I think his procedure is needed, I have saved my two favorites in content for final mention. Thomas Glick's essay on Spain is so full of a number of things about the Spanish intellectual scene that it is pedantic to quibble over trifles. When he says that the "diffusion of evolutionary ideas in the provinces was, both for depth and for rapidity of permeation, remarkable" (p. 324), one can ask what his standard of comparison is. But this is the kind of thing that can be worked out later. Darwinism and Spain are now on the map, so to speak. Even more startling is Roberto Moreno's essay. One might have thought that there was nothing much worth saying about Darwinism in Mexico. But Moreno has provided an elegant analysis, with such interesting variants as that Darwin was known more from *The Descent of Man* than from the *Origin of Species*, that Mexicans relied principally on French sources, that anthropology was the science most affected by Darwin's influence, etc. There is more to be known about the history of science everywhere than one might suspect, if one will only look for it as Moreno has done.

W. FAYE CANNON
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WILHELM BACKHAUS. *Marx, Engels und die Sklaverei: Zur Ökonomischen Problematik der Unfreiheit.* (Geschichte und Gesellschaft.) Düsseldorf: Pädagogischer Verlag Schwann. 1975. Pp. 267.

Because Wilhelm Backhaus' first concern is with Marx's analysis of slavery in the ancient world, he opens with a critical survey of early Soviet attempts to adjust the subject to the Procrustean requirements of the Marxian concept of class conflict and the operations of the dialectic.

As Engels stated retrospectively in 1886, Marx "recognized the historical inevitability, therefore the justification of the ancient slave owner, medieval feudal lords," etc., as "levers of human development for a limited period of history" and therewith the "temporary historical justification of exploitation, the appropriation of the products of labor." But Marx "proved" that such justification ceased when their continuation reached a point where it hampered "social" development and created "ever sharper conflicts." The Marxian

"proof" that this was the case in the Greek and Roman world, however, was extremely sketchy. Therefore, almost inevitably, the greater part of Backhaus' text is confined to the slavery question in the United States—the Civil War and various interpretations covering different aspects of the slave economy. The conflict in America was of immediate interest to Marx and Engels, coinciding as it did with serf emancipation in Russia and signs of unrest elsewhere in Europe, as portents of the approach of wider "social" crises that their theories envisioned and that they expected to experience directly.

In dealing with the American slave question, Backhaus offers a balanced account of the varying interpretations of the problem. He includes statistics on the slave population and its increase, the operations of the slave economy, slave labor on the land and in nonagricultural trades, competition between slave and free labor, and the contempt for physical work that the existence of slavery avowedly induced. Backhaus notes that Marx relied mainly on J. E. Cairnes' *Slave Power* (London, 1862) as a source of information, though he tended to ignore data that conflicted with his basic assumptions.

OSCAR J. HAMMEN
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V. G. KIERNAN. *Marxism and Imperialism.* New York: St. Martin's Press. 1975. Pp. xi, 260. \$18.95.

This excellent collection of essays is ideally suited for student use while informing the scholar as well. The author, V. G. Kiernan of the University of Edinburgh, though a Marxist, is no ideologue. He evaluates classical Marxist theory as it relates to the realities of imperialism in prose that is calm, informative, and often persuasive, even to those who start from a different perspective. Of the seven essays which comprise the book, six have appeared elsewhere—in *The Socialist Register* and the *New Left Review*—and their original venue may account for some of the reasonableness, in that they act as a powerful check to knee-jerk socialism. No other volume of which I am aware presents the basic Marxist interpretation in a way that all may understand.

The first essay, "The Marxist Theory of Imperialism and Its Historical Formation," is a substantial contribution to the literature, being lengthy (68 pages), carefully argued, and solidly documented. Another essay, "Imperialism, American and European: Some Historical Contrasts," is a sophisticated exercise in comparative analysis without sinking into a morass of model building or ascending to the heights of condemnation. It is not

unfriendly to the United States, and its concluding sentences represent the quality of the book and the direction of its commitments: "Peace and fraternity, America's early watchwords, have never lost all their virtue. If they do, if there is to be an ultimate failure of democracy in its first homeland, it will be a misfortune for the world only less great than an ultimate failure of the first homeland of socialism" (p. 131).

The final three essays, on the Labour Party and India, while somewhat less substantial than the rest of the book, also reach an equally interesting and perhaps more acute conclusion: "With Bertie Wooster to think was to act; with the Labour party very often to act was to think. I think, therefore I am" (p. 254). P. G. Wodehouse might not have been used to this company, or indeed welcomed it, but it is the measure of the book that it understands the human, and the humorous, dimensions of that bogey word, "imperialism."

ROBIN W. WINKS
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A. E. MUSSON. *Trade Union and Social History*. London: Frank Cass; distrib. by International Scholarly Book Services, Portland, Ore. 1974. Pp. ix, 211. \$15.00.

The title of this book is somewhat misleading. It concerns primarily the history of organized labor and is only remotely related to general social history. One can conclude from these studies that it is difficult, if not impossible, to extract social history from the documents of trade unionism. All but one of these essays have been previously published as magazine articles or as portions of the author's published books, with the majority having appeared in print during the 1950s. Four of the nine essays pertain to the history of trade unionism in the early nineteenth century, which culminated in the formation of the Trades Union Congress in 1868. Only Chapter IV, which is a quick survey of modern trends, touches on trade unionism in the twentieth century. The author's detailed studies of the craft unions are designed to refute the ideological historians who insist on the solidarity of the working classes and the revolutionary character of the labor movement. The author contends that many trade unions are conservative and remain deeply committed to the traditions of their separate craft unions. In addition to the history of trade unionism, the book has four chapters on such diverse subjects as employers' associations, freedom of the press, the ideology of cooperation, and the early factory system. But these fragments of information contribute little to an understanding of

complex subjects that have been more thoroughly treated elsewhere.

R. G. COWHERD
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ROGER H. STUEWER. *The Compton Effect: Turning Point in Physics*. New York: Science History Publications. 1975. Pp. xii, 367. \$25.00.

The Compton Effect was discovered in 1922 in the following way. By 1920 physicists based any acceptable theory of the nature of matter on the atomic hypothesis. Atoms were no longer unanalyzable entities; they contained two hypothetical entities: protons and electrons. At the same time fierce debate raged around the nature of electromagnetic radiation. Physics communities no longer treated such radiation solely as a wave phenomenon, yet one of the parameters used to characterize such radiation was the wavelength number. Physicists realized that when electromagnetic radiation interacted with matter rich in electrons, the emerging radiation not only suffered a change in direction, but also underwent a change in wavelength. Generalizing from this phenomenon, A. H. Compton found it convenient, indeed, he found it compelling, to treat the interaction as one might treat colliding billiard balls. Thus came the name Compton Effect. Compton's work was one of the milestones along the road to quantum mechanics. For his efforts, Compton shared with C. T. R. Wilson the 1927 Nobel Prize in physics.

Stuewer recounts the history of Compton's discovery and shows why it was a crucial aspect of the history of physics. Standard treatments of the development of this part of twentieth-century physics suggest that Compton was simply playing out the consequences of Einstein's theory of the quantum nature of radiation, first proposed in 1905. As with most of Einstein's ideas, almost no one at the time had understood what he was talking about. Most colleagues considered him either incompetent or mad. When Einstein's theory received strong experimental confirmation nine years later, Milikan, the man who did the experiment, was surprised and even dismayed at the outcome. Subsequently he tried to divorce the result from Einstein's theory. As Stuewer takes pains to show, Compton did not come to his insight out of an understanding of Einstein's work; he probably never even read Einstein's theory.

The fact that textbook accounts of the connection between the work of Einstein and Compton are misleading is neither surprising nor unusual. Textbook accounts of the history of physics are almost always fairy-tales designed to ration-

alize the present. What is surprising is that Stuewer, reflecting a lack of historical imagination, seems dismayed by such distortion. His history of the event is accurate, but it is dull. Much of the text recounts already-published historical accounts of related events. He devotes enormous amounts of space to mathematical derivations that serve no historical purpose. They may reassure Stuewer that he understands physics; they may make the book appealing to physicists; but they do not belong in a genuine history of physics.

In his introduction Stuewer writes of his intention to focus on conceptual issues rather than social and cultural ones. The problem is that these conceptual issues are intimately entwined in a single social and cultural skein, the separation of which leads to an unraveling of the entire skein into a number of weak strands.

STANLEY GOLDBERG
Hampshire College

JOHN T. BLACKMORE. *Ernst Mach: His Work, Life, and Influence*. Berkeley and Los Angeles: University of California Press. 1972. Pp. xx, 414. \$16.95.

Ernst Mach is a pivotal figure in the history of early twentieth-century science. Most know of him for his significant contributions to the study of physics and psychophysics, but some also consider him the father of many of the current schools of philosophical empiricism. This is the first comprehensive view of Mach's life, life style, and work. That in itself makes it an important book.

Blackmore fulfills his stated purposes: 1) He presents as much new biographical material on Mach as possible in order to make him more familiar to "the educated public." 2) He clarifies the extent and range of Mach's contributions. 3) He brings attention to the degree to which Mach was a central and controversial influence on developing ideas in twentieth-century physics and philosophy of science.

As a child Mach had a great deal of difficulty assimilating visual stimuli. He had trouble, for instance, comprehending perspective and shading in paintings. All of his life Mach considered perspective art to be misleading and unnatural. Blackmore shrewdly relates this to the fact that to the end of his life Mach maintained that "understanding" in science was synonymous with ordering and assimilating sense experiences, perhaps with mathematical descriptions, but without the encumbrance of such undefined hypothetical entities as atoms or theoretical structures as the kinematic theory.

In spite of sharp intellectual disagreements with Ludwig Boltzmann over the question of the valid-

ity of the atomic theory, Mach and Boltzmann held each other in high regard and not only valued their interactions as opportunities to learn from each other, but actually shared in a friendly fashion the same platform in public debate. In short, the biographical material that Blackmore provides adds an important dimension to our understanding of Mach's work and influence.

Though Blackmore may have fulfilled his stated purposes, the book has two serious shortcomings. Blackmore informs us that the original manuscript from which the biography was written was twice as long. In the process of cutting, Blackmore allowed his prose to become obscure. And while I felt that much had been chopped out, I sensed also that the material which remained had not been sufficiently edited. The chapter on Mach's influence on philosophy, for example, degenerates into a mere listing of those persons in philosophy who had read Mach and were willing to say so in public.

But the most serious problem with the book is Blackmore's failure to identify his audience. Just who is "the educated public?" At times it must certainly be well versed in physics, but at other times it must be told the formal definition on entropy. At times, it must know a great deal of philosophy, but at other times Blackmore feels the need to dwell *ad nauseum* on formal definitional distinctions between philosophical schools.

In spite of these shortcomings, Blackmore's biography of Mach is a useful and needed addition to the library of the historian of science. I, for one, extend him thanks for having undertaken and completed this awesome job.

STANLEY GOLDBERG
Hampshire College

H. STUART HUGHES. *The Sea Change: The Migration of Social Thought, 1930-1965*. New York: Harper and Row. 1975. Pp. x, 283. \$10.00.

In 1958 Stuart Hughes created a magisterial model of intellectual history to reveal with elegance, analytical precision, and methodological rigor the tensions between ideas and society. *Consciousness and Society* defined the major styles of twentieth-century thought as they developed concretely from 1890 to 1930 in an intellectual universe increasingly divided between reason and emotional value. Then, *The Obstructed Path* studied the generation of French thinkers who after 1930 turned from a national tradition morally and intellectually exhausted to a formalistic anthropology that left subjective and objective experience still sundered. In *The Sea Change*, his most personal book, Hughes concludes his remarkable revelation

of the roots and nature of contemporary thinking. The dual tradition bequeathed by Freud and Weber is resolved finally by the Central European émigré who combined, as does Hughes himself, scrupulous scholarship, intellectual honesty, and passionate ethical commitment.

The Sea Change is a psychosocial analysis of the effect of emigration upon the form and content of the émigré's social thought. Hughes concentrates upon the German-speaking intellectual who carried into exile doubts about the continuing validity of both "bourgeois humanism" and traditional culture or "Geist." The first chapter accepts the now conventional view that contemporary sociology and psychoanalysis resulted from a synthesis of German theory and Anglo-American empirical research, but the second argues that Wittgenstein in England, like Horkheimer, Adorno, Marcuse, and Tillich in America, were able to mediate between the two traditions only by refusing assimilation into their host countries. In the next chapter the enduring quality of Gaetano Salvemini's and Franz Neumann's critiques of fascism is attributed to their moral indignation, a fruitful use of broad Marxist categories, and a punctilious respect for evidence. Chapter four describes the preindustrial philosophical origins and the American isolation of the émigré critic of mass society who launched a precarious, but telling, cultural assault against both enlightened values and positivism. A fifth chapter treats ego psychology as the Freudian "Center" between the permissive "Left" of William Reich and Marcuse and the conformist "Right" of the neo-Freudian revisionists. In the concluding section, Hughes contends that there was an enriching exposure to Anglo-American thought only in those disciplines where nuanced language was unimportant. The half-understood, speculative émigré remained tied to his own European tradition and escaped solely in speculations on language and value. Émigré thinking, in its final effect, supports a conviction that neither Freud nor Weber would have accepted, but that Hughes finds correct: no individual's work can ever be either absolutely objective or value-free.

Hughes' richly textured, intellectually exciting trilogy offers a sustained and convincing explanation of our contemporary store of moral and intellectual concepts. In this final volume the crucial contribution of the embattled émigré becomes clear.

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ANTHONY DE CRESPIGNY and KENNETH MINOGUE, editors. *Contemporary Political Philosophers*. New York: Dodd, Mead and Company. 1975. Pp. xvii, 296. \$6.95.

As testimony to what they believe to be the vigorous state of political philosophy Anthony de Crespigny of the University of Cape Town and Kenneth Minogue of the London School of Economics have edited a collection of original essays by British and North American scholars on a dozen living political philosophers who have done their most important work since 1945. Six of the twelve thinkers are originally from the German-speaking world (Arendt, Hayek, Marcuse, Popper, Strauss, Voegelin), all of whom save Popper have taught and lived extensively in the United States. Three are French (Aron, DeJouvenel, Sartre), one is English (Oakeshott), one, Canadian (Macpherson), and John Rawls is American. The treatments are of an introductory kind designed to guide interested readers in their future explorations. Beginning with a brief introduction by the editors who deny the ideological, partisan, and prescriptive nature of authentic political philosophy, a position open to considerable debate, the volume concludes with biographical and bibliographical notes.

A number of problems are raised by the anthology. The first and obvious one is that of selection. Why is no one included from Eastern Europe, Italy, and the Third World? The left is far outnumbered by the liberal center and right, an inequality explained by the editors' reluctance to include what they term ideologists of a low intellectual level, although they admit that Marxists are "on occasions, formidably intelligent" (p. xiv). This criterion of selection seems to involve a most ideological notion of ideology, for surely Louis Althusser and Lucio Colletti are no more or less ideologists than, for example, Arendt, Hayek, Oakeshott, Strauss, and Voegelin, and at least as "formidably intelligent." Some compensation, however, for the ideological imbalance is the lengthy piece on Marcuse as a "representative figure," an excellent analysis by David Kettler. The choice of Popper instead of Isaiah Berlin is puzzling. Moreover, what is the justification for Aron's inclusion, especially since Ghita Ionescu throughout his able commentary insists upon referring to him as a political scientist? The essays are of an uneven quality, some being no more than basically uncritical eulogies—Dante Germino on Voegelin, Minogue on Oakeshott, De Crespigny on Hayek, E. F. Miller on Strauss—while others are competent evaluations—Anthony Quinton on Popper, Noel O'Sullivan on Arendt, Maurice Cranston on Sartre, and Samuel Gorowitz on Rawls. Only the slightest indication is given anywhere of the sizeable critical literature on most of the philosophers.

One must ask, of course, whether any of the twelve will be remembered in a few decades. It seems clear that some of them are largely the creatures of the whims and fancies of intellectual

fashion. What activist or student of political thought now pays much attention to Walter Lippman or Leonard Hobhouse who were so prominent a generation ago? Certainly Sartre will still be widely read in the twenty-first century, but how many of the others will only be studied, if at all, by curious historians seeking to map the culture of the age?

NEAL WOOD
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PETER N. STEARNS. *Lives of Labor: Work in a Maturing Industrial Society*. New York: Holmes and Meier Publishers. 1975. Pp. viii, 424. \$28.00

The title of this book conjures up an expectation that is unfulfilled. *Lives of Labor* does not contain E. P. Thompson's compelling and sensitive exploration of working class experience; neither does it offer the brilliant analysis one finds in Eric Hobsbawm's work; nor does it present the exhaustive documentation and the high level of theoretical discussion which distinguish a number of recently published French theses. The book lacks many of the attributes often associated with social history. It is informed neither by the rigors of systematic quantification nor by the insights about culture and human behavior of social scientists.

There are a number of tables and a statistical appendix. The tables, however, are often difficult to decipher. The sources of the data are rarely cited and the methods used to extract information from censuses are never specified. More important, however, many of the data are unrelated to the major argument of the book. Their presence lends an aura of precision and scientific accuracy to what is in fact a highly tentative and often carelessly thought-out argument.

The argument focuses on "the culture of work." Yet Stearns makes no attempt to define that term. He gives no hint of a theoretical discussion of the components of a "culture," no suggestion of how to analyze it. He indicates that he does not employ literature, songs, and theater (pp. 7-9), but that section is really a discussion of sources, not an attempt at conceptualization. Without a clear concept of culture, the argument founders. The evidence presented seems contradictory, for the reader is at once told of the diversity, complexity, and conflicting outlooks of different groups of workers, and also of the presumably homogeneous "culture of work" they evolved.

The historical aspect of the argument is also troubling. In essence, Stearns insists that in the two decades before World War I, workers developed some new but highly ambiguous attitudes about work. Their expectations for "job satisfac-

tion" were higher than those of their predecessors, in part because of "increased job choice." These workers failed, however, to create institutions which would either secure job satisfaction or compensate for its absence. Their adaptation to "mature industrialization" involved neither complete alienation nor total fulfillment. "The legacy" of the period was an "enduring tension between hope and reality" (p. 352).

This argument rests on an implied comparison with an earlier period, presumably the period of "immature" industrialization. Nowhere, however, is the comparison documented, nowhere are the differences spelled out. What *was* distinctive about the period from 1890 to 1914? Why not 1870 to 1914, or 1906 to 1936? In addition, many of Stearns' descriptions of changes in work pace, the fear of unemployment, and the displacement caused by technology apply to the early or mid-nineteenth century as well. His evidence of increased job choice comes primarily from public opinion polls, a measure of attitude which did not exist until this period. There is no evidence, in other words, that attitudes have really changed.

"Job choice," as Stearns uses the term, implies a desire for satisfaction at work and the freedom to choose among many options. It is a mid-twentieth-century notion of psychic fulfillment that has limited application even at the beginning of this century. "There were good reasons to choose textile work," says Stearns, "especially in places like Lancashire where parental example was obvious" (p. 71). Contrary to Stearns' implication, however, the "good reasons" had little to do with parental example or free will. No other work but textiles was available or paid as well in Lancashire. And that is why young people "chose" the same jobs as had their parents and probably their grandparents.

The case for "increased job choice" is flimsy and so is much of the rest of the book. Stearns' indictment of the labor movement for its preoccupation with wages and hours seems far-fetched. It distorts the reader's understanding of the relevant issues of the period, and it profoundly misinterprets the experience of working people at the turn of the century.

Above all, the book lacks discipline. The implications of the entire argument have not been worked out thoroughly. The evidence is not well-organized. Terms which demand definition ("maturation," "sophistication") are undefined. Even the sentences are often clumsy and unclear. *Lives of Labor* is a disappointing book. It does not fulfill the promise of its title, nor does it do justice to the importance and interest of its subject.

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ROBERT A. KLEIN. *Sovereign Equality among States: The History of an Idea*. Toronto: University of Toronto Press. 1974. Pp. xix, 198. \$10.00.

The appearance of Robert Klein's brief history of the idea of equality among nation states must be described as "timely" and "appropriate" in view of daily headlines reporting combinations of small states outvoting power, size, and wealth (usually the United States) or isolating other small states (usually Israel) at international assemblies. Klein's book reproduces the nineteenth- and twentieth-century background which led up to the current confrontation between the tradition of great power primacy, on the one hand, and the challenge posed by the more democratic, more chaotic "one nation, one vote" rhetoric and practice, on the other. Both Klein and Louis J. Halle in his generous foreword, share the intellectual persuasion that the idea of sovereign equality reflects a radical change in human thinking about international relations, a change in consciousness which, Klein argues, can be traced to episodes in inter-American relationships during the last decades of the preceding century, to the Wilsonians, and to the practices of regional associations of states in the twentieth century. Klein adheres to that school of intellectual formulation which believes in the moving power of ideas that have grown from seedlings to mature plants. His chapters cover the appearance of the idea of sovereign equality among nation states to sources in domestic legal forms, to tenuous roots at the Congress of Vienna, through occasional speeches at Latin American and European conferences in the pre-1914 generation, and then into the more familiar territory of the League of Nations, the FDR years of neighborliness, and the creation of the United Nations. Specialists in any of these epochs will recognize the names, events, and interpretations.

This study is essentially a restatement of the headaches created by the introduction of liberal democratic ideas by the western upper bourgeoisie into transatlantic societies during the late eighteenth century. From that point, both in the domestic and international order, the coupling of the word "equality" with "liberty" produced "discord" to quote Klein's major complaint about the new international order. "Discord" or contradictions must result from the utter impossibility of achieving equality and freedom simultaneously, a contradiction which many western liberals have finally learned to face. Restructuring, resurrecting, and defending great power responsibility in the international order may be a Band-aid solution to put off the day of reckoning upon another generation. Klein offers the impression that he knows

this unpleasant truth, but wishes it would go away.

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ARTHUR MARWICK. *War and Social Change in the Twentieth Century: A Comparative Study of Britain, France, Germany, Russia and the United States*. New York: St. Martin's Press. 1975. Pp. x, 258. \$16.95.

The subject of this essay in comparative history is the impact of the world wars on Britain, the United States, France, Germany, and Russia. There are four major themes: the destructive effect of war; the testing of societies by war; the participation of various groups in the wars (for example, the rise of the working class—which results from its usefulness in wartime); and the psychological impact of war. Arthur Marwick refers to these themes as his "four-tier model." All of this sounds very sociological, but in fact the author does not allow his "model" to dominate the presentation. Indeed, his history crowds out his sociology, and the "model" ends up being little more than a collection of useful topic headings; but there is nothing wrong with that.

Marwick emphasizes the use of film and fiction as sources for social history. There is a whole section devoted to archival film material. His annotated bibliography, notes, and index are all useful, and the plates are of considerable interest. The text is easy to read, and his judgments are quite sound. This will be a useful book not only for graduate students in military or social history, but also for upper level undergraduates—that is, if it comes out in a low-cost edition. The specialist, however, may find *War and Social Change* of limited value. Although it is nice to have so much statistical and bibliographical information in one volume, the book is brief and largely based on secondary sources. There is little new. Marwick is sensitive to the problems of methodology and causation, but his remarks on these topics—indeed, on all topics—are sketchy and hurried because he has so much to cover. Readers will be particularly interested in the author's comments on the intellectual and psychological aspects of his subject, though these too were frustratingly brief.

Marwick's conclusion is a relatively happy one: the human race has not only survived its world wars—so far—but it has also made progress, both despite and because of those terrible wars.

KARL G. LAREW
Towson State College

J. BOWYER BELL. *The Myth of the Guerrilla: Revolutionary Theory and Malpractice*. (Written under the auspices of the Center for International Affairs, Harvard University.) New York: Alfred A. Knopf. 1971. Pp. xiv, 285, ix. \$7.95.

JOHN DUNN. *Modern Revolutions: An Introduction to the Analysis of a Political Phenomenon*. New York: Cambridge University Press. 1972. Pp. xiii, 346. Cloth \$14.50, paper \$4.95.

MICHAEL ELLIOTT-BATEMAN *et al.* *The Fourth Dimension of Warfare*. Volume 2, *Revolt to Revolution: Studies in the 19th- and 20th-Century European Experience*. Totowa, N.J.: Rowman and Littlefield. 1975. Pp. 373. \$21.50.

These three studies are contributions to the rapidly expanding literature on what are variously called "guerrilla revolutions," "people's wars," or wars of "national liberation" in the twentieth century. All use the case study method. The authors agree on the nature of the phenomenon: the conjuncture of war and revolution in a largely peasant population, the preponderance of areas in colonial or semicolonial relation to the West, and the emphasis not so much on the conquest of the battlefield as of the mind—that is, the erosion of the will of the stronger and the development of an energizing myth capable of sustaining revolutionary ardor no matter what the sacrifice. Nevertheless, they differ in matters that concern origins.

J. B. Bell is concerned with the new generation of guerrilla fighters that appeared in the 60s. For perspective, he traces the use of guerrilla tactics from its first great "modern" success, the Irish Republican Army's Easter Rebellion of 1916, to Mao in the 30s, the World War II weakening of colonial powers, Dien Bien Phu, and corollaries to the general theory by Fanon, Castro, and Guevara. With Guevara's new "myth of the guerrilla," the fantasies of the "desperate and naive" now swept into fashion "over the old orthodoxies of political mobilization, parliamentary infiltration, trade-union militance, and balanced insurrections" (p. 42). Bell makes this new current the subject of three case studies: the wars of liberation in southern Africa, the terror tactics of the Palestine Fedayeen, and the attempt of Guevara to establish "another Viet Nam" in Bolivia—all substantial failures. He feels the "myth of the guerrilla" is out of touch with reality. The attempt to transform tactics into strategy, the notion that "repeated failure paid repeated benefits," has proved to be not only "ineffectual but often self-defeating" (p. 101).

A work of political theory, John Dunn's study also ends on a pessimistic note. The eight case studies from the Russian to the Cuban revolutions

are all examples of successful overturns of governmental authority. Five revolutions have in some sense been communist (the Russian, Chinese, Yugoslav, Vietnamese, and Cuban, though the latter "very much in retrospect"), and the communist revolutions affected the other three (the Mexican, Algerian, and Turkish revolutions). The bias of the examples, in contrast to Bell, is toward regimes that have attempted to establish an order of social relations inspired by Marxist ideas. Dunn agrees with Bell that twentieth-century revolutions are not based on Marxist theory, but he notes a "core of truth" in the Leninist theory of imperialism: these revolutions have taken the form of wars of national liberation, often from a colonial yoke. He concludes that revolution is "a metaphor of the recapture of control by the virtuous over an inimical destiny" (p. 255). In many ways it is a feeble remedy for the ills of some societies and "no remedy at all for many of them" (p. 257).

The general evolution of guerrilla warfare to its present advanced state is the theme of the Elliott-Bateman collection of essays. In contrast to the arguments of Bell and Dunn, its "modern phase" extends at least as far back as the French Revolution, though only since 1945 has there been an alarming acceleration of guerrilla activity, thereby introducing the "age of the guerilla [*sic*]" (p. 7). Ellis assesses select European revolutions since the Puritan to delineate the "guerilla-revolutionary marriage." He concludes: "The doctrines of a Mao or a Giap should not be regarded as a qualitative breakthrough in militarized revolution but rather as a more successful attempt to grapple with problems that had already been faced, and sometimes solved, in previous centuries" (p. 144). T. E. Lawrence's brilliant essay on the Arab revolt of 1916 and several essays on the Irish revolutionary experience by Foot and Bowden are also included. The closing essays by Elliott-Bateman on the "conditions" and the "battlefronts" of a people's war are designed to establish a "theory of this form of modern warfare."

All three studies have elements of strength, but they are not without shortcomings. Bell's concluding analysis of the "myth of the guerrilla" is perhaps as solid as anything in the literature, though he strangely neglects Georges Sorel's study of revolutionary myths. Dunn demonstrates a keen sensitivity to "Leninism," but he might have explored its application further in the case studies he cites. The case studies in Elliott-Bateman on the I. R. A. make exciting reading, though judgments concerning "conditions" and "battlefronts" often result in clichés or generalizations that require such qualification as to make the result of little or no value. But all three studies (with the possible

exception of Dunn's) are on uncertain ground when they try to establish "origins" or "revolutionary traditions." The shift in the twentieth century of the center of revolutionary change from the "core-countries" of Europe to those in colonial or semicolonial relation to Europe, a shift from urban to agrarian, from Western to anti-Western would appear to be of overriding importance.

The shift is instructive, because it also points to the centrality of the Bolshevik Revolution for the most important of these movements. The link is not Marx's pseudo-scientific theory of society. It is rather Lenin's model of a revolution which was, as Dunn rightly asserts, "elitist, manipulative, and unrelentingly opportunist." The semi-colonial status of Russia, the preponderance of a peasant population, and the anti-Western aspects of the civil war and intervention established the relevance of the Bolshevik Revolution for a variety of movements that followed, military techniques notwithstanding. Though innumerable qualifications may come to mind, the revolutionary wars of the twentieth century are related to the Bolshevik Revolution as nineteenth-century revolutionary disturbances in Europe were related to the French Revolution.

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Essays in Honour of E. H. Carr. Edited by G. ABRAMSKY. Hamden, Conn.: Archon Books. 1974. Pp. viii, 387. \$15.00.

Festschriften are notoriously difficult to review. Collections of articles tend to be different in content, disparate in quality, and altogether impossible to evaluate in a page or two. Without rising above its genre, the present volume, which was published to commemorate E. H. Carr's eightieth birthday, fortunately has more useful material and even more cohesion than many other similar works. Although some studies contained in it are clearly better than others, none is a total loss or in apparent need of wholesale correction. The several distinct parts of the book reflect the main aspects of Carr's huge scholarly activity, while they offer at the same time interesting evidence of contemporary British scholarship, particularly in the Russian field.

Part One, "Intellectual History in the Nineteenth Century," consists of four contributions: Isaiah Berlin's sparkling study of Georges Sorel; Monica Partridge's close examination of Alexander Herzen in his last phase; Arthur Lehning's exposition of "Bakunin's conceptions of revolu-

tionary organizations and their role"; and G. A. Cohen's logical analysis of the relationship between being and consciousness in Marxism. Part Two, "Diplomatic History and International Relations," adds another four contributions by four different authors. Beryl J. Williams writes on "the Revolution of 1905 and Russian foreign policy"; Eleonore Breuning on "Brockdorff-Rantzau: the 'wanderer between two worlds'"; and D. C. Watt on "the initiation of negotiations leading to the Nazi-Soviet pact." Finally, Roger Morgan concludes the part with an appropriate and not at all uncritical discussion of "E. H. Carr and the study of international relations." Part Three, "Soviet Studies," is the longest one in the book, some 160 pages compared to about 100 in Part One and about eighty in Part Two. Six new authors make their contributions. Alec Nove offers some observations "on Bukharin and his ideas"; John Erickson deals with certain "military and political aspects of the 'militia army' controversy, 1919-1920"; Michael Kaser draws some statistically grounded comparison between tsarist and Soviet education. Next, R. W. Davies and Moshe Lewin write on closely related themes: the first on "the Soviet rural economy in 1929-1930: the size of the Kolkhoz," and the second on "Soviet policies of agricultural procurements before the War." The sixth author, Maurice Dobb, engages in "some historical reflections on planning and the market" instead of delving into a more circumscribed research topic. The fourth and last part, "Related Studies," is the shortest and serves as something of a grab bag. The two contributors, Mary Holdsworth and Lionel Kochan, discuss respectively "Lenin's *Imperialism* in retrospect," and "Graetz and Dubnow: two Jewish historians in an alien world." In addition to the sixteen studies, the volume contains a frontispiece photograph of E. H. Carr, a brief tribute to him by Abramsky, a bibliography of his works by Beryl Williams, notes on the contributors, and a general index.

The *Festschrift* is obviously rich in content and quite well done. The most interesting studies turned out to be Berlin's perceptive analysis of Sorel's thought, which emphasizes its striking relevance to the present day (a version had been published in the *Times Literary Supplement* of December 31, 1971), Watt's probing search for the exact time of the initiation of the negotiations leading to the Nazi-Soviet pact, and Lewin's expert treatment of Soviet agricultural procurement, which presents better than any other short piece the tragedy of the Soviet peasant.

NICHOLAS V. RIASANOVSKY
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Berkeley

ANCIENT

DAVID DAUBE. *Civil Disobedience in Antiquity*. Edinburgh: Edinburgh University Press; distrib. by Aldine-Atherton, Chicago. 1972. Pp. xii, 167. \$7.50.

Civil disobedience proved to be an effective tactic of opposition in the sixties for groups excluded from the establishment in American society. In a series of public lectures delivered at Cornell University in 1971, David Daube, a distinguished legal scholar, appropriately focused attention upon similar dissent by disaffected social classes and minorities in antiquity. These lectures are now published in a small book of six chapters, dealing with women, the young, slaves, prophets, philosophers, religious minorities, and groups aspiring to statehood.

Adopting the lawyer's definition of civil disobedience as breaking of the law by nonviolent means, in which the perpetrator is willing to undergo the punishment prescribed by it, Daube explores his theme broadly and is able to maintain an interesting balance in each category of familiar and less-known cases from the Judaeo-Christian, Greek, Hellenistic, and Roman world. The Bible and writings from ancient Greece provide the best-known examples of nonviolent disobedience, but one is struck by the degree to which the Roman dissenters are less likely to be known to the intelligent layman. The secession of the plebs in the Early Republic is familiar enough from Livy, but Daube digs deeper to recall the publication of the *libri pontificum* and those Roman matrons whose extravagance prompted the Oppian Law and denunciation by the elder Cato.

Written in a chatty, casual style, the book, with its wealth of examples, develops historical perspective on a subject of current concern.

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Chapel Hill*

MEDIEVAL

JOSEPH F. O'CALLAGHAN. *A History of Medieval Spain*. Ithaca: Cornell University Press. 1975. Pp. 729.

A timely manifestation of the new interest in medieval Spain among English-language scholars, this book is a comprehensive narrative history based on an impressively wide reading in the sources and secondary literature. It should prove useful to teachers, students, and general readers in European history, although it is possibly too long—and, in its present form, too expensive—to serve as a required textbook. Soundly traditional in its organization, it gives primacy to political

events without neglecting institutional, social, and cultural matters. It may be said, on the whole, to have supplanted the pertinent chapters in the *Cambridge Medieval History*, being equally readable and much more nearly up-to-date.

Among the book's substantive merits may be mentioned its ample discussion of the Visigothic period, which is ceasing to be negligible as it becomes better known; its vigorous and sympathetic treatment of the Muslim realms; and its insistent attention to Portugal. Joseph F. O'Callaghan has scrupulously avoided interpreting the early Conquest generations in a Christian perspective or the Reconquest ones in primarily Castilian terms; nor does he minimize the interest of the Moorish remnant-states and Mudéjar communities in the later Middle Ages. He tends to accept the theses of Menéndez Pidal and Sánchez Albornoz on institutional and ideological continuity and political and social reconstruction. His exposition of political and military events after 1085, if not always easy reading, justly appreciates the complications of purpose in the ruling classes of the many human groups which retained or achieved political identity in Reconquest Hispania. The powerfully subsistent idea of a single Spain was in brutal conflict with equally powerful impulses of a particularist or dynastic nature.

The book seems to this reviewer better on political and institutional topics than on social-cultural ones, an imbalance which may bear some relation to the prevailing state of the underlying scholarship. Spain's importance for the development of associative structures—towns, military-clerical orders and representation—is well brought out. Feudalism and "courtly love" are less successfully interpreted, partly, at least, because the author is insufficiently critical of the conventional but misleading labels for these complex phenomena. But the comparative study of vassallic and feudal relations in the Hispanic realms is only just beginning, and in this matter O'Callaghan could have found little conceptual stimulation in the works he has relied upon. The treatment of early Catalonia is not altogether secure. Vich and Cardona are not in the Segre basin (p. 106); nor is there any reason to doubt, for unequivocal evidence survives, that the Catalán and Aragonese towns were represented in the assembly of Lérida in 1214 (p. 437). Moreover, the emphasis on Catalonia's singularity in the comital period, a main point of this book and hitherto generally accepted, has been shown to be mistaken in recent work by Bastier, Bonnassie, and Zimmermann.

The studies just mentioned, however, were published only when the present volume was complete. The point in question is mentioned simply to

illustrate the major difficulty with this courageous work. So much basic research remains to be done on medieval Spain, and is currently being done, that a book of this kind must inevitably be provisional in important respects. But the book was needed now, and both author and publisher deserve hearty thanks for producing such a valuable survey. May they be encouraged to keep it up to date and in print!

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ANDREW HANDLER. *The Zirids of Granada*. Coral Gables: University of Miami Press. 1974. Pp. 208. \$10.00.

Like Renaissance Italy, eleventh-century post-Caliphal Islamic Spain was a welter of principates distinguished for culture, intrigue, and bloody wars. Neglected until recently (Lévi-Provençal's magisterial history stops short; Prieto is relentlessly numismatic; and Menéndez Pidal illuminates fitfully), it has been yielding its secrets to determined researchers: Bosch Vilá (1959) on Albarracín, Huici (1970) and Sanchis Guarner (1965) on Valencia, Sosselló Bordoy (1968) and Busquets Mulet (1973) on the Balearics, Monroe on society, and Ashtor and Baer on the Jews, not to mention the literary and architectural researchers.

Now Andrew Handler, in his dissertation from Columbia University, has industriously sorted out the brief Berber sultanate of Granada (at the very time Arié and al-ʿAbbādī have covered its illustrious later namesake). The intractable sources limit him to a "political study," the mind-numbing but important sequence of battles, cruelties, revolts, and other drum-and-trumpet circumstance; a final chapter discovers Umayyad rather than African Zīrid administrative-social patterns. Handler systematically interweaves such well-known and often later chroniclers as Ibn al-Abbār, Ibn Abī Zarʿ, Ibn Bassām, Ibn ʿIdhārī, Ibn Khaldūn, Ibn al-Khaṭīb, al-Maqqarī, and al-Marrākushī, but especially the memoirs-cum-history by ʿAbd Allāh b. Bulūqqīn, the last Zīrid ruler. He also leans heavily on Lévi-Provençal, Idris, Dozy, Ashtor, and Baer. One feels unease at the somewhat restricted bibliography (neither Huici nor Bosch is there, for example, nor the posthumous Torres Balbás collection, nor, among the translations, Hoenerbach's 1970 Ibn al-Khaṭīb); but that is minor. Handler has done what he set out to do, in lucid and sober style, with the essential materials well researched and arranged—to fill the gap of a political history for Granada's Banū Zīrī.

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AZIZ AHMAD. *A History of Islamic Sicily*. (Islamic Surveys, 10.) Edinburgh: Edinburgh University Press. 1975. Pp. xi, 147. £2.50 net.

Muslim rule of Sicily began and ended with treachery or folly: in 827, the Byzantine rebel Euphemius called on the Tunisian Aghlabids as allies, offering them sovereignty over the island in exchange for the title of emperor for himself; almost a quarter of a millenium later, a desperate local qāʿid, Ibn al-Thumna, appealed to the Normans for help, in the foolish hope that they would return the island to him after having conquered it.

Even at the peak of Sicily's Islamic period under the Kalbite emirs, effective Muslim control remained uneven beyond the Val di Mazara in the southwest. The Greek population, concentrated in the northeast, clung tenaciously to its Christian identity. The Muslim majority—mostly Arabs and Berbers, with a sprinkling of Negroes, Persians, and Turks—was rarely without internal feuds and rivalries. Benevolent Norman rule brought out the best in the unique coexistence of two great cultural and intellectual traditions and made Sicily one of the major transmitters of Greek and Islamic learning to the Latin world. Yet in the same period we witness a steady decrease of Sicily's Muslim population through emigration and voluntary conversion. Ironically, it was the Hohenstaufen Frederick II, perhaps the greatest admirer of Islamic culture, who terminated the Muslim presence on the island. Resettled in the military colonies of Lucera and Grottefalcone on the Apulian plateau, the last Sicilian Muslims survived as a distinct ethnic group until the end of the thirteenth century. In 1300, they were forcibly converted by Angevin orders.

Aziz Ahmad gives us a compact, well-documented history of Islam in Sicily. If severe restrictions of space have reduced some passages to little more than enumerations of names and dates, a thirteen-page bibliography of Arabic and Western sources compensates to some extent for this shortcoming. Of the 283 items listed, a mere thirty-four are English titles, of which only thirteen are of immediate relevance. Nothing could demonstrate better the importance and value of this contribution.

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STANLEY RUBIN. *Medieval English Medicine: AD 500-1300*. New York: Barnes and Noble. 1975. Pp. 232. \$15.00.

Slightly more than a century ago the Reverend Oswald Cockayne edited for the Rolls Series the monumental *Leechdoms, Wortcunning and Starcraft of*

Early England. Since that time most readers in medieval medical lore have been fascinated by the bizarre nature of what they assumed to have been standard medical practice in Anglo-Saxon times. No doubt magic, astrological calculations, and religious incantation belonged to the leech's art, and shocking, nauseous substances such as burnt mice, frog's blood, and ox's urine were among the ingredients of some medical recipes. Yet, by over-emphasizing such oddities, often to the exclusion of the more basic elements, many successors of Cockayne have served to denigrate all medieval English medicine.

Stanley Rubin, in his *Medieval English Medicine*, adopts a more realistic approach. Assuming that no art based mainly on nonsense can long endure, he shows that medieval English medicine was essentially rational, and that many of its practitioners were persons of compassion and good sense. Here he accepts the thesis of C. H. Talbot who a decade ago (*Medicine in Medieval England*) challenged some of the assumptions attributable to Cockayne's work. Though Rubin's book is a good introduction to the subject, he makes no claim to definitiveness. His use of primary sources is assuring, and he has described the more significant ones in appendices. Although he has established the year 1300 as the *terminus ad quem*, the Anglo-Saxon era receives the major emphasis. Here Rubin accepts the current thesis that English medicine in the ninth and tenth centuries was not inferior to that of its continental neighbors, since the best of classical medical knowledge was incorporated into Anglo-Saxon medical literature even before it became a part of the Salernitan tradition.

A laudable feature of the Rubin book is the opening chapter devoted to archaeological and paleopathological evidence, a source of data frequently overlooked. There is a splendid chapter on leprosy and society's attitudes toward it. In certain areas the paucity of sources necessitates cautious generalities, as is true in the matter of the development of medicine as a profession and in some of the features of monastic medicine. Rubin has resisted the temptation to read more into his sources than is warranted, and his interpretations are well considered and generally defensible.

E. A. HAMMOND
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G. O. SAYLES. *The King's Parliament of England.* (Historical Controversies.) New York: W. W. Norton and Company. 1974. Pp. x, 164. Cloth \$7.95, paper \$2.95.

This brief book, which is based on the published and unpublished works of G. O. Sayles and H. G. Richardson, should command the attention of all

students of constitutional history. For half a century these indefatigable scholars have contributed to the understanding of the governance of medieval England. It is the simple truth that any discussion of medieval parliaments must start with a review of their findings.

The King's Parliament of England consists of seven essays, varying somewhat in length and degree of specialization. The first, "Modern Myths and Medieval Parliaments," sets the emphasis for the book. In spite of the misconceptions fostered by seventeenth-century polemicists and such nineteenth-century scholars as Stubbs, who studied the history of parliament in terms of what interested them, parliaments were not originally, and never primarily, representative assemblies preoccupied with legislation and taxation. They were courts rather than legislatures, their business primarily judicial; the absence or presence of representatives of the commons was irrelevant to their competence and authority. Sayles is gentle and patient with those slow to grasp this fact, which the records amply support. Yet he does get in a swipe or two at the History of Parliament Trust. "We only delude ourselves," he writes, "if we believe that an understanding of the medieval parliament can be reached by compiling biographies of members of the House of Commons." The interpretation is further developed in the succeeding essays, especially those on the Provisions of Oxford, the Parliaments of Edward I, and the Ordinances of 1311.

In the last essay, "The King's Parliaments in Perspective in the Later Middle Ages," Sayles discusses some of the changes which occurred as the judicial work of parliaments decreased and political matters became their major concern. He reminds the reader that as late as the reign of Henry VII, it was accepted that certain matters were for parliaments only, while other important questions came fully within the competence of great councils meeting without elected representatives.

The Appendix contains a list of parliaments from 1258 to 1377, an admirable glossary, of special utility for beginning students, and a useful bibliography.

CATHERINE STRATEMAN SIMS
Agnes Scott College

R. H. HILTON. *The English Peasantry in the Later Middle Ages.* (The Ford Lectures for 1973 and Related Studies.) New York: Oxford University Press. 1975. Pp. 256. \$21.00.

R. H. Hilton's first concern in these Ford Lectures is to establish the position of the peasants as a social class. This he does in a five-point definition (p. 13). He studies the peasantry of the West Midlands—a region he has made his own—during the century after the Black Death. The peasant family

holding was usually one yardland or half as much. The typical peasant household included three generations, but the generation gap was narrow, and grandparents took care to make their own place secure before handing over. It was from this extended family that jurors, tithingmen, reeves, and ale-tasters were drawn. These notables were in *de facto* control of village affairs; the manor court enforced their authority.

In his lecture on "The Small Town as Part of Peasant Society" Hilton is not concerned with Coventry or Bristol, but with the scores of towns with 500 or fewer inhabitants, each "contained in a rural envelope of arable fields, meadows, and pastures." Despite their involvement with surrounding fields and pastures, "the weekly market was the focus of their lives" (p. 83). In "Women in the Village" Hilton suggests that peasant women did every kind of agricultural work and were paid as much as men. Customary law awarded widows more than common law, and there was less compulsion for widows to remarry than in the aristocracy.

Hilton presents his evidence in detail. Nearly all his references are to unpublished sources. Conclusions are infrequent, however, which is disappointing, for Hilton's acquaintance with the West Midland peasantry is unequalled, and one would wish for more of the fruits of his reflection.

The six lectures fill 110 pages and the rest of this small volume is given over to some of Hilton's related papers, "now difficult to obtain." They are "The Social Structure of Rural Warwickshire in the Middle Ages" (1950); "Gloucester Abbey Leases of the Late Thirteenth Century" (1953); "A Study in the Pre-History of English Enclosure in the Fifteenth Century" (1957); "Rent and Capital Formation in Feudal Society" (1962); and "Lord and Peasant in Staffordshire in the Middle Ages" (1960).

W. O. AULT
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CHARLES ROSS. *Edward IV*. (The "English Monarchs" Series.) Berkeley and Los Angeles: University of California Press. 1974. Pp. xvi, 479. \$25.00.

Charles Ross' book is the first full-length study of Edward IV since Cora L. Scofield published her two-volume "Life and Reign" in 1923. Scofield's book, a painstakingly minute narrative of politics and diplomacy, will remain valuable for its detailed information. It is intolerably dull, however, and suffers from the dearth of information on late fifteenth-century social and political conditions available at the time it was written, and it offers the minimum of interpretation. Ross' book, though far less detailed in its political narrative, is

grippingly readable. It presents a new, vivid synthesis based upon the author's own researches and upon his cool analysis of the work of the present generation of historians who, since about 1956, have vastly increased our understanding of the social and governmental structure of late fifteenth-century England. Its most characteristic contributions lie in Ross' comments on the royal finances, foreign policy, and the related subjects of the nobility, patronage, and the king's controversial marriage to Elizabeth Woodville.

Ross is skeptical of recent panegyrics of Edward's financial acumen. He accepts B. P. Wolffe's thesis on the importance of Edward's reformed administration of crown lands, but he rightly questions the high level of efficiency Wolffe claimed for the new system. Ross seems to press too far his doubts about the existence of the king's policy of estate accumulation. He takes insufficient account of the harsh, political realities of patronage, even bribery, which made it essential for a king, who had only just managed to seize the throne by force, to buy the support of powerful men.

Ross could have taken a more critical view of the effects of fiscal feudalism and the efficiency of the customs system. The famous comments of the Second Anonymous Croyland Continuator on the king's increasing financial severity may reflect little more than the resentment of feudal tenants and merchants who naturally disliked attempts to tighten control. For example, though Edward appointed strict supervisors of the customs in most of the ports, he never attacked the problem of scandalous undervaluation. Ross does not seem to be aware that the conventional customs valuation of many goods was as little as one-seventh of their true commercial value.

The author agrees with recent reassessments of the nobles' role—that they were drawn only with the greatest reluctance into the Wars of the Roses and that Edward never attracted the active loyalty of the peerage as a whole. Yet they and their affinities were vital for controlling the countryside, and Edward, creating a large number of new peers to support his throne, distributed his patronage with this in mind. In the end he failed because he distributed patronage too narrowly, and he alienated powerful families by arbitrarily interfering with the common-law rules of inheritance at their expense. Thus, in the last years of his reign politics became the preserve of about one-third of the nobility. This narrow basis of support, together with the universal (perhaps Ross exaggerates here?) unpopularity of the queen's family, the Woodvilles, left the peerage deeply indifferent to the fate of Edward's son and thus made possible the usurpation of Richard III. So we are left with the harsh verdict that Edward failed in the most important

task of a dynast: to secure the succession of his heir.

J. R. LANDER
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MODERN EUROPE

LAWRENCE STONE, editor. *The University in Society*. Volume 1, *Oxford and Cambridge from the 14th to the Early 19th Century*; volume 2, *Europe, Scotland, and the United States from the 16th to the 20th Century*. (Written under the auspices of the Shelby Cullom Davis Center for Historical Studies, Princeton University.) Princeton: Princeton University Press. 1974. Pp. ix, 352; viii, 355-642. \$12.50 each, \$22.00 the set.

This book of essays, a cornucopia of facts, figures, and interpretations, is a product of a research seminar held at the Shelby Cullom Davis Center for Historical Studies from 1969 to 1971. All thirteen contributors were participating members of the seminar, either as visiting fellows of the Center, as faculty at Princeton University, or as graduate students; and, as Lawrence Stone, Dodge Professor of History at Princeton and editor of this book, points out, "in one way or another they are all interested in the relationship between formal education and other social processes, rather than with either [*sic*, ed.] the history of educational institutions as such, or with [*sic*] the history of changes in the curriculum and scholarship as such" (p. v). Their primary concern is with the sociology of education.

Volume I deals with developments in Oxford and Cambridge from the fourteenth to the early nineteenth century. Lawrence Stone presents data about the size and composition of the Oxford student body (1580-1909); Guy Fitch Lytle analyzes patronage patterns and Oxford colleges (c. 1300-c. 1530); and James McConica discusses findings about scholars and commoners in Renaissance Oxford. Victor Morgan examines Cambridge University and "the country" (1560-1640); Sheldon Rothblatt describes student subculture and the examination system in early nineteenth-century Oxbridge; and Arthur Engel writes about the emerging concept of the academic profession at Oxford (1800-1854).

Volume II is concerned with significant educational and societal developments in Europe, Scotland, and the United States from the sixteenth to the twentieth century. There are essays on a variety of topics: universities in Castile (1500-1810), by Richard L. Kagan; culture and society in the eighteenth-century province (the case of Edinburgh and the Scottish Enlightenment), by Nicholas Phillipson; American student societies in the early nineteenth century, by James McLachlan; univer-

sity reformers and professorial scholarship in Germany (1760-1806), by R. Steven Turner; the sources of German student unrest (1815-1848), by Konrad H. Jarausch; economists as experts and the rise of an academic profession in America (1870-1917), by Robert L. Church; and the values and goals of freedmen's education in America by James M. McPherson.

In general, the authors demonstrate commendable scholarship, productive approaches to research, and intelligent use of sources. Their interpretations are provocative without being dogmatic, and, with the exception of one contributor with a penchant for hyperbole, the authors' judgments are usually sound. The statistical data provided by Stone (pp. 3-110) and the perceptive essay by Sheldon Rothblatt (pp. 247-303) should prove to be of particular value to historians. Contemporary scholars, caught in the academic vise that, on one side, pressures them to teach, and, on the other side, pressures them to do research and writing, may also want to spend considerable time with R. Steven Turner's essay, "University Reformers and Professional Scholarship in Germany 1760-1806" (pp. 495-531).

In brief, although no attempt is made to synthesize these essays, the book reflects the achievements of a number of fine scholars.

DANIEL ROSELLE
National Council for the Social Studies

MOSES A. SHULVASS. *The Jews in the World of the Renaissance*. Translated by ELVIN I. KOSE. Leiden: E. J. Brill. 1973. Pp. xv, 367. 58 gls.

This work was first published in Hebrew in 1955. The present English title is a misnomer, since the book deals only with the Jews of Italy in the period 1300-1600, and not with the Jews of the general European Renaissance.

Moses A. Shulvass' solid study shows how the Italian Jewish community developed from a relatively indigenous one centered in Rome to a mixed one, as refugees flowed in from France, Germany, Spain, and the Levant. Gradually three major communities developed: Italian, Ashkenazic, Sephardic. Local situations plus the variations in the traditions and practices of the groups led to different kinds of communities, with different economic activities, Jewish organizations, linguistic practices, etc. Shulvass gives a detailed and often fascinating picture of what Jewish life was like at this time, what its cultural achievements were, and how Jews interacted with Italian society.

Generally, this study focuses on the Jewish communities themselves (Shulvass estimates the Jewish population around 1600 at 35,000) with very little emphasis on the effect of Judaism on the non-

Jewish world around it and on the emerging free Jewish world in the Netherlands. Shulvass notes the impact of the Renaissance on the traditional religious communities and shows how these communities managed to retain their orthodoxy despite the influences of the Renaissance. What has become more interesting for intellectual historians of the period—the influence of Jewish, and the formerly Jewish writers and thinkers on the general Renaissance and Reformation currents—is hardly treated. He does not mention Jewish Averroism. Nor does he examine the roles of the Jewish humanists around Pico and Ficino or of Abraham de Balme in retranslating Arabic thought. Off-beat Jewish movements, such as that led by David Reubeni are likewise excluded, as are the roles of various Jewish writers like Leone Ebreo and his father, Don Isaac Abarbanel, plus some of the converts, on Messianic and Millenarian movements of the time. Finally, the polemical writings of some of the Marranos, like Elijah Montalto, that were to be so important for Amsterdam Jewry (whose chief rabbi, Saul Levi Morteira, came from Italy) are not discussed.

The author's study deals primarily, and almost exclusively, with normative Judaism and how it preserved itself in the Italian context. As such, the work is rich in detail. Those interested in how Judaism and heterodox Jewish thinkers affected the wider world and contributed to aspects of the Italian Renaissance, however, will have to look elsewhere.

RICHARD H. POPKIN
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FRANCES A. YATES. *Astraea: The Imperial Theme in the Sixteenth Century*. Boston: Routledge and Kegan Paul. 1975. Pp. xvi, 233. \$21.75.

For almost three decades, Frances Yates, through her knowledge of art, music, literature, and philosophy, has shed new light on our perceptions of European ideas, especially in the Renaissance and seventeenth century.

Astraea fills in areas between the author's earlier books: *The French Academies of the Sixteenth Century* (1947), *The Valois Tapestries* (1959) and *The Rosicrucian Enlightenment* (1972). *Astraea* traces the symbols and ideas of the universal empire as they were revived and used for Emperor Charles V, translated and transformed for Elizabeth of England, Charles IX, Henry III, and Henry IV of France. The Roman world-ruler concepts and trappings, including eponymous Trojan founders, are imitated and altered to serve the national monarchies of France and England. In England the depictions of Elizabeth as Astraea, Virgin Justice reforming

the world, expressed an aspect of the imperial ideal. In France the elaborately planned royal processions and celebrations displayed the French monarchy as the ideal of reconciliation in times of religious civil wars. Reform and reconciliation, attributes of universal rule, are transferred to fit the national monarchies.

Astraea, though stimulating and rewarding, is uneven. It is a collection of items dating from 1945 through the 1950s; and in spite of the author's assertions, the parts do not always relate clearly to the whole (e.g., the chapter on the Joyeuse Magnificences). Nor does the evidence always support Yates' statements (e.g., Erasmus as a link to an Elizabethan view of Charles V as a reformer [pp. 55-6]). Though there are flaws, these essays make up a stimulating and worthwhile book.

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CHARLES TILLY, editor. *The Formation of National States in Western Europe*. (Studies in Political Development, 8.) Princeton: Princeton University Press. 1975. Pp. xiv, 711. Cloth \$22.50, paper \$4.95.

This volume represents an effort of political scientists and historians to test current theories of political development against the European experience. Supported by grants from the Ford, Rockefeller, and National Science Foundations, an international group of scholars under the leadership of Gabriel Almond and Charles Tilly participated in seminars and workshops which produced the papers presented here.

Tilly, "a sociologist who often works with historical materials," makes the major contribution to the success of the book. Historians will find themselves most at home with his introductory essay, which, in addition to providing reviews of all the papers and a summary of themes, includes some sophisticated reflections on the nature of the available evidence for the study of European state-building and on the kinds of questions which studies of this sort might be expected to answer. With respect to the pre-sixteenth-century background, Tilly stresses the cultural homogeneity of Europe, the prevalence of a peasant society, the existence of a decentralized but relatively uniform political structure, the early development of specialized organizations, the openness of the European periphery, and the growth of cities and early capitalism as factors favoring the growth of national states.

Tilly's discussion of recurrent themes points out that although his authors did not try to agree on a common definition of state, they tended to converge on a rather narrowly political notion of

"stateness." They found too that "nation-building"—the development of national consciousness, participation, and commitment—was something that generally occurred in Europe after the formation of strong states. Contributors agreed on the tremendously high costs of state-building in terms of death, suffering, loss of rights, and the forced surrender of land, goods, and labor. The largest single incentive to extraction of resources and the chief means of state coercion was the formation of standing armies.

Space allows little more than a brief listing of the other essays. Samuel Finer, a British historian, analyzes the building and maintenance of armed forces. Gabriel Ardent, a Swiss economist and historian, treats the fiscal means by which states extracted resources from their populations. Rudolph Braun, a Swiss historian, contributes a comparative study of taxation in Britain and Prussia. David Bayley, an American political scientist, studies the development of police systems. Tilly's essay deals with problems relating to food supply and public order. Wolfram Fischer and Peter Lundgreen, German historians, write about the recruitment and training of administrative and technical personnel. Finally, Stein Rokkan, a Norwegian political scientist, proposes a set of variables as a possible paradigm for research on variations within Europe. All the contributions are highly informative, but none match Tilly's for readability.

Ironically, given the sponsorship and purpose of the volume, Tilly concludes "that our ability to infer probable events and sequences in contemporary states from an informed reading of European history is close to nil." Despite the enormously different circumstances that make direct inferences nearly worthless, however, he still believes that "some general relationships among the ways of building state power, the forms of relationship between men and government, and the character of the political institutions which emerge from the process of state-building which held within the European world still hold today." Whether this hope is justified, these essays are well worth reading by anyone interested in the political history of modern Europe.

RICHARD A. LEBRUN
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OREST RANUM, editor. *National Consciousness, History, and Political Culture in Early-Modern Europe*. (The Johns Hopkins Symposia in Comparative History.) Baltimore: Johns Hopkins University Press. 1975. Pp. x, 177. \$10.00.

This book comprises six essays on the nature of "national consciousness" in the period from the

Renaissance to the outbreak of the French Revolution in six countries that in the nineteenth century can clearly be recognized as "nation states." The authors are Felix Gilbert (Italy), William F. Church (France), Leonard Krieger (Germany), John Pocock (England), Michael Cherniavsky (Russia), and Helmut Koenigsberger (Spain). Orest Ranum's introductory essay ties together the findings of the six other historians and also explores some of the basic issues connected with the theme. Each of these seven essays is highly original and thought-provoking, and each merits a full discussion which a brief review cannot give. A few general observations must therefore suffice.

Though the essays follow different lines of approach, they seem to arrive at the same general conclusion—national consciousness was not a force to be reckoned with in the early-modern period in any of the six countries under investigation. In fact, it did not even exist, except among some English revolutionaries and, toward the end of the period, in the minds of a few continental intellectuals. It was not national consciousness that underlay the dynastic unification of the states, but rather the dynastic policies of the rulers that made possible the emergence of national consciousness in a later age. The trouble with the concept "national consciousness" lies in the adjective "national," with its anachronistic overtones of present-day "nationalism," which glorifies national popular unity and national culture, and which is quite distinct from the age-old xenophobia evident in relations between neighboring provinces and cities in the medieval and early-modern era. Though the authors do not explicitly advocate the total abandonment of the term "national consciousness" for the early-modern period, in fact each of them analyzes the changing contents of collective identities of various groups, most much smaller than the nation—a cultural elite, a dynasty, an aristocracy, or a provincial community. But a group can also lay claim to universality, as in the case of the post-Tridentine Catholic Church, the Holy Roman Emperor of the Germans, or the Christian Roman Empire of the Russians. In most instances these group identities, whether particularist or universalist, impeded rather than favored the appearance of national consciousness.

One can determine the essence of the identity of a group by analyzing its interpretation of history, its religious attitudes, and its artistic tastes. Most of the authors deal with perceptions of history by different social groups or by individuals who helped shape the outlook of the political classes. The ethos of some groups, however, may differ from, or even be antithetic to the mental disposition of the politically dominant groups. Witness

the millenarianism of the English Puritans, the lower-class antiaristocratic insistence on the purity of Christian blood in Castile, or the antitsarist concept of Holy Russia. Consideration of many such frames of mind coexisting with the establishment-approved currents adds further depth to the perspectives revealed in this book.

ANDREW LOSSKY
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PETER GAY, editor. *Eighteenth-Century Studies Presented to Arthur M. Wilson*. Hanover, N.H.: University Press of New England. 1972. Pp. viii, 197.

Arthur Wilson's many friends and admirers, including the present reviewer, will be pleased to welcome this tribute to a well-loved and highly esteemed scholar, who crowned nearly forty years of distinguished and devoted service to Dartmouth College with the completion of his remarkable *Diderot* in 1972. In view of Wilson's versatility as professor of both biography and government, this *Festschrift* is fittingly characterized by considerable diversity, for it embraces history, the history of ideas and literature. The first essay, "The Age of Personal Monarchy in England," by Stephen B. Baxter, shows that in spite of his declining role in the eighteenth century, the king could still exert considerable influence. Blake T. Hanna's study of the physicist Pierre Polinière analyzes an apparently much-used manual of the time, his *Expériences de physique* (1709), which is of interest as the work of a teacher and scientist who represents the transition from rationalism to empiricism. Paolo Casini's "Crudeli Affair: Inquisition and Reasons of State," carefully unravels the complicated threads of a fascinating little episode in Italian history and concludes that Crudeli was somewhat different from the radical thinker portrayed by Diderot in his *Entretien d'un philosophe avec la Maréchale de **** (1777). James Clifford has drawn upon his expert knowledge of Samuel Johnson to give a lively account of Johnson and his foreign visitors; he proves that the doctor's notorious hatred of foreigners in general did not exclude friendly reactions to them in particular.

Several essays deal with the philosophes. As it would certainly have been inappropriate for this volume not to contain at least one contribution by a specialist on Diderot, Jacques Proust examines the difficult question of Diderot and the legal theories of antiquity in a way that effectively opens up several avenues for further exploration. Ernest J. Knapton studies Napoleon's reactions to the philosophes, but concludes that with the exception of some extensive notes and comments on Rousseau (already discussed by F. G. Healey) and some

brief remarks on Montesquieu, the impact made by the philosophes upon Napoleon was slight. Roland Desné scrutinizes the twenty-fifth letter (the famous "Anti-Pascal") of Voltaire's *Lettres philosophiques* (1734) with a view to showing its tenuous but significant links with the English emphasis of the rest of the work. Deserving of close attention is Richard Kuhn's painstaking study, "Hume's Republic and the Universe of Newton," which examines Hume's criticism of and indebtedness to the spatiotemporal concepts of Newtonian physics.

Two thought-provoking essays deal with wider aspects of the Enlightenment. In its attempt to answer the fundamental question, "Why was the Enlightenment?" Peter Gay's stimulating contribution decisively rejects any narrow sociological explanation in terms of the influence of a single class—the bourgeoisie—and stresses the complexity of the philosophes' reactions to the world and culture of their time. Roland Mortier also answers the question his title poses—"Sensibility, Neoclassicism or Preromanticism?"—by pointing out that this question is not a matter of simple and distinct concepts but of notions which are in many ways inseparable and complementary, even when apparently opposed.

All readers interested in the eighteenth century will find something worthwhile in this volume, which is aptly introduced by a tribute to Wilson from his former colleague, John Sloan Dickey.

RONALD GRIMSLEY
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GEORGE RUDÉ. *Debate on Europe, 1815-1850*. New York: Harper and Row. 1972. Pp. xxiii, 277. Cloth \$7.50, paper \$3.95.

George Rudé's intention is as nebulous as the title of his book. There is no "debate on Europe" except for the chapter on the Peace Settlement. Instead, he gives brief historiographical treatments of standard topics in the history of Western and, occasionally, Central Europe: Restoration, Risorgimento, liberalism, nationalism, Romanticism, socialism, the Industrial Revolution, the Revolutions of 1830 and 1848. Believing that "controversy lies at the heart of all historical inquiry," Rudé stages debates—"the successive and varying opinions of historians"—on each topic among conservative, liberal, and Marxist historians of the nineteenth and twentieth centuries. In a book of 266 pages, this often means perfunctory confrontation. In spite of his effort to define his terms, Rudé's classification of historians is not analytical but too often pigeonhole labelling. For example, Rudé calls Albert Sorel a liberal because he favors the French Revolution, but does not question the liberalism of his pro-Napoleonic stance in the pas-

sage cited. In the very issues where the labels might seem more appropriate—economic growth, urbanization, the standard-of-living controversy—Rudé admits that the labels often do not fit. This must be vexatious for the undergraduate or graduate student cramming for “Prelims,” for whom his book seems designed.

Rudé does not cover adequately monographs and substantial syntheses but relies heavily for his citations on surveys and compilations such as the Heath series. Many historians one would expect to find are omitted: the old masters, Ranke and Macaulay; more recent historians such as Ritter, Holborn, Joll, Schnabel, Krieger, and Beik; others are dismissed with one or two references: Gordon Wright, Duroselle, Heaton, Rothfels, David Thomson, Brogan, Priscilla Robertson, Landes, to mention only a few. Considerations of space are evidently not the sole reason, since E. J. Hobsbawm is referred to as an authority on all topics: he is cited nearly 50 times (not all listed in the index) in text or footnotes, including about 20 pages of laudatory exposition or quotation. Only Marx and Engels rate as much attention. An inordinate amount of space is devoted to E.P. Thompson and the Hammonds.

Whether intentionally or not, the book serves as an unproclaimed manifesto of the new orthodoxy of the “newer social history,” or “history from below” of Hobsbawm, Thompson, and Rudé himself, and he devotes roughly a third of the book to this wave of the future. One is puzzled that he mentions, only to drop, a crucial issue facing social historians: the debate of the “impressionists” like Hobsbawm versus the quantitative historians and demographers.

EVALYN A. CLARK
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CHARLES TILLY *et al.* *The Rebellious Century, 1830-1930*. Cambridge, Mass.: Harvard University Press. 1975. Pp. xi, 354. \$15.00.

The Tillys begin this book by informing us that they have “reduced the unending richness” of all forms of collective violence to statistics: participants per 100,000 population (p. 16). Their admission provides a fitting epitaph for this fundamentally unsatisfactory book. Not only do they display an appalling disregard for the subtleties and complexities of history, but they fail even to measure up to their own standards. Thus the authors scavenge for evidence and misrepresent major developments.

Charles Tilly writes on France, Louise Tilly on Italy, and Richard Tilly on Germany. Charles’ chapter, the only one propped up by significant statistical scaffolding, reasonably argues that no explicit

deterministic relationships exist between economic phenomena such as price and wage fluctuations and the incidence of social violence. He believes that violence expresses struggles for political power. By taking that proposition to extreme lengths, however, and excluding a broad range of historical experience, he gets himself immediately into trouble. The statistics on violence that he cites either demonstrate the obvious, that 1848 witnessed more violent events than any other year and that police repression in the 1850s kept violence to a minimum, or they refute his own argument by showing that political upheaval occurred precisely at those times when social tensions reached intolerable levels (the late 1840s, 1897-1910) or when the entire nation was gripped by a general economic crisis (the 1930s). Charles also fails to take full advantage of his chosen approach. He would be less confident about the autonomy of politics were he to consider such measurable quantities as work speed-ups, the relation of wages to profits, the rate of the concentration of capital, and the subordination of independent craftsmen to capital—to suggest only a few.

The confusion that remains may result from Tilly’s refusal to permit history’s actors to intrude onto his statistical abstraction. Thus, he casually remarks that he will not discuss the Paris Commune, because his model has no room for the massive arrests that took place after the Commune was suppressed and “therefore did not enter into our statistics.” He judges that 1870-71 remains a “doubtful case in the correlation between extent of violence and extent of political change (p. 60).” One might ignore such a *gaffe* were it not indicative of the entire book’s tendency to trivialize momentous episodes of social struggle in the name of statistical neatness. Elsewhere, Charles concludes a discussion of the street battles of February 1934, with the astonishing assertion that they originated in “nonviolent” efforts to take power (p. 52). Are we to suppose that the demonstrators (Camelots du Roi? Communists?) expected that they need only huff and puff and blow the government down? Elsewhere he reproduces from Pierre Pierard some graffiti from Lille in 1868. One reads: “Nous ne voulons pas nous laissés mourir ou mangé par les Anglais” (p. 20). He does not pause to consider the curious spectacle of workers taking up the protectionist slogans of their bosses.

Neither Louise nor Richard Tilly can match their collaborator’s statistics. Hence comparisons, which the authors attempt, prove fruitless. Louise gets her Italian history mostly straight with the aid of standard secondary accounts. In her effort to homogenize very different events (a goal that all three authors strenuously pursue), however, she resorts to a sociological evasion. Faced with the

task of making sense of the special circumstances surrounding urban and rural insurrections over a century, to say nothing of the Fascist coup, she ascribes much of the violence to a pervasive "localism" that pitted regions against each other and against the national government. Her theoretical foundation for this interpretation rests on several studies of mid-twentieth-century Italian society. Irrelevant comparisons abound. Louise mentions that the incidence of collective violence in Italy does not approximate the "enormous frequencies of death, damage, and destruction" in present-day India (p. 123). Surely the peasants of the *mezzogiorno* and the laborers in the rice fields of Lombardy would have rejoiced to learn that their miseries could not compare with those of the street beggars of Calcutta.

Richard Tilly finds for Germany that "occupational groups" (labor unions) increased their political action in direct proportion to the decline of "simple crowd" violence. He explains this by reference to the disappearance of "communal" interests, which are replaced by workers' experience in productive relationships, brought on, he says, by "specialization (p. 226)." Does he mean that non-specialized (unskilled?) labor did not experience and identify exploitive relationships? Does he mean that unskilled labor vanished from the labor market by the end of the nineteenth century? He cannot mean that early capitalist production did not disrupt the local market, for he asserts (p. 201) that concentration on producers' goods in the 1840s wreaked havoc in traditional society. Richard notes the progressive replacement of soldiers by police to put down demonstrations. He says that "even as late as 1910" generals had "serious misgivings" about using soldiers to maintain order (p. 219). The significance of why, as war clouds gathered, German generals would be reluctant to order German soldiers to shoot down German civilians escapes him.

Language provides a smokescreen behind which the Tillys perform their sociometrical tricks. Charles characterizes the social struggles of the 1840s and the 1930s in France as "energy flowing into collective violence and . . . particles in which energy was emitted (p. 70)." Louise speaks of the "demobilization" (p. 93) of workers following the Fascist victory, by which she seems to mean the smashing of trade unions by Fascist *squadre* and the police. Richard uses similar language. "Modernization" possesses "significant power to influence collective violence (p. 236)." Vormärz Germany experienced "social distress (p. 208)." Crime, violence, and political radicalism may be symptoms of "social breakdown" (p. 215).

Finally, the authors assert that they are Marxists—albeit modernized. This is false on two

counts. First, they ignore the fact that Marx's theoretical and historical analyses turn on the dynamics of class relationships. Only within that framework do political struggles of this kind make sense. The Tillys substitute the meaningless notion of "solidary groups" [*sic*] for the concept of class. Marx, they tell us, emphasized the impact of industrialization on the political process. Actually, he did not. He emphasized the progressive development of capitalist social production and the political stresses that accompanied it. That is clear in a passage from *The Class Struggles in France* that the Tillys themselves quote to prove the opposite of what it says (p. 272). Second, they make Marx an accomplice in sentimentalism and spurious radicalism. "Our problem today," they write, "is to transform a single world of war and injustice into a single world of peace and justice (p. 299)." Such was, they suggest, Marx's vision. Perhaps—but he would have been quick to recognize that he and they did not belong on the same side of the barricades.

SANDFORD ELWITT
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La guerre en Méditerranée, 1939-1945: Actes du Colloque International tenu à Paris du 8 au 11 avril 1969. (Comité d'Histoire de la 2^e Guerre Mondiale.) Paris: Éditions du Centre National de la Recherche Scientifique. 1971. Pp. 792. 96.80 fr.

The meetings in Paris, of which this book is the verbatim record, convened more than 160 persons from more than 20 countries under the auspices of the international Comité d'Histoire de la Deuxième Guerre Mondiale. Forty-eight previously prepared papers and critiques of papers were read. They were interspersed with responses to the critiques and were followed by one session devoted to general discussion. A sedulous effort to maintain scholarly objectivity and an international approach was in the main successful. The four sessions with central themes dealt with general problems of policy and strategy, conduct of war and military operations, international relations, and nationalism in northern Africa and the Levant. The bits and pieces, when combined in one book, provide both substance and guidance to further investigation.

Most aspects of the Second World War in the Mediterranean region are either mentioned or more fully treated: the divergences in the war aims of the members of each coalition, the erosion of the original Axis concept of two parallel wars, Hitler's need of French collaboration in view of British obduracy, the usefulness to both sides of a neutral Spain, Turkey's tightrope act as a neutral, the consequences of changes in the membership of

each coalition as the Soviet Union was expelled from its association with the Axis powers and as Italy was pulled away from its ties with Nazi Germany, the value of the contribution to victory made by the Mediterranean campaigns in view of Soviet successes elsewhere, the failure to subdue Malta, logistic support of the German-Italian *Panzer Armee Afrika*, the political motivation for operations in southeastern Europe undertaken or proposed by the British, the impact of French defeat, division, and dependence on others for modern arms upon Anglo-American relations with French leaders, Stalin's role in the decision to invade France from the south as well as the west, contributions by organized Resistance units to Allied successes in southern France and northern Italy. The tactics of campaigns claim little notice, however, except for a succinct account by Colonel Le Goyet, a French Army historian, of the outstanding performance by French forces in Italy.

Certain participants in the colloquy were already well-known specialists in the history of World War II; others, less prominent, also dealt effectively with primary matters of fact and interpretation. A few persons were able to provide data from their first-hand, wartime experiences. One was René Massigli, the diplomat. Others were French officers involved in the French Forces of the Interior and Italians who had opposed the Fascists before the armistice of Cassibile and the Germans until the war ended. While more than half of those attending were French, historians came from communist countries of eastern Europe, from Israel, Turkey, Greece, Austria, both Germanys, the Low Countries, Italy, the United Kingdom, Switzerland, and the United States.

The significance of the colloquy is dual; it is a stride toward a sound history of the war; it is also an episode in the history of history.

GEORGE F. HOWE
Harrisville, N.H.

DONALD CAMERON WATT. *Too Serious a Business: European Armed Forces and the Approach to the Second World War*. Berkeley and Los Angeles: University of California Press. 1975. Pp. 200. \$8.50.

Georges Clemenceau's dictum that "war is too serious a business to be left to soldiers" provides the title and one theme for this ambitious study of the background to World War Two. Donald Cameron Watt examines the way military leaders in Britain, France, Germany, and Italy carried out their multiple assignments in the interwar period. He concludes that they all failed to some extent to come to terms with the new technology of war and produce realistic strategic doctrines. This failure, he argues, stemmed from the structural defects

inherent in the internal divisions of their national societies. Military men confronted the prospect of war in 1939 with a gloomy pessimism unlike the prevailing attitude of 1914. When the war came in spite of their warnings, it was—until 1941, at least—a civil war which reflected the breakdown within Europe.

This book originated as a series of lectures at Cambridge University in 1973. It is brief and suggestive rather than conclusive at many points. In his use of the concept of European civil war, for instance, or in supporting his contention that "despair or at least despondency to the point of defeatism" was common to the General Staffs of possibly three of the four powers at war in 1939, Watt will leave some readers anxious for more evidence. He succeeds brilliantly, however, in synthesizing a wide range of secondary and primary sources and presenting an original and useful interpretation of the origins of the Second World War. This book should stimulate scholars to move away from traditional diplomatic and military history into a broader consideration of how strategy relates to politics and where the European system failed.

DONALD S. BIRN
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Albany

LIVIA ROTHKIRCHEN, editor. *Yad Vashem Studies on the European Jewish Catastrophe and Resistance*. (Yad Vashem Studies, 10.) Jerusalem: Yad Vashem; distrib. by Ktav Publishing House, New York. 1974. Pp. 326. \$15.00.

This is the tenth volume, besides its bibliographic series and documentary texts, that Yad Vashem, the Martyrs' and Heroes' Remembrance Authority established by the Israeli government, has published since 1957. It deals primarily with the Holocaust, the term now widely used to describe the destruction of the Jews by the Germans during the Second World War. The steady accumulation of Yad Vashem's studies and their improved caliber, especially under Livia Rothkirchen's editorship, have produced a useful, if undistinguished, corpus of historical papers on the Holocaust.

Three papers in the current volume are devoted to the problems and politics of Jewish leaders under German occupation in Holland (by Joseph Michman), in Hungary (by Randolph L. Brahm), and in Germany 1933-38 (by Abraham Margalit). Based mainly on unpublished sources, these three papers provide some new and interesting data, but they fail to provide guidelines or establish criteria for using evidence of the involved parties. Since the personal bias and political partisanship of eyewitnesses and survivors customarily

warp the historical record, the historian is obliged to treat these sources critically and, if possible, rectify the distortions.

Leni Yahil, author of the splendid *Rescue of Danish Jewry*, has brought together eleven selected documents from the British Foreign Office and Cabinet concerning the illegal immigration to Palestine 1939-40. Her historical introduction is marred, however, by ideological bias.

Martin and Eva Kolinsky, in "The Treatment of the Holocaust in West German Textbooks," examine twenty-five elementary and secondary textbooks of postwar Germany in light of the country's educational policy and objectives. The authors submit a variety of topics (National Socialism, the Nazis, anti-Jewish policies and programs, the destruction of the Jews, German concepts of Jews, German attitudes toward war crimes) to close critical analysis and subtle textual explication. They conclude that, with the notable exception of Hannah Vogt's *Schuld oder Verhängnis?* (published in English as *The Burden of Guilt*), the textbook treatment of the Holocaust in West Germany is characterized by neglect and evasion.

LUCY S. DAWIDOWICZ
Yeshiva University

GEORGE CLINCH. *English Costume: From Prehistoric Times to the Eighteenth Century*. Totowa, N.J.: Rowman and Littlefield. 1975. Pp. xxii, 295, 131 illustrations. \$14.50.

G. T. SALUSBURY-JONES. *Street Life in Medieval England*. Totowa, N.J.: Rowman and Littlefield. 1975. Pp. 213. \$12.75.

Here are two examples of old-fashioned social history: no four-cell tables, no chi-square correlations, no attempt to relate the subject to social and institutional structure. These reprints have a mean age of 51 years (though Salusbury-Jones did a 1948 re-edition wherein he drew some parallels between life in the years of World War II and the street-life milieu of the later Middle Ages). Both are pleasant and informative books from a long time ago. If neither reflects great analysis or deep conceptualization, they do show a sympathetic eye for some basic if prosaic aspects of preindustrial life.

Clinch's book is more a work of reference than a narrative. He moves chronologically, in short chapters, from prehistoric costume to that of the eighteenth century, and then concludes with chapters on such topics as the garb of medieval times (with items of clothing listed alphabetically), of the church, of the orders of chivalry, etc. This book, with its many illustrations, is an antiquarian handbook that takes its place beside similar works

of the day on church furnishings, monumental brasses, and academic and legal costume. With brasses, effigies, and illuminations as his main sources, Clinch describes the outer garments (though not the underwear) of various social classes through the centuries. He ignores the social and sexual aspects of fashion, the economics of cloth and the clothtrade, and the technological side of the garment industry. He offers a useful handbook to deal with that basic daily question, "What should I wear today?" as asked by millions of people.

Salusbury-Jones' book consists of six essays on various facets of urban social life. Traffic congestion, the hours and enforcement of the curfew, and the lack of bequests to build public latrines are the sort of things he discusses. His main sources are printed municipal and borough records such as the *Calendars of the London Letter Books* and the *Coventry Leet Book*. His social history is of the genre of Salzman's *English Life in the Middle Ages* (1926) and Jusserand's *Wayfaring Life* (3rd edition, 1925). The author conveniently puts together things we would have to dig around for on our own. He tends to take the records at face value, but like Clinch, he offers ready information we do not get, let alone remember, from reading scholarly monographs.

Neither book was reviewed in the *AHR* upon first appearance. Their belated recognition symbolizes an attempt to bridge the gap between books used by theatrical costume- and set-designers and historical novelists, on the one hand, and professional teachers on the other. To still be amusing and informative, and for a wide audience at that, is not a bad fate for a couple of old horses.

JOEL T. ROSENTHAL
State University of New York,
Stony Brook

GLANMOR WILLIAMS. *Glamorgan County History*. Volume IV, *Early Modern Glamorgan: From the Act of Union to the Industrial Revolution*. Cardiff: Glamorgan County History Trust Limited; distrib. by University of Wales Press, Cardiff. 1974. Pp. xviii, 717. £15.

Early Modern Glamorgan, the third volume of the *Glamorgan County History* to appear, carries the account forward from the Act of Union of 1536 to the beginning of the Industrial Revolution. Its ten chapters—backed by genealogical tables, maps, and a wealth of excellent illustrations—trace the economic and social life of Glamorgan; its political, religious, and literary history; and aspects of education in the county during the period 1660-c.1775.

In many ways the early modern period was a golden age in Glamorgan history. Union with Eng-

land marked the end of the turmoil of the Middle Ages and inaugurated an era of growth and prosperity during which many of Glamorgan's leaders ventured with notable success onto the wider stage of English political life. Historians of English politics are already familiar with the achievements of Sir Leoline Jenkins and successive generations of the Herbert family; they may be surprised to discover that these were only part of a galaxy of talented individuals who helped to shape Glamorgan society during this period.

In a corporate work of this nature it is invidious to single out individual contributions. But Penry Williams' essay on the political and administrative history of Glamorgan between 1536 and 1642 deserves to stand as a model for this genre. A masterpiece of clarity and compression, its value is enhanced by the existence of parallel accounts, notably for Wiltshire in the *Victoria History* of that county.

Indeed, it is with the "general" volumes of the *Victoria History* series that this venture seems most likely to be compared. In format they are similar. But freed from inhibiting traditions, this volume of the *Glamorgan History* has achieved a cohesion and clarity which occasionally escapes its English counterpart. Whether multi-volume county histories have become an expensive and expendable luxury is a perennial question. But, at £15 (\$30) for more than 700 excellent pages, *Early Modern Glamorgan* hardly constitutes an overwhelming case for the abolitionists.

J. S. COCKBURN
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College Park

R. W. DUNNING, editor. *A History of the County of Somerset*. Volume 3. (The *Victoria History* of the Counties of England.) New York: Oxford University Press, for the Institute of Historical Research. 1974. Pp. xx, 293. \$77.00.

The appearance of a new volume of the *Victoria History* of the Counties of England for a certain "unmentionable county" in the West of England is an event so rare as to fire the highest expectation in a historian of Somerset. This volume, the third, comes almost two-thirds of a century after the last (vol. 2, 1911), which followed the first volume (1906) by only a few years. The first two volumes constituted the "general" history of the county; the present volume is the first of a series of "topographical" histories of individual hundreds, parishes, and boroughs. This volume, really an entirely new endeavor as far as Somerset is concerned, lives up to expectation. It maintains the highest standard of scholarship, the aim of the original editors of VCH, with the current broad-

ening of concerns and wholehearted acceptance of new facets and methodologies in history that the present general editor, Dr. R. B. Pugh, has brought to the project since assuming its direction in 1949. Perhaps the contrast between the older, "Victorian" scholarship of VCH and the newer "Elizabethan" scholarship of our day is made more vivid by the long lapse of time between this volume and its predecessors. But the contrast is heightened by the meticulous modern scholarship of the editor of volume 3, R. W. Dunning, and his assistant, R. J. E. Bush, two young scholars who are almost entirely responsible for the text. They have executed a remarkable achievement: a comprehensive, chronologically even, accurate, multifaceted, and—perhaps incredible in such a compendium—readable history of four south-central hundreds of the county, Kingsbury East, Pitney, Somerton, and Tintinhull. Pursuant to the new canons of VCH, each parish or town within a hundred is treated uniformly in terms of manors and landholding, economic developments, local government, ecclesiastical history (established church and Nonconformity), education, and charities, with attention paid to historic buildings. The chronological range is truly from prehistory to the present.

Perhaps the principal criticism that can be leveled at the volume is in fact one aimed at the whole endeavor. Somerset desperately needs a new, modern brace of "general" history volumes—those of 1906 and 1911 are hopelessly out of date. The enormous cost of producing—and, at £24, purchasing—a volume of so limited geographic scope is such as to put in question the entire concept of VCH in the 1970s. Had the original objective of writing all the histories of all the counties been accomplished within a few years of the demise of the regal patron, VCH would have been an eternally useful, albeit increasingly dated, recueil for the study of local history. Now the disparity between the new and the old is such as to rob all of an essential unity. One remarks ruefully that the *Victoria History* of the Counties of England has not only outlived Victoria but also the ancient counties. Somerset, along with other ancient counties, has just been chopped out of recognition in the last Tory government's scheme of reorganization of local government. VCH has been fated "to hang / Quite out of fashion, like a rusty mail, / In monumental mockery."

THOMAS G. BARNES
University of California,
Berkeley

WILLIAM J. BARBER. *British Economic Thought and India, 1600-1858: A Study in the History of Develop-*

ment Economics. New York: Clarendon Press of Oxford University Press. 1975. Pp. viii, 243. \$19.25.

This book studies the relationship between the activities of the British East India Company from its founding until the Sepoy Mutiny and the state of British economic thinking. Its contributions lie in the area of the history of economic analysis, and some knowledge of economic theory is needed by the reader if he is to profit from the book.

Since British policy in India was of considerable economic importance and the subject of a great deal of political interest, it is scarcely surprising that most outstanding British economists of the period between 1600 and 1858 became involved with Indian problems. Various economists were Company administrators, apologists, or critics. They sometimes served in more than one of these roles. The list of such economists discussed by Barber is an impressive one: Thomas Mun, Sir James Steuart, Adam Smith, Lord Lauderdale, James and John Stuart Mill, Thomas Malthus, J. R. McCulloch, and Karl Marx. Naturally, the level of intellectual involvement with India was not equally great, but Mun, Steuart, Lauderdale, James Mill, Malthus, and McCulloch all devoted major amounts of time and effort to Indian problems.

Three of the episodes discussed in the book are particularly interesting and informative. The first is Thomas Mun's defense of silver export within the intellectual context of mercantilism. His case rests on specie's being a factor of production in international trade activities.

The second matter concerns the anonymous pamphlet *Considerations Upon the East-India Trade* published in 1701. This remarkable work was a precursor of the doctrines of the classical economists in its analysis of competitive markets. Barber makes a convincing case that this apparently precocious work was really a logical result of the policy discussions then in progress.

The third section concerns the dispute between James Mill and J. R. McCulloch as to which aspects of Ricardo's doctrines should be applied to the governance of India. Mill was obsessed with the theory of differential rent and its implications for the theory of land taxation, while McCulloch stressed the benefits of free markets and the absence of governmental interference.

The book is well written and should be of considerable interest to students of the history of economic analysis.

LARS G. SANDBERG
Ohio State University

CHRISTOPHER HAIGH. *Reformation and Resistance in Tudor Lancashire*. New York: Cambridge University Press. 1975. Pp. xiii, 377. \$23.50.

Lancashire was the most Catholic county in Elizabethan England, but the factors causing this phenomenon have never been fully explored and closely examined. In his well-documented monograph, based largely upon manuscript sources, Christopher Haigh puts forward a feasible explanation. Lancashire was remote, isolated, and backward, especially when compared with southern counties, and its religious history was unique. Medieval heresies may have thrived elsewhere, as Dickens has demonstrated, but not in Lancashire. The unwieldy ecclesiastical structure—with its divided jurisdiction, its exceptionally large parishes, and its unremunerative benefices—was not congenial to either Erasmian or Cromwellian reforms. In fact, both the clergy and the laity resisted the Henrician Reformation, particularly the dissolution of the monasteries. The Edwardian reforms were well received in the southeastern parishes—those closest to Oxford, London, and the outside world—but they were not enforced in the rest of the country. Thus the old faith, revitalized during the reign of Mary, survived almost intact. The Elizabethan Settlement, however, polarized Lancashire society. Attempts to enforce the established religion and eliminate old practices gave rise to recusancy, crypto-Catholics, secret masses, Jesuit missions, flights to Ireland and Douai, and appeals to Rome. These, in turn, evoked a Puritan reaction characterized by sermonizing and witch-hunts. The Established Church, caught in these crosscurrents, proved to be weak and powerless in the struggle. Thus did cohesiveness of an isolated society give way to competition, and Lancashire became the "cockpit of conscience."

Except for repetitious restatements of the thesis, the work is solid and well written. The author has included relevant statistical material and comparisons with parallel studies. A map and a definitive bibliography enhance the value of the book.

VERNON F. SNOW
Syracuse University

L. M. HILL, editor. *The Ancient State Authorities, and Proceedings of the Court of Requests by Sir Julius Caesar*. (Cambridge Studies in English Legal History.) New York: Cambridge University Press. 1975. Pp. xlvii, 281. \$25.00.

The rivalry between the conciliar or prerogative courts and the common-law courts in the late sixteenth and early seventeenth centuries was once thought to be a conflict between "English" common law and "Roman" civil law. W. J. Jones in his work on Elizabethan Chancery demonstrated that this was not so. Rivalry there was, but the cause was more mundane than ideological. Two separate, but not totally distinct, judicial systems competed for fees, fines, influence, and jobs. Long

before Star Chamber and Chancery felt the full force of the common lawyer's assault, the "Poor Man's" Court of Requests came under challenge. Sir Julius Caesar, a Master of Requests and an Admiralty judge, rose to its defense with the publication of his *Ancient State Authoritie*, which is now reprinted for the first time.

Caesar's book serves today as one of our principal sources for the work of the Court and demonstrates the pragmatic nature of the contest between the rival systems. In 1591 Caesar was appointed a Master of Requests, and the court's jurisdiction was challenged in *Locke vs. Parsons* in Common Pleas. Caesar immediately began his compilation of *The Ancient State Authoritie*, using the same methods in defense of Requests that his common-law opponents used in defense of their system—the historical record. Locke and other common lawyers might challenge the *de jure* existence of Requests, but Caesar proved its *de facto* existence. He also asserted that to attack Requests was to attack the jurisdiction of the Council itself—an attack no one was yet ready to make.

L. M. Hill has provided a useful introduction to the book and to the life and career of Caesar. He is honest enough not to inflate the importance of Caesar's work, admitting that the usefulness of the court did more to prolong its life than did the publication of Caesar's defense.

STUART E. PRALL
Queens College,
City University of New York

CLIVE HOLMES. *The Eastern Association in the English Civil War*. New York: Cambridge University Press. 1974. Pp. x, 322. \$17.50.

This excellent book illustrates and explains much more than local history. It establishes several important correctives to the older accounts. Far from exhibiting an ideological homogeneity, the Eastern counties manifested various shades of political and religious opinion on the eve of war. The achievement of unity and commitment required strenuous political activity, both in the counties and at Westminster. Holmes focuses on the activities of the committee of the associated counties of Suffolk, Norfolk, Essex, Cambridge, Lincoln, Hertford, and Huntingdon for the years from 1642 to 1645. Expert analysis of a rich body of records, manuscript as well as printed, elucidates the problem of getting the counties to act together and sheds further light on personnel of local administration and the fiscal innovations of the war. In respect to the last, Holmes has provided the fullest account of the monthly assessment, the most important of the tax measures devised during the revolution. Although enormous sums were raised, a recurrent litany of

fiscal despair runs through the official correspondence, balanced by complaints from Manchester's soldiers regarding their arrears of pay. Holmes gives much attention to the recruiting of the army, modifies often-quoted descriptions of the Eastern Association's cavalry as composed primarily of men of substance, and proves the strong ingredient of tradesmen and apprentices. He explains Cromwell's quarrel with Manchester much more fully than anyone has done and notes the importance of the quarrel in the disputes between the parties in Parliament. On the whole, no other book gives so good a picture of the place of localism in the Civil War.

P. H. HARDACRE
Vanderbilt University

C. D. CHANDAMAN. *The English Public Revenue, 1660-1688*. New York: Oxford University Press. 1975. Pp. xii, 386.

This is an important, but flawed book. The tables which provide detailed information about government revenue, issues, and assignments from 1660 to 1688 will replace the incomplete, misleading, and sometimes inaccurate figures presented by the late William A. Shaw in his introductions to several of the first eight volumes of the *Calendar of Treasury Books* (London, 1904-23). This new set of accounts is accompanied in the extensive appendices by an elaborate explanation of the information which can be extracted accurately from the Exchequer accounts, but readers must understand the explanation before using Chandaman's tables.

The administrative history of the English public revenue is explained in chapters concerning the customs, excise, hearth money, direct taxes, and numerous small and casual branches of revenue. While this book does not supersede the works of E. Hughes and C. A. F. Meekings on various aspects of revenue administration, it is a comprehensive account containing a great deal of previously unpublished information. However, there are errors. A regrettable lapse which might cause the unwary some inconvenience is the use of the Public Record Office classification GD when citing the important Leeds and Shaftesbury manuscripts, a classification which was changed to PRO 30 almost fifteen years ago. Chandaman's interpretation of the political and constitutional ramifications of government revenue is not entirely convincing, and more sophisticated statistical and analytical techniques would have made his arguments more persuasive. But with these limitations in mind, the book must be valued for the important contribution it makes to our knowledge of government revenue and administration. If the book's price does not prevent its purchase, it will serve as the new starting point for further research

in this important area of seventeenth-century English history.

GLENN O. NICHOLS
University of Maryland,
Eastern Shore

LOIS G. SCHWOERER. *"No Standing Armies!" The Antiarmy Ideology in Seventeenth-Century England*. Baltimore: Johns Hopkins University Press. 1974. Pp. x, 210. \$10.00.

Armies, however useful in defending societies, habitually pose a threat to their own civil governments. This fact has been recognized since the earliest years of the modern state, when rulers first began trying to create permanent armies, and representative assemblies stoutly resisted them as a menace to public liberties and an unwarranted burden on national treasuries. In England, geography afforded sufficient security against foreign dangers to give the antiarmy argument unusual weight, and a taste of military rule under Cromwell was equally persuasive. When the peacetime standing army first appeared in England on a regular basis after 1660, therefore, it had to do so furtively, like the Cheshire cat, and it was not until the century's end that it was grudgingly accepted, under parliamentary auspices. The struggle left its marks, and the special character of the Anglo-American tradition of civil-military relations, combining a deeply rooted skepticism about the army with a determination to keep it limited in size and clearly subordinate to the civil power, had been firmly established.

Lois Schwoerer's monograph examines the body of pamphlet literature and parliamentary debate about the creation and control of the standing army and the regulation of the militia in the crucial period after 1628. She shows how long-lived the seventeenth-century arguments were, frequently plagiarized well into the following century, but suggests that after 1660 antiarmy prejudice was often used in partisan fashion merely as a convenient stick to beat ministries.

Doubtless this work will be useful primarily to specialists in later Stuart politics where its main strengths lie, but it is also a piece of the important but long neglected enquiry into the origins of the liberal tradition of civil-military relations. The relevance of that larger study to our own age ought to be clear.

S. J. STEARNS
Richmond College,
City University of New York

EDWARD B. POWLEY. *The Naval Side of King William's War, 16th/26th November 1688-14th June 1690*. Fore-

word by SIR ARTHUR BRYANT. [Hamden, Conn.: Archon Books. 1972. Pp. 392. \$11.75.

This book should be entitled *William III's Navy and the Struggle in Ireland*, because that describes its main contribution, but it is not easy to construct a title for a book so disjointed and amorphous. The work is ostensibly a detailed naval history from November 1688 to June 14, 1690, a fortnight before Beachy Head. That battle was to the English navy what Pearl Harbor was to ours, and England has probably never been so close to successful invasion in modern times. Yet Powley's treatment of its antecedents is, when compared with the rest of the book, hurried and sketchy—seemingly unfinished. John Ehrman's chapter in *The Navy in the War of William III* remains the best account, though we still do not know the precise role of financial turmoil, victualling collapse, and sickness among the seamen in the mobilization effort of 1690.

The book is not, as Sir Arthur Bryant asserts in a foreword, "a work of scholarship of the highest quality"; rather it might be compared with an ordinary Ph.D. dissertation—impressive in research and detail, but atrociously conceived and written. The organizational scheme resembles that of the Anglo-Saxon Chronicle; the style wanders between archaism and modern colloquialism; and the misleading syntax offers challenges to experts and laymen alike. The fact that the author died before the book was prepared for the press may account for some of the flaws. Nevertheless, since its only value lies in the richly detailed information (especially on Irish campaigns) that it provides for specialists, from not only obvious materials but also ships' logs, French archives, and other recon-dite sources, a microfilmed version would have served well enough.

DANIEL A. BAUGH
Cornell University

HARRY CARTER. *A History of the Oxford University Press*. Volume 1, *To the Year 1780*. With an appendix listing the titles of books printed there, 1690-1780. New York: Oxford University Press. 1975. Pp. xxxi, 640. \$48.00.

Georges Clemenceau's view that war is "too serious a business to be left to soldiers" is now a commonplace, often applied to other, less bellicose activities. Thus readers may be surprised to discover that this impressive history was written not by a historian but by "one who is primarily a printer." Whether they will be disappointed depends on their expectations. Because the history of any distinguished press is inextricably linked to intellectual history, it cannot properly be confined to administration, as Harry Carter fully appreciates. The present volume, intended to be the first

of two or three, stands on the archives of the press, university, and Public Record Office and on other diverse sources of information. The approach, informal in tone, is bibliographical, in part because of the author's competence and in part because a collaborative history of the university is now in progress.

Ten introductory chapters describe the character and career of presses at Oxford up to 1690, the mounting emphasis on the need for a university press (no German or French university was without one), and the great influence, first of Laud, then of John Fell in this direction. Fell actually established a press for the printing of learned books, of which Carter supplies brief descriptions, including typesets and other technical matters. Four years after Fell's death in 1686 his press became in effect the Oxford University Press, as he had provided in his will. During the next sixty-five years the Press had an uneven history, owing to incompetent administration. Happily in 1755 the half-century of "unsalutary neglect" ended with the appointment of Blackstone as delegate. Thenceforth, in every respect—technical, administrative, and intellectual—the Press fulfilled the desires of Laud and Fell.

Rounding out his narrative, Carter has appended a two-hundred page list of titles printed at the Press, 1690–1780, English Bibles, Testaments, and Books of Common Prayer not included. Here one may trace, in part, the intellectual history of eighteenth-century England. From the outset Fell's ideal of printing only learned books was eroded by the appearance of pieces of the moment, the best known being Clarendon's *History of the Rebellion* (1702). Throughout the entire period Latin texts had a large place, though the source might be Greek or Arabic. The first year saw a Euclid, Suetonius, and Xenophon, the latter outshining any other historian. As might be expected, sermons, homilies, and university proceedings abounded. History and geography got persistent attention, as did grammar and, increasingly, science. The threat of Dissent was always manifest. Though by no means conforming to a pattern, the proportion of English titles steadily increased. The number of titles varied considerably from year to year—seven in 1724 and 1779 and thirty in 1755, with an average of about fifteen.

The volume is enhanced by informing illustrations, and this reviewer hopes that its successors will soon follow.

CHARLES F. MULLETT
University of Missouri,
Columbia

JULIAN GWYN. *The Enterprising Admiral: The Personal Fortune of Admiral Sir Peter Warren*. Montreal:

McGill-Queen's University Press. 1974. Pp. xvi, 292. \$16.00 Canadian.

This is a study of the opportunities that war presented to one Royal Navy officer for making himself wealthy. The officer was Peter Warren, third son of an Irish Jacobite, who saw his best prospects in joining the navy as an ordinary seaman in 1716. Through luck and patronage he rose in eleven years to the rank of captain. Through luck and a good marriage he acquired social status and business contacts in colonial New York. But it was war that made Peter Warren.

The war, King George's War in the English colonies, gave Warren the rank of Vice Admiral and wealth commensurate. He attained both largely through taking numerous "prizes-of-war"—French merchant ships and naval vessels in the waters of the West Indies, off the coasts of North America, and especially at Louisbourg, Nova Scotia, when that fortress fell to forces under his command in 1745. Fame and status compounded fortune, and his family, deprived of its father by Warren's early death in 1752, estimated its wealth at about £250,000 sterling by the end of the century. Indeed, "the war had made Warren a man rich in his own right" (p. 15). But the story is what he did with his money.

Julian Gwyn's book establishes in detail the origin and development of each of the several components of the Warren family fortune. We learn of Peter Warren's buying and selling of land in New York, Ireland, and England; of his monies lent out and repaid with interest in the New World and the Old; and of his investments in the London securities market. Gwyn is convincing in showing Warren to have been "a keen businessman" (p. 202), although "the impression [of Warren] . . . as an international financier" (p. 199) is clearer to the eyes of his biographer than it is to this reader. What Warren did do was to recognize and take advantage of good investments in several different places. War provided him the means by giving him the money; Warren's own talent as wise investor helped him turn a windfall into a fortune. "Enterprising" is the key word.

JOHN J. MCCUSKER
University of Maryland,
College Park

ROGER ANSTEY. *The Atlantic Slave Trade and British Abolition, 1760–1810*. Atlantic Highlands, N.J.: Humanities Press. 1975. Pp. xxiv, 456. \$22.50.

The publication in 1944 of Eric Williams' *Capitalism and Slavery* dramatically disturbed accepted notions about the operation of the trade in African slaves by Europeans and the process by which this trade was abolished during the nineteenth cen-

tury. Although Williams was not the first to point out the relationship between slavery and the international economy—C. L. R. James had made the same essential argument in *The Black Jacobins* in 1938—his provocative style, his slightly polemical posture, and his timing all contributed to establish his work as a point of departure for new thinking on an old subject. Williams' work, after all, coincided with two seminal publications in American historiography: Gunnar Myrdal, *An American Dilemma* (1944), and Frank Tannenbaum, *Slave and Citizen* (1946). Williams' attack on British altruism—conventionally called humanitarianism—was revolutionary and invited strong counterattack. Most authors, however, were more interested in attacking the author or in refuting his thesis that abolition was essentially an economic gesture, than in trying to understand the subject. In this remarkably thoughtful and indefatigably well-researched book Roger Anstey has given a thorough examination of the history of a vital part of the British slave trade, reviewed the process of abolition, and substantially demolished the foundation of the Williams thesis.

The work is divided into four related sections. Part One deals with the slave trade and its profitability between 1761 and 1810, as well as the possible impact on some Atlantic African societies. Part Two examines the general intellectual milieu of the eighteenth century, with particular reference to theological, reformist, and anti-slavery thought. Part Three focuses on English evangelicalism, relating its substance, evolution, associations, and relationship to Quaker activity on both sides of the Atlantic. Part Four meticulously analyzes the parliamentary process of abolition, with considerable detail on the political sophistication of those who espoused the cause of anti-slavery.

The most memorable, and perhaps the most significant section is that dealing with the economics of the slave trade. Anstey considerably expands his earlier published salvoes attacking the notion that the slave trade was an enormously profitable enterprise. He concludes that the average annual return for British slavers was about 10 percent, for French slavers about 7 percent, and about 2 percent for Dutch slavers. This relatively slim and highly inconsistent profit margin, he believes, was totally inadequate to foster significant industrial growth. Indeed, the estimated annual profit of the trade—a gross of about £200,000—would have contributed only about one-eleventh of one percent of the total national industrial investment required to launch and perpetuate the Industrial Revolution. Nevertheless, he concedes that in specific industries, such as cotton manufacture, and in specific localities such as the Liverpool hinterland, the impact of slave-trade profits could have been "meaningful."

Anstey does not attempt to calculate the social cost of slavery to the participating African communities. Instead, he limits his investigation to regional economics and demography. He finds that larger political groupings tended to benefit at the expense of smaller, segmented societies, but that several factors contributed to economic success in any particular region. The operation of the trade was a complicated business for both European and African merchant. The cost in manpower was probably negligible in the Senegal-Biafra slave-trading zone, but considerably higher in the Congo-Angola region.

The politics of abolition were, according to Anstey, fundamentally caught up in the changing ideological and theological attitudes of the later eighteenth century. The most essential aspect of this was the apparent incompatibility of slavery and liberty and the fusion of ideology and theology which lay at the root of Evangelical success. Nowhere else is the political astuteness of Quakers and abolitionists treated more clearly and more succinctly than in this work. Anstey argues convincingly that the abolitionists initiated political lobbying and exploited every opportunity from the war with France to the death of Fox to infuse some morality to national action. And by subtly separating abolition of the slave trade from a general acceptance of institutional reform, they ironically opened the floodgates for a century of reform in British institutions and British society.

Some aspects of Anstey's work are debatable. Terms such as "triangular trade" and "Negro" (as a synonym for African) will undoubtedly remain controversial. But overall this is a stimulating complement to the variety of new works, some of which are themselves brilliant, which have dealt with the Atlantic World.

FRANKLIN W. KNIGHT
Johns Hopkins University

M. L. CLARKE. *Paley: Evidences for the Man*. [Toronto:] University of Toronto Press. 1974. Pp. viii, 161. \$10.00.

In this unpretentious monograph, M. L. Clarke examines the life and writings of the eighteenth-century theologian and moralist, William Paley, archdeacon of Carlisle and, in his own time, an author of enormous reputation. Clarke devotes the first half of his study to Paley's rather uneventful rise to fame, from his early days in Yorkshire, through his successful career at Cambridge, to his final years as a comfortably affluent member of the Established Church. Paley excelled as an undergraduate at Christ's College and became one of the university's most popular teachers. His natural informality and bucolic wit charmed his students, and his lectures revealed the same admirable lu-

city and penetrating common sense that characterized his later efforts as a writer. Clarke spends the last half of his study recapitulating these now almost forgotten works. In his *Principles of Moral and Political Philosophy*, Paley advocated a policy of utilitarianism that was more religious and less radical than that of his contemporary, Bentham. In his theological works, Paley distilled the ideas of his eighteenth-century predecessors into a system of natural and revealed religion that earned him wide respect among his Christian readers. Indeed, although the Darwinian revolution would eventually sweep away Paley's mechanistic view of nature embodied in his *Natural Theology*, the *Evidences of Christianity* would be an official part of the Cambridge curriculum until 1920.

Clarke's biographical chapters draw upon an impressive range of sources and, through a generous use of anecdote, convey the engaging personality of this neglected divine. Regrettably, the chapters on Paley's thought prove less valuable. Clarke rarely asks interesting questions and, as a result, never really grasps the underlying coherence of Paley's works or explores satisfactorily the complex intellectual traditions which shaped his thinking.

D. L. LEMAHIEU
Lake Forest College

MALCOLM I. THOMIS. *The Town Labourer and the Industrial Revolution*. (Studies in Economic and Social History.) New York: Barnes and Noble. 1975. Pp. 247. \$11.00.

The Town Labourer by John and Barbara Hammond was almost completely written before the First World War. Although there is a whole library of later literature on the subject of the early Industrial Revolution in England, the Hammonds' book remains in print and on reading lists for courses in British history and modern European economic and social history because it is the classic statement of the opinion that industrial change was a traumatic experience for the workers. It is this view that Malcolm I. Thomis seeks to correct.

Some sophisticated students might consider Thomis' efforts unnecessary after the achievements of Sir John Clapham, T. S. Ashton, and R. M. Hartwell. There is very little original that this book (researched mostly in secondary sources) can offer by way of a contribution to the "standard of living" debate. The author is judicious, however, and the evidence which he presents is interesting. The book might have been even better if Thomis had not focused so much on the Hammonds to the extent of quoting passages from *The Town Labourer* as chapter mottoes; much of his argument is obvious in view of the contributions of several generations of researchers.

This book is a useful addition to debate on a survey-course level, but scholars will find the arguments familiar. Since the appeal of the Hammonds has always been to the readers' emotions it is questionable whether the reasonable weighing of evidence by Thomis will change the opinions of those undergraduates whose minds on this subject were made up in secondary school. It is hoped that Thomis' book will be widely read in teacher-training institutions on both sides of the Atlantic.

JOHN W. OSBORNE
Rutgers University,
New Brunswick

KEITH BURGESS. *The Origins of British Industrial Relations: The Nineteenth-Century Experience*. (Croom Helm Social History Series.) Totowa, N. J.: Rowman and Littlefield. 1975. Pp. xiii, 331. \$19.50.

ARTHUR J. TAYLOR, edited and with an introduction by. *The Standard of Living in Britain in the Industrial Revolution*. (Debates in Economic History.) London: Methuen and Company; distrib. by Barnes and Noble Books, New York. 1975. Pp. lv, 216. Cloth \$18.50, paper \$10.00.

These volumes are testimony to the persistence in the profession of the narrow conceptual frameworks of previous generations. Ironically, Keith Burgess claims as "the most important justification" of his study "the desire if not obsession in every generation of historical writing [*sic*] to reinterpret the past in a way that seems relevant to contemporaries" (p. i). His thesis, however, exemplifies the old narrowness:

What is clear is that the special socio-economic characteristics of the engineering, building, coal, and cotton industries in the last quarter of the nineteenth century constituted a situation which made possible the resolution of conflict between employers and employed. Formalized bargaining procedures shifted attention away from the contradiction between legal fiction and social reality inherent in the wage contract, and in masking the disparity between the buyers and sellers of labour contributed to the survival of capitalist relations of production in British society (p. xi).

The assumption which makes his discussion seem so familiar is that industrial relations are to be understood only by looking at what happens in the work place. If modern historians have advanced in the study of the relations into which working people enter, it is in the understanding that what happens at work is most fruitfully examined in the context of the wider political and social relations of the class. So narrow is Burgess' focus that he writes at several points about "the improved bargaining strength of labour after 1850" (p. 306), when many historians would argue that

the class was in a very weak position with any chance of mounting a challenge to capitalism as dead as the Chartist movement. The origins of British industrial relations missing from this study extend beyond working- and middle-class politics, consciousness, and social relations to include the process by which laboring people were stripped of their traditional defenses, craft skills, and—in many cases—self-respect. Missing also are such aspects as the use of violence, intimidation, and the language of menace by both employers and workers.

In looking at the details of union formation and collective bargaining settlements, Burgess has very ably chronicled the occasions essential to maintain industrial peace; in describing the ways managers co-opted working-class leaders to keep the employees in line, he has found a necessary condition for that peace in a free society. His focus is too narrow, however, to come to grips with the causes. This is not the new history of the origins of British industrial relations but rather a very good but incomplete narrative history of the origins of some of the institutional forms which governed those relations in the last half of the nineteenth century.

The debate about the standard of living is also narrow in focus, but it is beset by further difficulties: the lack of data to support any firm conclusions at this time; the lack of agreement about which workers, regions, and years are under discussion; and the lack of any meaningful relation to the satisfactions and happiness of British people of the period. The issues are ideological, and the results can be measured quantitatively in dozens of footnotes and thousands of paperbacks sold to students. The seven essays in Arthur J. Taylor's book were published between 1936 and 1966, and all are readily available. The new material includes an extension of Taylor's 1960 article describing the debate, together with contributions from E. J. Hobsbawm and R. M. Hartwell (with S. Engerman). The new material would have fit admirably into an article for the *Economic History Review*, where it belongs in the interests of scholarship and economy. There is no reason for libraries to pay \$18.50 for the hardback version of this book, nor should students be asked to pay \$10 (only £2.90 in the U.K.) for a paperback. Publishers have been given to complaining about the "failure of the market," but they must bear the responsibility for failing those of us who form the market by a spate of unnecessary books at grossly inflated prices.

THOMAS MILTON KEMNITZ
Barrington, N. H.

ALAN ARMSTRONG. *Stability and Change in an English County Town: A Social Study of York, 1801-51*. New

York: Cambridge University Press. 1974. Pp. xxi, 254. \$15.50.

Specialists in urban history or population studies will welcome this revised version of Alan Armstrong's thesis on York, especially as this famous county town may represent an international type (cf. Viennot on Dijon) and not merely a neglected English one.

Two chapters are devoted to the social and economic background, then intelligently related to four chapters on facets of York demography in the first half of the nineteenth century. Some may find the former thin, but there are discussions of interest, notably the attempt to apply Rowntree's concepts to the 1840s. The second part is, as Armstrong admits, sometimes a matter of "fragile conclusions" resting on "heroic assumptions." In the absence of access to the original civil registration returns, it could hardly be otherwise. The principal sources are the Census Enumerators' Books for 1841 and 1851 supplemented by (above average) ecclesiastical records, poor-law documents, and a contemporary "Sanatory" table, which leads to a charming cross-tabulation of parish mortality with mean altitude. Do ratebooks or electoral registers survive?

Within the limitations of the basic statistics Armstrong demonstrates agility and balance. His more interesting arguments include: York no less than the industrial towns was predominantly composed of immigrants from the surrounding countryside; mortality rates in the 1840s were comparatively high, indicating that urbanization itself brought serious health problems; the higher classes were already successfully limiting their family size; and superficial similarities in household size between pre-Industrial and mid-nineteenth-century communities can conceal structural changes, particularly a rise in the proportion of kin and lodgers at the expense of servants.

Armstrong humbly forestalls some methodological criticism and rightly underlines the need for more advanced statistical manipulation as well as for guidance from the social geographers, but is perhaps less convincing in defending himself against those skeptical of the new social history. Anderson is the surer standard-bearer.

T. J. NOSSITER
London School of Economics

J. P. D. DUNBABIN. *Rural Discontent in Nineteenth-Century Britain*. New York: Holmes and Meier Publishers. 1975. Pp. 320. \$19.50.

This book serves its theme by assembling seven detailed monographic studies on radicalism in

rural East Anglia, agricultural unions in Oxfordshire, Scottish farm servants, tenant rights, the crofters' "Land War," and the Welsh "Tithe War." The first two are by A. J. Peacock and Pamela Horn, the rest by J. P. D. Dunbabin. Although the whole century is encompassed, and all of Great Britain, there is no pretence of full coverage. The book succeeds because of some authoritative chapters in which Dunbabin stitches the regional studies together and interprets them. It thus manages both to break new ground and to offer insights that can come only from a broad perspective.

One of Dunbabin's findings is that, throughout the century, supply and demand triumphed over protest and organization; in the eastern counties, for example, where laborers' unions became strongest, wages rose least. Although the supply-demand situation for rural labor strongly favored the employer before 1850—farmers often spread work around the parish as best they could—it was mixed thereafter. Rural protests in the earlier period were basically distress signals; they commonly occurred in hard times, were disorganized, pitiful, sometimes menacing and revengeful, and failed either to evoke a sympathetic response from the national government or to bring tangible benefits to the laborers. After 1850 organization was the rule, and successes came when times were favorable or political support could be obtained. Dunbabin insists that in the late nineteenth century national politics were far from irrelevant to the fate of ordinary country folk. Noting this, as well as the social significance of the new county councils, one can easily grasp why Gladstonian Liberalism established its hold on these people. Except in the case of the Welsh "Tithe War," British rural movements had narrow economic aims, though they did not lack ideology. A sense of righteousness gave spirit to these causes, and it derived much more from the Bible than from socialism or anything else.

DANIEL BAUGH
Cornell University

MARTHA VICINUS. *The Industrial Muse: A Study of Nineteenth-Century British Working-Class Literature*. New York: Barnes and Noble. 1975. Pp. x, 357. \$20.00.

The Industrial Muse is a noteworthy addition to the growing literature on working-class culture, but any comparison with the writings of E. P. Thompson, Raymond Williams, and Richard Hoggart must underscore the defects in Vicinus' book. For all the exhaustive research in local history and folklore collections on which this study is based, it

is narrowly conceived and methodologically traditional. Since nineteenth-century British working-class literature offers few developmental signposts, Vicinus has imposed her own structure by focusing on several distinct and generally unrelated areas—broadside and street ballads, political fiction of the Chartist period, dialect literature, self-educated poets, and music hall entertainment, the last of these relating to the working-class in its origins only. Such diversity has fostered an idiosyncratic process of selection: Vicinus scrutinizes closely the propaganda of the coal miners' unions in the 1830s and 1840s, but she ignores improving literature and late-Victorian radical and socialist writings, and she assesses dialect writing with scant reference to Scotland.

Curiosity value aside, many of the works so industriously brought to light are ephemeral and sometimes unreadable, scarcely confirming the author's contention that "good working-class literature was produced which spoke with a clear and confident voice about and for the people" (p. 4). Their interest lies in their relation to the cultural environment in which they were produced, but, despite avowed intentions, it is just this contextual dimension that the book lacks. Works are subjected to conventional stylistic analysis, their imagery and characters delineated, but the laboring world which they reflect remains dimly in the background. One commendable exception to this pattern is the illuminating chapter on street literature, which enlarges Mayhew's picture and affirms the importance of broadsides in promoting literacy, providing accessible entertainment, and integrating newcomers into the urban community. The author is also particularly informative about the economics of publishing and the difficulties that beset working-class writers seeking commercial success.

Vicinus applies ideological as well as literary criteria in attempting to document "the struggle to create and sustain a distinctive literature in the face of bourgeois economic and cultural control" (p. 2). Extolling those who fomented class struggle and incited hatred of the employers, she tends to disparage the writers who offered no challenge to dominant cultural values. Yet it is by no means clear that the literature of social cleavage was any the more authentic an example of worker self-consciousness than that which advocated self-help or expounded domestic pieties. Furthermore, the taste of the audience itself, not merely the dictation of bourgeois publishers, determined what was written, and nineteenth-century working people preferred their reading matter to be escapist rather than exhortative.

F. M. LEVENTHAL
Boston University

GEORGE DODD. *Days at the Factories; or, the Manufacturing Industry of Great Britain Described, and Illustrated by Numerous Engravings of Machines and Processes*. Reprint. Totowa, N.J.: Rowman and Littlefield. 1975. Pp. 408. \$14.50.

HOWARD M. GITELMAN. *Workingmen of Waltham: Mobility in American Urban Industrial Development, 1850-1890*. Baltimore: Johns Hopkins University Press. 1974. Pp. xvi, 192. \$10.00.

Although the locale of their topics is separated by the Atlantic Ocean, these two books deal with important phases of the work scene. George Dodd wrote in the early 1840s and deserves reprinting. To a large degree he deals with process industries such as brewing, sugar refining, distilling, and tanning. In addition he includes stone cutting, bell founding, tower-clock making, and the manufacture of pianos. Nineteen topics, sometimes related, are covered, and ample space is accorded to each out of the over four-hundred-page total. This allows for numerous illustrations and enough detail in the text to make the process intelligible without becoming tedious. The locale is London and its environs. The style of the book is an easy and familiar narrative which manages to convey many details of the busy commercial metropolis. From the historical contrasts presented, one can easily appreciate the advances brought about in such industries by the Industrial Revolution. The greater number of the articles originally appeared in "The Penny Magazine" and in their day were used in schools, including the engineering class of Kings College. This book remains a unique and valuable reference source.

Howard Gitelman, in his *Workingmen of Waltham*, gives deep insight into the manpower behind industry. He treats the period 1850-90 in one of the earliest Massachusetts industrial communities, on the edge of Boston. His prime focus is "Mobility in American Urban Industrial Development." The site chosen is a good one because the two leading industries are widely diverse: a cotton mill and a leading watch factory. Other smaller industries were largely supportive or derived from the leaders. The leading foreign-born labor force, Irish, is described in detail as it gradually achieves more and more acceptance among the old Yankee stock. Moreover, the community remained small enough during the period of study so that the volume of statistics remained manageable. Beyond the main thrust of the book a clear picture is presented of the process by which figures were derived. There is much notice of the various factors which could not be determined, but which modify the sharpness of all figures. Where inferences have been made they are so identified. The various chapters are readably written and offset the stark statistics. The index consists of only two pages and does not do

justice to the wealth of peripheral material in the text. An appendix of over five pages explains the research methods. A second appendix enumerates all of the occupational classifications. These last not only aid the reader but also make the book an effective guide for other researchers engaged in similar projects.

EDWIN BATTESON
Smithsonian Institution

EDWARD ROYLE. *Victorian Infidels: The Origins of the British Secularist Movement, 1791-1866*. Totowa, N.J.: Rowman and Littlefield. 1974. Pp. viii, 357. \$19.50.

The Victorian infidels of Edward Royle's impressive study were those mid-nineteenth-century secularists who, under the leadership of George Jacob Holyoake, attempted to accommodate English radical unbelief to Gladstonian liberalism after the collapse of Chartism and Owenism in the 1840s. In contrast to the militant deists and atheists who had dominated the freethought movement from the time of Thomas Paine, Holyoake and his followers believed that the political and social realities of the 1850s called not for confrontation but for positive cooperation, even with Christian reformers. As a consequence, the stridently sectarian and millenarian tradition of unbelief in the utopian schemes of Owenite socialists and other early nineteenth-century radicals was consciously abandoned in 1851 for a moderate, progressive, realistic, and respectable future under the aegis of "Secularism." The ideas of the new movement which were largely embodied in John Stuart Mill's essay, *On Liberty*, and promoted by the numerous secular societies and journals that appeared in the 1850s form the core of Royle's book.

Although *Victorian Infidels* concentrates on the middle decades of the century and the role of Holyoake, the author also provides a lucid and perceptive account of the English freethought tradition from the Enlightenment to the thwarted hopes of Chartism and Owenism. He describes the ideological and personal conflicts plaguing the freethought movement in particular and radicalism in general. These conflicts, as Royle so well demonstrates, were if anything exacerbated by the rise of Secularism as Holyoake fought to defend his new policies of cooperation against the increasingly bitter and eventually successful attacks of Charles Bradlaugh and the aggressive atheists who captured the movement and its principal organization, the National Secular Society, after 1861.

Royle's book is not only a thoughtful, well-documented account of a neglected aspect of nineteenth-century religion, it is an important contribution to an understanding of Victorian radical-

ism as well. His analysis of ideas is strengthened by a careful discussion of the geographical distribution of secular organizations and the social composition of their membership. Victorian infidels were for the most part skilled, self-educated workers, like Holyoake himself, who supported a number of radical causes. They were most active in Yorkshire, Lancashire, and the northwest, as well as in London.

The author has made effective use of the papers of Holyoake, Bradlaugh, and Owen, among others, but he has relied primarily upon the publications, mainly weekly journals, of the radical freethought and secular societies. These lend themselves best to a study of the ideas and policies of unbelievers as seen through their own eyes, and that is what Royle has emphasized. Near the end of the book he sometimes treats them in a disjointed and cursory manner rather than integrating them carefully into chapters, but this is a minor defect in a very good piece of scholarship.

RICHARD ALLEN SOLOWAY
University of North Carolina,
Chapel Hill

THE MARQUESS OF ANGLESEY F.S.A. *A History of the British Cavalry, 1816-1919*. Volume 2, 1851-1871. Hamden, Conn.: Archon Books. 1975. Pp. 519. \$27.50.

The second volume of the Marquess of Anglesey's *History of the British Cavalry, 1816-1919*, covers the years of the Crimean War and the Indian Mutiny. A good half of the work is devoted to a minute reconstruction of individual cavalry actions in those campaigns and, more briefly, to the cavalry's role in the "Arrow War" of 1860 in China and the Abyssinian campaign of 1867-8. The balance of the book, and much the most successful part, deals with the social history of the cavalry at mid-century.

Academic historians are notorious for their condescension to antiquarians for their mindless pursuit of old material. As often, however, academics are quick to appreciate the genuine services that antiquarians perform in bringing useful evidence to general attention. Anglesey's work in its methods and intentions is antiquarian, with all the virtues and defects that the term implies. His purpose is to raise a literary monument to the courage and achievement of the old horse cavalry by describing its exploits and daily life. He does this in a well-written account, larded with extracts from a wide range of printed and manuscript sources and a generous ration of plummy anecdotes. Using a mass of blue-book material he illustrates nicely the harsh conditions of the troopers' life—poorly paid and fed, housed in crowded, badly ventilated bar-

racks, under appalling sanitary conditions (wooden urinals did double duty as wash basins); their mortality rates from disease were twice those of the foulest civilian slums. The richness of detail about the recruitment, training, education, and diet of "other ranks," and about veterinary services, the purchase of horses, and the market for officers' commissions is equally interesting. This is useful antiquarianism.

The accounts of cavalry action, detached from either a coherent campaign history or a systematic argument about the theory and practice of mounted warfare, are another matter. Only cavalry buffs will be pleased with these disjointed fragments, which are meaningless in the absence of a larger framework of narrative or analysis. A "definitive history" of the Victorian cavalry is not the sum of its battles and cannot be achieved by using Fortescue as a model, ignoring the changing political and technological context of warfare. Let us hope in the remaining volumes of this work Anglesey will not content himself with telling stories, but will pose, and try to answer, some substantial questions. A proper monument is made of durable material.

S. J. STEARNS
Richmond College,
City University of New York

T. J. NOSSITER. *Influence, Opinion and Political Idioms in Reformed England: Case Studies from the North-east, 1832-74*. New York: Barnes and Noble. 1975. Pp. xii, 255. \$22.50.

Those interested in T. J. Nossiter's two short, suggestive articles on elections and voting behavior in *Political Studies* (1970) or his essay on electoral behavior in E. Allardt and S. Rokkan (eds.), *Mass Politics* (New York, 1970), will be pleased to see this expanded version of his earlier work. As before, he views politics in nineteenth-century England as a matter not only of institutions but of process, paying particular attention to how the system worked and how contemporaries viewed it. As before, he sees changes in the process as essentially linear: from traditional patronage politics in which long established patterns of deference prevailed, through market politics in which the vote was treated as an economic asset, to the politics of individual opinion in which each voter considered political issues alone. And as before, Nossiter emphasizes the importance of retail merchants—the "Shopocracy"—as a major instrument of radical politics because they had the collective ability, motive, and opportunity to press individual opinion in the alien environments of patronage or market politics. He also reiterates his belief that the study of electoral politics should concentrate on

investigations of individual voters to establish the social basis of voting behavior. Finally, he continues to argue that aggregate, statistical analysis can be used to illustrate the progression from predominantly local to regional-centered politics.

These are ambitious themes and important ones in understanding nineteenth-century political life, but they are not presented as part of a larger unity. One of the problems Nossiter was unable to overcome was orchestrating his thematic material to give a central, comprehensive focus to his study. Nor is it clear that Nossiter has added significantly to his earlier conceptual or methodological points. What he does offer to the reader (or rather what his publisher offers for a substantial price) is a more leisured discussion of his arguments and a greater abundance of local detail as evidence to support them.

V. M. BATZEL
University of Winnipeg

BRIAN JENKINS. *Britain & the War for the Union*. Volume 1. Montreal: McGill-Queen's University Press. 1974. Pp. 315. \$12.50.

In this first volume of a projected two-volume study, Brian Jenkins interweaves developments in the Confederacy, the North, Canada, and Great Britain to show how British leaders managed to maintain neutrality during the first year of America's Civil War. By May of 1862, however, they were faced with serious handicaps in the form of Canada's vulnerability, evidence of a breakdown in the effort to maintain a common front with France, unsettled conditions in Europe, and pressure from various segments of the British public.

The author relies heavily on the studies of other scholars, published documents, collections of letters, diaries, and memoirs, and he supplements these with newspapers and manuscripts in public archives and private collections in Canada, Britain, and America.

Most of the themes stressed are familiar: the South's "King Cotton" strategy; British insecurity in regard to Canada; confusion about the purpose of the war; the tension between Britain and the North over the Proclamation of Neutrality and methods of restricting trade; conditions and attitudes among the British working class; reasons for the reluctance of Palmerston and Russell to recognize the Confederacy. The author adds little to the insights and conclusions set forth in older studies by E. D. Adams, Frank L. Owsley, Robin Winks, and others. His work is significant, insofar as he integrates these themes, showing how the events in one center affected developments in the other countries. It is a challenging task and sometimes

the "seams" show; but, on the whole, the result is good.

The most serious shortcoming of the book for the student of this period is the manner of documentation. Endnotes are used whenever the author includes quoted phrases or sentences (often unnecessarily), but there are few citations of sources on which he bases important observations when no quotations are given. When he quotes from letters or other documents cited in secondary sources, he often fails to mention the original depository. Also, it would be helpful to have a listing of the manuscript sources.

The study will be of interest to the general reader because of the way in which it brings together so many of the threads of diplomacy during the first year of the Civil War. If Volume Two has not yet gone to press, one would hope that the strengths of this first book can be maintained while the defects in documentation can be corrected. It would then be of more value to the professional historian.

ALICE O'ROURKE
Rosary College

D. W. FORREST. *Francis Galton: The Life and Work of a Victorian Genius*. New York: Taplinger Publishing Co. 1975. Pp. x, 340. \$14.95.

If the distinction between right- and left-brain dominance has any truth, Francis Galton was surely a left-brain man. Rarely has the urge toward quantification, purportedly controlled by the left brain, been more fully realized. In nearly every situation Galton found something to count: fidgets per minute in a restless audience, geographical distribution of "pretty girls" in the British Isles, brush strokes in a painting. In the words of D. W. Forrest, Galton's new biographer, "Not all of this work was pursued to very definite or useful conclusions, but it is worthy of mention on account of Galton's method, which was to measure whatever and whenever he could" (p. 183). Eventually Galton did settle on a problem worthy of his zeal, that of reducing the laws of heredity to mathematics. As it turned out, his statistical method was less immediately useful for understanding the mechanisms of heredity than the simpler ratios of the Mendelians. Even so, by the time of Galton's death in 1911 his mania for counting had brought statistics into the mainstream of biology and won him the highest honors of the scientific world. The present study adds to Galton's reputation by documenting the sheer volume of his work, and, within certain limits, brings him to life as "not only a genius, [but] also unmistakably a Victorian" (p. 288).

Writing a sympathetic but honest biography of Galton could not have been an easy task, since both his domestic and scientific behavior ranged from the brilliantly idiosyncratic to the suspiciously bizarre. Personally snobbish and overbearing, Galton's daily life was a mirror of Victorian faults; professionally a maverick, he transgressed disciplinary lines with little sensitivity to those he brushed aside. More seriously, Galton's assumptions were racist to a degree seldom matched in the work of other great men of Victorian science. Altogether Galton would be an unattractive subject were it not for his genuine achievements and mad humor.

Forrest excels in describing the essential aspects of Galton's life, private and professional. The summaries of Galton's enormously varied researches (geography, meteorology, biology, anthropology, psychology, statistics, fingerprinting) are accurate and succinct. And despite obvious regard for his subject, Forrest does not flinch from identifying Galton's personal faults. Nonetheless, this valuable work is marred by an overly narrow and chronological approach to biography. While provided with a great deal of information about Galton, the reader can only infer from passing remarks the location of Galton's work with respect to longer-term developments in English science and politics. Galton's place in the history of genetics is not well defined; nor are the political origins and intentions of his eugenical activities taken seriously. These deficiencies would be less striking if other historians had not already developed such material. Unfortunately, Forrest does not refer to their work. Thus, praise of this study must be qualified: within the limits of his approach Forrest succeeds admirably; outside them he provides only clues.

SANDRA HERBERT
University of Maryland,
Baltimore County

HUGH MCLEOD. *Class and Religion in the Late Victorian City*. Hamden, Conn.: Archon Books. 1974. Pp. xii, 360. \$16.50.

Here, without preliminary fanfare, is the best single volume on Victorian religion. The particular subject matter of the book is fairly precise. It concentrates on the Christian denominations in London between 1880 and 1914, and re-enforces its general analysis of working-class and suburban religion in the metropolis with particular studies of Bethnal Green and Lewisham. The conceptual frame of the book is equally clear. Hugh McLeod is concerned not with religion and the churches in

themselves, but with the social conditioning of religious behavior. Yet having delimited his subject and defined his concern with it, he brings to bear upon it broad reading in the religious history of different periods and countries, as well as in fiction. He displays a sensitivity to the nuances of behavior within the finely graded hierarchy of the English class structure and a talent for succinct analysis, unhurried as he peels back the layers of custom and conscious intent, bold when he has an argument to advance or a conclusion to draw. He expresses himself with unassuming calmness.

There is much to savor in this book apart from its thesis. The book, in fact, can serve as a superb introduction to its general subject. At the same time, the book presents a sustained and provocative argument: that the seeming strength of mid-Victorian Christianity depended upon the social code of "respectability," a code which possessed coercive force among the middle, especially the upper-middle, classes; that the Victorian working classes, on the contrary, tended to seek consolation for the miseries of their existence "by withdrawing into a more local world, within which their words and actions *were* of some consequence" (p. 282); that religious observance began to give way after 1880 as the code of respectability lost its binding force; and that the appeal of religious associations to ambitious individual spirits among the working class was undermined and eventually all but destroyed by the social code of class solidarity, a weapon almost as useful for socialists as respectability had been for evangelicals.

PETER MARSH
Syracuse University

WILLARD WOLFE. *From Radicalism to Socialism: Men and Ideas in the Formation of Fabian Socialist Doctrines, 1881-1889*. (Yale Historical Publications, Miscellany, 104). New Haven: Yale University Press. 1975. Pp. 333. \$17.50.

This is a probing and imaginative study of the shaping of socialist ideas among the early Fabians: George Bernard Shaw, Sidney Webb, and to a lesser degree Annie Besant, Sydney Olivier, and others. But Willard Wolfe also makes a general argument about the nature of British socialism. He begins with a discussion of John Stuart Mill and his important influence in shaping a non-doctrinaire socialism, then turns to the revival of socialist thought through the influence of Henry George and the formation of the Social-Democratic Federation. But he devotes the greatest part of the book to comparing the early Fabians, and Wolfe is particularly conscious of the influence of Comte and Positivism upon their ideas. He de-



monstrates that Fabian collectivism grew naturally from British radicalism and that both helped shape the limited socialism of the Labour Party. The argument is well done and supported by an extensive use of manuscript sources. Yet again, one is made vividly aware of both the attractiveness and irritating qualities of the traditions of British moderation.

Because continuity tends to submerge change, the legitimate emphasis on the similarities between radicalism and socialism runs the danger of oversimplifying the comparison. There is also too much emphasis on the quasi-religious nature of British radical and socialist thought. The nineteenth century was an age when people were obsessed with religion, but Wolfe, through the excessive use of such terms as conversion and faith, seems to be arguing that his figures' political views were, in large part, substitutes for religion. Undoubtedly, their political and social ideas filled some emotional needs, and religious terminology, for Fabians as well as more popular leaders, was an obvious way to make concepts immediate. But Fabian socialism was not only, as Wolfe argues, a better faith than radicalism; it appeared to stand a better chance of solving Britain's problems. Wolfe's approach provides many new insights, but perhaps he has not given sufficient attention to the degree to which the Fabians were reacting to the world around them, seeking not a faith but a cure. He is also a little too anxious to point out his own originality and the errors of others. But his book is strong enough not to require such prods to the reader. It is a highly important contribution to the story of the development of left-wing thought in late-nineteenth-century Britain.

PETER STANSKY
Stanford University

A. J. A. MORRIS, editor. *Edwardian Radicalism, 1900-1914: Some Aspects of British Radicalism*. Boston: Routledge and Kegan Paul. 1974. Pp. x, 277. \$18.00.

That Edwardian subjects remain popular is evidenced by the serialized television dramatization of Galsworthy's *Forsyte Saga* and the interminable *Upstairs, Downstairs*. Perhaps this is as it should be; for a generation as battered and jaded as our own instinctively turns for consolation to a past where problems were simpler, where the lower orders knew their place, and where all was right with the world. Yet nostalgia is, after all, only escape into a mythologized world of wish-it-would-have-been.

The fourteen years separating the "dirty little war" against the Boers from the infinitely dirtier Great War found British society strife-ridden and

beset by controversies ranging over every aspect of domestic and foreign policy. Playing a significant part in these controversies were the radical Liberals, that is, those in the Benthamite tradition of middle-class dissent from an Establishment dominated by the aristocracy and Church of England. Edwardian radicalism is a complex subject that has generated a vast literature. Although the complete story has yet to be told, this book will surely play a part in the telling. The editor has assembled fifteen essays, blending the work of some of the keenest young historians with that of seasoned veterans. Each contributor directs a beam at a facet of the radical phenomenon, thereby helping to illuminate the whole. As regards social reform, G. H. S. Jordan and Roy Douglas deal with old-age pensions and the land question. Stephen Koss, A. J. Lee, A. K. Russell, and A. J. A. Morris contribute essays on political subjects: Non-conformity and the Liberal Party, the radical press, Liberal party organization, and Charles Trevelyan. John Grigg, Catherine Ann Cline, E. C. Moulton, and Howard Weinroth explore foreign affairs, focusing on Lloyd George and the Boer War, the journalist E. D. Morel, radical attitudes toward India, and the issue of nationalism. My own favorites are Dame Margaret Cole's perceptive essay on H. G. Wells' conflict with the Fabian leadership and Marvin Swartz's on the radicals' reaction to the beginning of war in 1914. F. N. Leventhal deals masterfully with H. N. Brailford's search for a new international order—the time has come for a full-scale biography of this influential journalist. Clive Trebilcock's "Radicalism and the Armament Trust" is an eye-opener; apparently armaments manufacturers were not the calculating Molochs of radical propaganda. I for one eagerly await Trebilcock's book on the armaments industry.

These essays are not without their infelicities. Marvin Swartz, for example, asserts that Norman Angell regarded war as "uneconomical." Really! The author of *The Great Illusion* considered war absolutely catastrophic in an interdependent industrialized world. Moreover, a solid essay on the radicals and the suffragettes might have gone a long way toward resolving a particularly vexatious problem. But on the whole, these criticisms are minor; and A. J. A. Morris has rendered students and interested general readers a service.

ALBERT MARRIN
Yeshiva College

ROSS MCKIBBIN. *The Evolution of the Labour Party, 1910-1924*. (Oxford Historical Monographs.) New

York: Oxford University Press. 1974. Pp. xviii, 261. \$18.50.

This important monograph demonstrates persuasively why the Labour Party replaced the Liberals as the principal party of the left after the First World War. By carefully studying Labour's formative years—the activities of trade councils, labor representation committees, and later constituency parties as well as those of the Head Office—Ross McKibbin shows how surprisingly limited were the changes made by its 1918 constitution. He argues that most historians have been off the mark in seeing World War I as the main determinant of Liberal decline and Labour success. Instead, he makes a convincing case for the enduring antebellum character of the Labour Party, illustrated by its continuity of leadership and personnel, its essential continuity of policy, and especially its continuity of organization. Inextricably linked to the trade-union movement, the Labour Party was national in character and centralized in form. The new constituency parties were successful mainly as they reflected trade-union strengths. Attempts at "federalization" of the party, opposed by the great unions, were likewise rejected by party leaders. Centralization was limited only when it conflicted with the interests of the unions. The war accelerated rather than generated a tendency for the Labour Party to base its strengths not upon broadly articulated principles but upon intense class loyalties and class consciousness. In such a situation, particularly after the further expansion of the franchise in 1918, class self-awareness meant that Labour achieved significant gains at the expense of the Liberals, who could make no particular claims on the loyalties of any class. Within the Labour movement, such loyalties overshadowed socialist doctrine, so that despite postwar rhetoric the movement itself, and service to the movement, became a substitute for any firmly held ideology. In effect, McKibbin concludes, the criticism that the Labour Party has never served the cause of socialism or the socialist vision of the true interests of the working class is wrongheaded. The Party was never designed to do so.

HENRY R. WINKLER
Rutgers University

PARTHA SARATHI GUPTA. *Imperialism and the British Labour Movement, 1914-1964*. New York: Holmes and Meier. 1975. Pp. xviii, 454. \$25.00.

This is an altogether admirable study of British Labour's attitudes toward empire from the end of the First World War to the present. The author sets his problem by quoting Lenin who in 1920

wrote that the English craft unions were "imperialist minded and imperialist corrupted." Gupta rests his argument primarily on unpublished sources in the Public Record Office, the Colonial Office, the Dominions Office, the India Office Library, and the National Archives of India. He has shown in some detail how the anti-imperialist idea became dominant in British Labour circles.

Since its beginning the British Labour Party had both an imperialist and an anti-imperialist wing. Fabians and moderate trade-union leaders belonged to the former, while the Independent Labour Party militants and left-wing elements constituted the latter.

Lenin failed to understand that his model of imperialism, which he conceived of as the last stage of a bankrupt capitalism, would be less relevant if democratic socialist policies prevailed. It was actually not the craft unions that were pro-imperialist. British economic troubles in the twenties were due to an overvalued currency rather than to depressed wages caused by foreign competition. Trade unionists did not look to policies of imperialism in the Kipling mold for an answer. They did pressure Labour governments in the case of Indian cottons and textiles, but the radical anti-imperialist tradition triumphed.

Bevin was the only Labour leader who thought the British craftsman's prosperity depended on empire. In 1965 Labour's acceptance of the idea that military defense demanded empire was really a carry-over from wartime attitudes. Attlee's imperialism seems to have been mostly a matter of prestige. The Fabians were essentially racist, and this caused them to misunderstand the situation in West Africa. "Though the conduct of Bevin and Morrison in these post-war years," Gupta writes, "showed that imperialism could become linked with social democracy and appear to be essential to the latter, it remained true that in the final analysis reformist social democracy neither needed nor could afford an imperial policy."

MARK NAIDIS
Los Angeles Valley College

STEPHEN LYON ENDICOTT. *Diplomacy and Enterprise: British China Policy, 1933-1937*. Vancouver: University of British Columbia Press. 1975. Pp. xv, 209. \$15.00.

This is a brief, yet detailed, examination of the bureaucratic infighting behind British Far Eastern policy. Set against a backdrop of fading hopes that international cooperation, not militarism, would govern world affairs, the narrative contends that the Treasury Ministry's triumph over the Foreign Office was crucial in determining Great Britain's

response to Japanese expansion in China; that is, Britain's shift after 1931 from a policy of placating Japan to one emphasizing financial assistance to China is shown to have been the product of the favor bestowed by the Cabinet on the Treasury's formula. The Foreign Office, worried by adverse turns of events in Germany and concerned lest Japanese ambitions ultimately threaten vital imperial interests in Asia, had steadfastly urged an accommodation with Tokyo. The Treasury's victory, Endicott believes, was the result of the government's susceptibility, in the midst of a great social and economic crisis, to pressures exerted by business firms having a stake in the China market.

The outcome (a policy that wavered uncertainly in the early 1930s) is condemned on all counts: the uncertainties scarcely soothed Far Eastern tensions; London's assistance to the Kuomintang-Nationalist government only served to prop up a cruel militarism, undeserving of support; and Great Britain herself managed to antagonize the Japanese without getting anything in return. The research producing this indictment is developed within the framework of the "revisionism" that has colored in recent years much of the writing on American foreign relations. Endicott is most persuasive when he speaks to the narrower products of his research, namely, his discovery that a bureaucratic rivalry had an impact on policy, and that capitalistic interests were important pressure groups influencing policy. His study is much too limited, however, to sustain some of his broader polemics on the sins of private capitalism.

BURTON F. BEERS
North Carolina State University

MARY PROUDFOOT. *British Politics and Government, 1951-1970: A Study of an Affluent Society*. London: Faber and Faber; distrib. by Humanities Press. Atlantic Highlands, N.J. 1974. Pp. 240. \$12.50.

The belief that they were finally the masters surged through every Labour breast in 1945. Conservatives cringed at the thought of becoming a permanent minority. But within six years the Labour Party was out, not to return to Downing Street until a narrow win in 1964. A decisive sweep in 1966 was followed by an unexpected defeat four years later. Mary Proudfoot's useful summary of events concludes that by 1970, "There was very little . . . to choose between the two Parties." Perhaps this is one of the reasons for the indecisiveness of the two elections of 1974, the increase in Liberal votes, and the emergence of Welsh and Scottish nationalism.

During the dark days of World War II even *The Times* was prepared to agree that political equality

must not be "nullified by social and economic privilege." Though Proudfoot believes that she is writing about an "affluent society," she neglects the extent to which wealth is concentrated in a small number of hands, while equality, social justice, and educational opportunity are denied to many citizens. The "other" Britain is as evident as the "other" America.

Proudfoot's assessments of prominent political figures are always interesting, but they tend to be rather thin. She excuses all because, "The options open to the British in the fifties were few indeed." There is no sustained analysis of how the British establishment rallied after 1945 to retain its power and wealth and to prevent the move to a more truly democratic society.

When one considers the daring and imagination of Britain's scientists and men of letters in the postwar era, one is appalled at the timidity of British political leaders. As a social democratic alternative to both capitalism and communism, Britain might well have pointed the way to the future, just as she did when she became the workshop of the world and a parliamentary democracy.

THOMAS J. SPINNER, JR.
University of Vermont

NIGEL NICOLSON. *Alex: The Life of Field Marshal Earl Alexander of Tunis*. New York: Atheneum. 1973. Pp. xiii, 346. \$10.00.

Here, at last, we have a sympathetic account of Field Marshal Earl Alexander written by a former young captain in British army intelligence, later an M.P. under Alexander as defense minister, who saw "Alex" first at the Tunisian capitulation. The sometime Governor-General of Canada was badly served by the John North-edited *Memoirs* (1962) which were properly to be called "Lord Alexander Revisits the Battlefields." General W. G. F. Jackson, a major historian of the Italian campaign, wrote a careful appraisal of a dutiful soldier in *Alexander of Tunis as Military Commander* (1971). In Nicolson's book, well-based on documents, we have an adulatory biography of a proper guardsman who can be seen as a warm person behind his aristocratic British reticence. As his biographer, Nicolson was lucky that Alexander wrote memoirs and preserved letters of his early life and that, when the pressures of World War II came along, he was high enough up the chain of command that the official papers replace the fullness of his former sources. Alexander was present for the retreat to Dunkirk; he pulled the British Army out of Burma; and finally he served in the sticky job of being Montgomery's Commander-in-Chief in the

Western Desert, North African, and Italian campaigns. In the latter he stayed after Montgomery's departure. Although never quite at home as a politician, Alexander went on to be Governor-General of Canada and then Minister of Defense in the 1951 Churchill government.

ROBIN HIGHAM
Kansas State University

T. M. DEVINE. *The Tobacco Lords: A Study of the Tobacco Merchants of Glasgow and Their Trading Activities c. 1740-90*. Edinburgh: John Donald Publishers. 1975. Pp. xi, 209. £8.00.

Rather than provide a comprehensive and focused survey of Glasgow's tobacco trade between 1740 and 1790, T. M. Devine divides his work into sections which deal with specific questions: who were the tobacco merchants? how many were there? what did they do with their profits? why were they successful? how much did their business suffer because of the American Revolution? The analysis begins with a study of the merchant community. Devine demonstrates that after 1740 most tobacco merchants came from middling, not poor, backgrounds, often from families of other merchants. While new men could enter the trade, the number of tobacco merchants was small, and three or four firms generally controlled half the trade or more. This section of the work, which also includes details on specific firms, is the most significant, but could have been improved greatly by comparisons with other trades, ports, and an earlier time period.

Devine's eclectic study makes three additional contributions. First, he reaches the modest conclusion that investments by tobacco merchants had a limited but significant impact on the growth of particular Scottish industries. Estimates of the total investment in the Scottish economy and the growth of that economy would have been helpful, but Devine's painstaking investigation of investment patterns of particular firms and in particular industries is commendable and takes one as far as this methodology can. Second, Devine demonstrates that most financing of the Scottish tobacco trade was done by private individuals. He contradicts the view that banks played the major role. Third, the author shows that Glasgow's trade with Virginia did not collapse because of the American Revolution. After 1783, stores were re-established in the Chesapeake, and Scottish merchants shifted from the re-export to the direct-carrying trade.

PAUL G. E. CLEMENS
*Rutgers University,
New Brunswick*

PETER BERRESFORD ELLIS. *Hell or Connaught! The Cromwellian Colonisation of Ireland, 1652-1660*. New York: St. Martin's Press. 1975. Pp. 268.

T. C. BARNARD. *Cromwellian Ireland: English Government and Reform in Ireland, 1649-1660*. (Oxford Historical Monographs.) New York: Oxford University Press. 1975. Pp. ix, 349. \$29.00.

The two works under review deal with approximately the same decade in Irish history (a period from which Irish historians seem instinctively to recoil), but otherwise they are as different as chalk is from cheese. The main themes of Peter Berresford Ellis' book are concerned with the central topics of the 1650s: with the vast agrarian upheaval caused by land confiscation, plantation, and transplantation; with religious persecution; and to a lesser extent, with the impact on the Irish administration of political changes in England. This is narrative history written in great detail but anecdotal and wayward in its course. Though he modestly disclaims an "academic status" for the work and eschews footnotes, it is evident that he has read widely and, through some acquaintance with contemporary Irish literature, provides a welcome if somewhat opaque window into the Gaelic world. Unfortunately he makes no attempt to evaluate his sources and curiously does not refer to R. C. Simington's great sets of source material concerning the Catholic Irish landowners and their Cromwellian supplanters.

T. C. Barnard's book, in contrast, has little to do with "Hell or Connaught" and therefore might be considered peripheral to the central issue; but it is a most original contribution to the history of the 1650s. In spite of the destruction of most Irish Commonwealth records he has tapped a vast amount of source material. Indeed his industry has been truly prodigious.

After the Cromwellian conquest Ireland was virtually a *tabula rasa* on which the English parliament sought to imprint its own ideas on government, central and local; on finance and trade; and on religious, educational, and legal reform. When Henry Cromwell took over government from the military commanders in 1655 the harsher measures of the military administration were ameliorated, but the new policies of the civil administration equally failed in their main purpose to "anglicize" the native Irish population and to make them Protestants. Nor could they impose uniformity among Protestants themselves. Barnard indicates the reasons for failure—notably that, since the Irish parliament had been abolished, legislation for Ireland had to be passed through the English parliament. Inevitably its interest in Ireland was intermittent, lacked continuity, and was subject to shifts of power in London.

Before the rising of 1641, the Protestant element,

mainly administrators who had come over from England in the late Tudor and early Stuart periods, though not the major landed interest, had a near monopoly of political power. Barnard shows that this element (the "old Protestants") came back into favor under Henry Cromwell at the expense of the Cromwellian planters (the "new Protestants"). They facilitated the restoration of Charles II and thus perpetuated their own survival as the "Protestant Ascendancy." In these transactions a key figure was Roger Boyle, Lord Broghill. One of the novel features of Barnard's book is the space devoted to the present fashionable history of ideas on education, on "the advancement of learning," and on the rise of science in the early seventeenth century. Here too Broghill was a key figure. Making use of the recently available Hartlib papers, Barnard demonstrates substantial Anglo-Irish support for these ideas. This support lasted into the Restoration period and led to the establishment of the Dublin Philosophical Society, which was the forerunner of the present Royal Dublin Society. Thus, in spite of the cataclysmic nature of the Cromwellian catastrophe, one is reminded of the continuities in Irish history. This is a most stimulating book.

DONAL F. CREGAN
St. Patrick's College,
Dublin.

MAURICE R. O'CONNELL, editor. *The Correspondence of Daniel O'Connell*. Volume 3, 1824-1828. New York: Barnes and Noble. 1974. Pp. vi, 441. \$30.00.

There is little to say about this volume in Maurice O'Connell's edition of Daniel O'Connell's letters, not because of any flaws, but because of its excellence. As nearly every reviewer of previous volumes in this series has noted, O'Connell is a skilled editor, economical in his interpretative notes, yet fully adequate in his explanations. The production of this volume by the recently revived Irish University Press is excellent.

The years covered here, 1824-28, are of great importance both to Daniel O'Connell and to Ireland, for they were the time of the establishment of the Catholic Association and O'Connell's almost singlehanded creation of the modern Irish nation. O'Connell's letters to and from bishops and leading politicians are fascinating, but even more interesting is his domestic correspondence. In his letters to his wife one finds delightful evidence of a fact too easily ignored: O'Connell, the hard-headed political strategist, was when necessary an immensely charming man—gentle, courtly, witty, and solicitous. Undeniably, his control of the masses led directly to his political ascendancy, but his charm and unctious in the drawing rooms of the

Catholic gentry and merchants were important to the financing and functioning of his movement.

The only problem this fine volume raises is the fear that before the entire series is completed the economics of present-day scholarly publishing may force its discontinuance or a deleterious alteration in its format.

DONALD HARMAN AKENSON
Queen's University
Kingston, Ontario

W. S. SCOTT. *Jeanne d'Arc*. New York: Barnes and Noble. 1974. Pp. 239. \$13.75.

BLAISE DE MONLUC. *The Habsburg-Valois Wars and the French Wars of Religion*. Edited by IAN ROY. (Military Memoirs.) Hamden, Conn.: Archon Books. 1972. Pp. x, 253. \$9.00.

Though they deal with very different personalities, these two books are similar in some respects. One is the autobiography of a military hero; the other, a biography of one of the few military heroines in history. Both characters are slightly overblown by their respective authors.

Joan of Arc has captivated the imaginations and the pens of numerous historians and biographers, though her career spanned only two and a half years, and her actual accomplishments are questionable. In 1973 and 1974, three biographies of the Maid appeared in English, including Scott's, which is one of the most puzzling. Scott, a retired English priest, appears to have devoted his entire life to a study of Joan, a study which has not proved particularly productive for historians. The book is charming in its way, filled with antiquarian facts, scholarly arguments about the routes Joan took, and local legends (the words "there is a strong local tradition that . . ." appear frequently in the text). What is missing is a meaningful appraisal of Joan that fits her squarely into the era when she lived. In his generally admirable annotated bibliography, Scott lists only one book dealing with the general history of France during the Hundred Years' War, and it is almost a century old. Works such as those by E. Perroy are notably absent, perhaps because Perroy believes that Joan was a far less significant historical figure than does Scott.

Scott is guilty of perpetuating all the clichés about Joan. He views her as a patriot fighting to save the French nation as well as Charles VII; he regards her as a pure, almost angelic girl; and he believes that the outstanding feature of Joan's character was that she was "an individualist."

Scott's lack of broad vision, and his over-concentration with the minutiae of the Maid's life, have created a one-sided, over-glamorized por-

trait. Had he stopped to consider what Joan was *really* able to accomplish during her brief career, he might have written a more balanced account.

Blaise de Monluc puffs himself out of proportion because his *Commentaries*, written in 1570–71, were designed partly as an attempt to regain royal favor. Thus Monluc emerges from his own pages as a valiant, swaggering, lovable Gascon who is also courteous, kind, resourceful, and obedient to those in command. In glorifying his exploits, Monluc was also writing a book to instruct young commanders in the tactics and even civilian machinations of the military life: how to behave at court, how to deal with women (it's best to avoid them), how to conduct a siege or lead a successful retreat. Monluc's autobiography is fascinating, and it is a pleasure to read selections from it in English. Ideally, a new translation could have been provided; but the one presented here, first written by Charles Cotton in 1674, has been modernized and is eminently readable.

The selection of materials presented in this volume is entirely adequate. Approximately one-fourth of the original *Commentaries* has been included, and while one could wish that more of Monluc's descriptions of court life and of the Wars of Religion had been inserted, the reader can find here the general's dramatic account of his defense of Siena in 1554.

The most interesting aspect of this military memoir is the ambivalence Monluc feels for the court. Though he despises courtiers, he is also completely captivated by the prevailing notions of honor and "good repute." Noble deeds on the battlefield are not enough. The way to succeed in the sixteenth-century French army is to ensure that one's exploits are suitably reported to the king or queen.

Ian Roy's introduction is highly informative, his footnotes are succinct and helpful, and his brief biographies and glossary of military terms are extremely useful. This is definitely a book which both military historians and historians of France generally will find valuable and instructive.

PETER M. ASCOLI
Utah State University

NATALIE ZEMON DAVIS. *Society and Culture in Early Modern France: Eight Essays by Natalie Zemon Davis*. Stanford: Stanford University Press. 1975. Pp. xviii, 362. \$15.00.

As a collection of essays on peasants, artisans, and the illiterate populace of the cities of early modern France, this book should interest historians who are not directly concerned with the period or with popular history. Through what she calls "case studies" of the *menu peuple*, Natalie Davis demon-

strates her impressive and wide-ranging research not only into little-known or used documents of her period but also into the scholarship of the social sciences in general. Though five of these essays have appeared in periodicals, their presence in this volume allows readers to appreciate the coherence of Davis' research and methodology and to acknowledge the significant contribution of her work to opening new vistas on the society of early modern Europe.

Davis clearly demonstrates the importance of collective behavior, playlets, pamphlets, welfare rolls, village festivals, political tracts, and sermons to understanding relationships among people and grasping the cultural traditions and symbols of a period. But this collection is scarcely a mere potpourri of popular sources relating to the *menu peuple* and the culture of early modern Europe. As the author points out, she has presented her material according to certain views of social structure and process, which have emerged out of her years of work with the documents. Using the studies of sociologists, anthropologists, and psychologists to shed light on the society and people she studies, Davis has demonstrated the value of the cross-disciplinary approach and has provided scholars with a multidimensional and dynamic scheme of early modern society that acknowledges many more variables in the social order and more complex causes and results of social change than property, power, sex, age, and religion alone or together can encompass.

In these essays, as in all her work, Davis raises questions and poses answers that are crucial to an understanding of this society and point the way for further investigation. She asks what kinds of experience might contribute to the formation of social consciousness among male artisans. What prompted Protestant allegiance among groups, in particular urban women. What political and social use, if any, did public festivals serve. Further she explores patterns of sexual inversion in literature and popular festivals; the shape and structure of popular religious violence, and the goals and actions of the participants in religious riots in sixteenth-century France. She looks for the relationship between written and oral material, which leads her to ask what impact did the establishment of new communication networks have when printing touched popular life in the sixteenth century. To further enrich the readers' understanding of this society and culture, the book is illustrated with eight pages of woodcuts, paintings, and engravings chosen with a combination of Davis' precise scholarship and delightful humor.

Because Davis is interested in social structure, the context of social change, and the relationship between the religious and secular as well as be-

tween male and female, her work will inevitably raise questions for historians of other periods and places. Students of the sixteenth-century Reformations will be interested especially in these glimpses into the lives and values of the men and women—artisans, tradesmen, and craftsmen—who experienced the impact of the changes taking place around them but who have been largely ignored because they left practically nothing in writing.

This work offers scholars two rewards. First, Davis' important contributions to early modern European studies are now collected and edited (in many cases with new footnotes and insights), making them easily accessible. Second, having sampled this *apéritif*, the historian will eagerly await the *pièce de résistance*: Davis' anticipated study of the printers in sixteenth-century Lyon.

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GEORGES FRÊCHE. *Toulouse et la région Midi-Pyrénées au siècle des lumières (vers 1670-1789)*. Paris: Éditions Cujas. 1975. Pp. xviii, 982.

With the publication of this massive study of upper Languedoc by Georges Frêche, students of Old Regime France acquire a wealth of new information on social and economic conditions and their regional diversities. The broad outline of Frêche's findings fit the now-familiar pattern of population growth, price increase, and economic expansion. The particular contribution of this work is to illustrate the details of economic change with, possibly, greater precision than ever before. Though not so profoundly innovative as Goubert on Beauvais or Le Roy Ladurie on lower Languedoc, Frêche has written a model regional study, in which the pursuit of methodological rigor is evident on every page.

Frêche covers this extensive portion of southern France, inhabited by over two million people, by focusing on the material framework in which they lived: the distribution of land, agricultural production, prices, population, taxes, and markets. His approach, in nearly all instances, is to seek the quantifiable aspect of his subject. It should be noted at once that Frêche frequently achieves the highest goal of quantitative history: to pose questions with a meaningful human dimension and answer them as rigorously as possible. The changing professional composition of villages and towns; the number of rural schools, charity boards, and medical personnel; the price of popularly-consumed grains—these few examples may serve to suggest the wide range of subjects Frêche considers. Surely, the author's success in telling us about *people* by counting and calculating derives from his

absolute mastery over an immense variety of documents and research methods. The technical demands of this study were clearly awesome.

The key socioeconomic development in upper Languedoc was its evolution into a center of cereal monoculture for export toward the Mediterranean. "The production of wheat commands all," says Frêche, and he traces the consequences of this to the concentration of land in the hands of the urban rich and the increasing pauperization of the rural population, which was growing even faster than the national average. The author locates the source of economic dynamism in improved market conditions, produced by free-trade policies, by better roads, and, above all, by the construction of the Canal des Deux Mers. Landlords benefitted from high prices, lower labor costs, and a five-fold increase in cereal exports during the eighteenth century.

Frêche has not lost sight of national trends in the richness of his regional data. Readers should find his comparative treatment of prices and subsistence crises illuminating. The extensive methodological discussions will be immensely useful to local specialists.

So thoroughly has Professor Frêche described material conditions in the Midi-Pyrénées that one wants to know more about how people thought and acted. The texture of urban life, the family, and protest are not major concerns here. But our appetite for still more information—coming after 800 pages of dense text—can only be a tribute to the energy, ingenuity, and expertise that produced this work.

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OLWEN H. HUFTON. *The Poor of Eighteenth-Century France, 1750-1789*. New York: Oxford University Press. 1974. Pp. x, 414. \$29.00.

JEAN-PIERRE GUTTON. *La société et les pauvres: L'exemple de la généralité de Lyon, 1534-1789*. (Faculté des Lettres et Sciences Humaines de Lyon, 26.) Paris: Société d'édition "Les Belles Lettres." 1971. Pp. lv, 495.

JEAN-PIERRE GUTTON. *L'État et la mendicité dans la première moitié du XVIII^e siècle: Auvergne, Beaujolais, Forez, Lyonnais*. Lyon: Centre d'études Foréziennes. 1973. Pp. 248.

JEAN-PIERRE GUTTON. *La société et les pauvres en Europe (XVI^e-XVIII^e siècles)*. (L'Historien, 18.) Paris: Presses Universitaires de France. 1974. Pp. 207.

Olwen H. Hufton's study of the poor examines the last four decades of the Old Regime in France, while Jean-Pierre Gutton, in his *thèse de doctorat*, concentrates primarily upon Lyon and its

surrounding region in order to range over two and a half centuries of change in attitudes and institutional responses toward poverty. Both explore the common features of poverty during the Old Regime, seeking to define the threshold between simple poverty and destitution in terms of occupation, diet, health, housing, and family.

Gutton and Hufton find most of the king's subjects dwelling insecurely on the threshold, "*pau-périssable*." Hufton cites St. François de Sales' telling image of charity as a kind of rich embroidery on the plain fabric of poverty; scholars must look to about 1850, Gutton tells us, for the tide of endemic mendicity to recede. Such conditions lead to "an economy of makeshifts," amplified by Hufton in chapters on relief, begging, theft, smuggling, prostitution, and the family. Gutton shows—"aux origines de l'errance"—how many trifling occupations border on vagrancy, how many *petits métiers* were casual and interchangeable expedients.

Hufton's work will attract the general reader as well as the specialist. Her description of seasonal migration conveys vividly the fatigue and dust of the journey. The reader can imagine the feelings of the poor toward the pious ladies who made them sell their last decent utensil before they might receive domestic charity. The mechanisms of public relief disbursed by intendants in bad years are seen as a contest between local authorities over scraps cast abroad in order to keep up hope of "something coming." Women's roles emerge clearly: the exploited lace maker struggling to amass a dowry; the mother spending her entire wage on child care, directing the ruses of a beggar brood, or obscenely taunting the reluctant almsgiver; the linen thief, the bawd, the decoy for a band of thieves. Hufton concludes the life cycle with the birth of an unwanted child: what must a mother have thought or felt who left her child in a drain, rather than keep it or entrust it to charity?

Gutton's focus on the Lyonnais—rich in its diversity of social milieux, institutions, and local records—allows him to weave large patterns, if on a smaller frame than Hufton. He can, for example, trace various paths of downward mobility in legacies to hospitals and in other fiscal records. He frequently looks beyond Lyon, however, in order to interpret the repeated efforts made in France and in other parts of Europe to achieve a total regulation of the idle, desperate poor, especially by means of institutional confinement.

If indeed the founding of the Aumône-Générale at Lyon in 1534 set a precedent for later moves to confine the poor, Gutton's special contribution is to have traced the undertow against this tide, citing Pascal's desire to die with the *incurables* and Massillon's apostrophe, "Is it not wretched

enough to be reduced to feigning wretchedness?" Royal declarations portray the *hôpitaux-généraux* as places of confinement; their administrators had other purposes, still evincing a refractory spirit when called upon to execute the Declaration of 1724 against vagabonds—hospital registers at St. Etienne reveal connivance with inmates in eluding harsh penalties for recidivism.

Both works have minor shortcomings. Hufton's avowedly qualitative work abounds in estimates and generalizations—deftly drawn contours around a profusion of detail. These invite refinement or revision. In some cases the firmness of line is deceptive. Hufton follows Christian Paultre's gross misreading of the monthly ration accounts of the *dépôt de mendicité* at Bourges, for example, to conclude that there were 3,388 inmates rotting in idleness there, rather than some 115.

Given his major theme, it may appear that Gutton has skimmed important aspects of policy and ideology. The reviewer hoped for a fuller re-examination of the conflict between royal and municipal authorities, which came to a head in the missions of the inspector Jean Colombier in the 1780s. More than a footnote might have been devoted to the efforts of the administrators of *La Charité* to elicit public discussion, via the *Affiches de Lyon*, of such matters as the means for employing the poor confined in hospitals (1769).

Gutton's *État et la mendicité* is based on his *thèse de troisième cycle* of 1967 entitled, "L'exécution de la déclaration royale du 18 juillet 1724 concernant la mendicité: généralités de Lyonnais et d'Auvergne." A model study of policy, administration, and society, it fills an essential gap and includes an account of Trudaine's efforts, while intendant of the Auvergne, to establish workshops to employ the beggars and vagabonds arrested and confined in hospitals under the terms of the Declaration.

Gutton elaborates earlier themes on a European scale in *Société et les pauvres en Europe*. He underlines the socially disruptive effects of poverty and the mentality of social fear. His work and Hufton's highlight the vulnerability of Old Regime society to the depredations of the unruly poor.

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WARREN ROBERTS. *Morality and Social Class in Eighteenth-Century French Literature and Painting*. Buffalo: University of Toronto Press. 1974. Pp. xvi, 188. \$15.00.

Warren Roberts develops the point that the literature and painting of eighteenth-century France reflected developments in French political and so-

cial life, especially developments in the history of the French monarchy and aristocracy. Accepting the now familiar view that aristocracy did not decline but, in fact, revived in the eighteenth century, the author explains the manners and morality pictured in literature and painting as being as much the product of this resurgent, but contradictory, aristocratic society as it was the result of the evolution of artistic forms.

Eighteenth-century French painting and literature reveal three major tendencies: one sentimental and romantic which stressed the Christian view of love, marriage, and the family; another openly erotic, sensual and un-Christian; and a third combining wit, sex, cruelty, and destructiveness with sharp social criticism. Seventeenth- and eighteenth-century aristocratic life provided the models and values for the various types found in this literature and painting.

The author also argues that to the extent that literature and painting revealed bourgeois values, these values and the art forms in which they were expressed grew from earlier aristocratic roots. He finds this especially true of the sentimental-romantic literature and didactic painting which preached the virtues of old-fashioned Christian love and marriage as "bourgeois" virtues.

Roberts' thesis is well argued. Aristocratic art, manners, and morality imposed themselves on bourgeois society. The bourgeoisie criticized the aristocrats, but they also copied them and longed to join them. It would be a mistake to conclude from Roberts' discussion, however, that the bourgeois spirit was *only* a borrowing from the aristocratic spirit. For the Christian morality of love, marriage, and the family, for example, was not the monopoly of the aristocracy (or of any class), and its social expression was not confined to the arts. The Parisian poor also believed, and they were not consumers of art. All classes simply shared a common heritage.

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Proust, Kafker, and Venturi, Lough raises many of the fundamental problems posed by the complexities of attributing authorship to the 79,000 odd entries in the *Encyclopédie* and the *Supplément*. The author surveys briefly the contributions made throughout the span of the vast publishing enterprise by the original "*société de gens de lettres*," delineating these initial contributors from the many other specialists who were recruited by them. Lough is concerned, too, with the sociological origins of the contributors, concluding that it is difficult to go beyond "the obvious fact that the contributors represent a cross section of the upper and middle classes of French society in the period 1745-1765" (p. 51). He rejects as insignificant the findings of Proust and Kafker who analyzed the writings of the surviving contributors during the Revolution, and especially the Terror, and who concluded that these men were "moderate reformers," not radicals. Quite correctly Lough suggests that the intellectual significance of the contributors is to be sought in the context of the first two decades of the reign of Louis XV, when this generation flowered, and not in that of the Revolution in which longevity became the most distinguishing quality of this talented group.

It is in his topical treatment of the authorship of the articles that Lough demonstrates most clearly his mastery of the complex materials. Here some readers may be surprised, for instance, to find that Holbach's main contribution lay in the realms of mineralogy and metallurgy. Yet, Diderot's own preoccupation with science and technology and his concern to propagate the scientific spirit and the Baconian method are, of course, recognized.

If the progress of knowledge today has almost nullified the value of the *Encyclopédie* articles, the writers' attempt to give a rational explanation of the universe remains as impressive a collective effort as ever; and Lough's volume is indispensable for the specialist in assessing their undertaking.

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JOHN LOUGH. *The Contributors to the Encyclopédie*. London: Grand and Cutler. 1973. Pp. 120. £1.90.

In some respects this valuable reference work stands in the same relationship to the monumental *Inventory of Diderot's 'Encyclopédie'* of Richard Schwab as the four-volume text of the *Supplément* to the *Encyclopédie* did to the seventeen volumes of the original work itself. In particular, John Lough's book complements and extends the author index of the known contributors provided by Schwab.

Drawing upon the findings of scholars such as

KEITH MICHAEL BAKER. *Condorcet: From Natural Philosophy to Social Mathematics*. Chicago: University of Chicago Press. 1975. Pp. xiv, 538. \$22.00.

Resisting the temptation to portray Condorcet as the sanguine martyr to Enlightenment ideals, Keith Baker has composed an intellectual biography around a single abstract theme: the philosophe's evolving conception of social science. Much more than the history of a disembodied idea, however, this significant book subtly interweaves Condorcet's published and unpublished thought with

the key episodes of his experience—the early years in Julie de Lespinasse's salon, rise to prominence in the Paris Academy of Science, admission to Turgot's short-lived administration, emergence as permanent secretary of the Academy, and, finally, tragic return to political life during the Revolution. Condorcet's social science depended upon participation of the intellectual in public affairs. Therefore, Baker also expends considerable effort linking Condorcet's thought to the great political and social questions of late eighteenth-century France.

The vicissitudes of Condorcet's mature career convinced him that physical science offered social science the model for creating an enlightened political and social order. The conviction derived from skepticism concerning the certainty of scientific truth. Physical science coaxed Nature into revealing not her hidden essence, but rather her probable workings relative to human observation and analysis. Therefore, could not similar observation and analysis be applied to the conduct of human affairs? Armed with the calculus of probabilities as his appropriate measuring device for evaluating human experience and conduct, Condorcet harbored no sanguine illusions about a scientific elite imposing moral perfectibility upon an untutored world. Physical properties submit far more easily to observation and ordering than do human affairs. Nevertheless, Condorcet believed that in the calculus of probabilities he had found an essential epistemological link between the moral and physical sciences. Thus, for Baker, the climax of Condorcet's work was not necessarily his paeon to the idea of progress, the *Esquisse d'un tableau historique des progrès de l'esprit humain* (1793), but rather the *Essai sur l'application de l'analyse à la probabilité des décisions rendues à la pluralité des voix* (1785). The *Essai* represented Condorcet's major attempt at reconciling the responsible decision-making of an enlightened elite with the principle of voter consent, the problem which, with later emendations, became the philosophe's enduring legacy regarding political and social thought.

No summary review can do justice to this immensely rich, at times overwhelming, book. It is occasionally repetitious. A few lengthy segments, such as the elaboration of Condorcet's calculus of consent, discussion of citizenship education under the Old Regime, or analysis of the historical evolution of probable belief, might have been summarized more concisely. Beside the virtues of *Condorcet*, however, such reservations degenerate into quibbles. Baker's challenge to Koyré's interpretation of the significance of the Newtonian synthesis for the science of man, the author's analysis of the role of the scientist as social authority, his discussion of the reasons for Condorcet's political failure, and his important statements on the dis-

tinctions between Condorcet and Comte, are extremely suggestive. Baker's *Condorcet* is a superb account of a key Enlightenment figure and his France. It complements Cahen and is a worthy companion to Shackleton's *Montesquieu*, Wade's *Voltaire*, and Wilson's *Diderot*.

RAYMOND BIRN
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HENRI GRANGE. *Les Idées de Necker*. Paris: Librairie C. Klincksieck. 1974. Pp. 669.

This massive, enthusiastically partisan study of Jacques Necker's ideas has the indisputable merit of emphasizing the solid intellectual substance behind his career. Like his ministries under the Old Regime and the early Revolution, Necker's thought can be damned for a too-careful balance between more exciting alternatives, but we need to be reminded that he was no mere cautious opportunist, but a thinker whose ideas were positive, wide-ranging, and remarkably consistent. The main concern of Grange's volume is an analysis of Necker's intellectual system as revealed in his many books; there is also some attention to the public reception that these books received, and their subsequent influence and modern relevance.

Grange divides Necker's system into economic and social ideas, political theories, and the defense of religion, and demonstrates convincingly the interrelationship of all three, as stemming from the deplorable but inevitable institution of property and the subsequent authoritarian sanctification of exploitation of the poor by the rich. (Here Grange notes Necker's indebtedness to the bitter social theory of Linguet, who deserves more than five and one-half pages of commentary.) The most crucial portion of Grange's analysis is thus his long section on Necker's economic and social theory, with emphasis upon such themes as the appropriation by employers of surplus value, the indispensability of luxury and of a privileged class as a barrier against centralized despotism, the absolute right of the poor to survival and subsistence, and the obligations of an "interventionist" government to assure such subsistence and the common good. Here Grange makes a case for Necker's originality in steering a positive path between mercantilism, aimed at state grandeur and power, and the economic liberalism of late eighteenth-century theorists. Grange then reviews exhaustively Necker's political theories (largely pragmatic and basically conservative) and religious ideas (as a defense of the status quo, and as a basic theological unity); in the latter field the author insists upon Necker's honesty in seeing the Catholic faith as essential to an irrational, mystery-loving French people. In all

of this, Grange demonstrates an admirable range of intellectual and historical grasp, with only rare lapses. For example, though he is right to stress Necker's basic cultural pessimism, recent research demands more subtlety than Grange's reference to the "unshakable optimism" of Necker's contemporaries. Nevertheless, the work is a significant reevaluation of an important historical figure, and a genuine contribution to intellectual history.

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ROLAND MARX. *La Révolution et les classes sociales en Basse-Alsace: Structures agraires et vente des biens nationaux*. (Commission d'histoire économique et sociale de la Révolution française. Mémoires et documents, 29.) Paris: Bibliothèque Nationale. 1974. Pp. 572.

From 1791 to 1805, the French government sold about one-fifth of the cultivable land in northern Alsace. Of the area sold, 82 percent had belonged to the Catholic clergy, the rest to émigrés; 56 percent seems to have gone ultimately to villagers, many of whom were cultivators, and 44 percent to town dwellers. The business involved 35,000 original transactions and thousands of resales between private persons. The massive documentation in which these facts are recorded, and some questions they suggest, form the core of this book, originally a thesis submitted at Strasbourg in 1969.

Alsace is unique in more ways than most provinces. This book fits usefully into the body of historical writings about the province, but somewhat awkwardly into any larger geographic frame. Marx's interpretation concentrates on totals, averages, and trends for the whole Bas-Rhin, an emphasis which produces a weakness and a strength. The weakness appears in the analysis of local differences, which is sketchy. It could scarcely be thorough and methodical when Marx employs one sample of 139 communes to study local percentages of Catholic church property, another sample of 28 communes to study the social distribution of laymen's property before 1791, another sample of 78 communes to study population increase from 1723 to 1791, and yet another sample of 60 communes to study the social identity of original and ultimate purchasers. These samples range from about 5 to about 25 percent, and overlap in part.

The strength of Marx's interpretation appears in the subtle and instructive discussion of fluctuations in land sales, trends in the property distribution, and the relationships between these and political and religious options. Perhaps most valuable is the large amount of trustworthy quantitative information, which places the Bas-Rhin

with the Nord and the Sarthe among the departments where the revolutionary land sales have been best studied.

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R. R. PALMER, editor and translator. *The School of the French Revolution: A Documentary History of the College of Louis-le-Grand and Its Director, Jean-François Champagné, 1762-1814*. Princeton: Princeton University Press. 1975. Pp. ix, 300. \$15.00.

R. R. Palmer, builder of monumental sculptures, also works in burnished miniatures. His edition of documents concerning the Parisian college of Louis-le-Grand aims his own fifty-year accumulation of historical skill and wisdom at a single institution's half-century encounter with a revolutionary era. The documents, with extensive comments, trace an elite public boy's school from the expulsion of its Jesuit masters and consequent reorganization in 1762-64 through its enthusiastic response to the Revolution—it spent eight years as Equality College and was the only school to remain open continuously through the Revolution—to an inspection tour by First Consul Napoleon Bonaparte in the Year IX; Palmer takes the story to the empire's end via his own summaries and documents concerning the reorganization of educational institutions in general. Along the way we encounter such telling details as the award of a scholar's prize to Maximilien Robespierre in 1781 and the principal's account of a student "insurrection" in 1792. The documents and commentary emphasize the administration and educational thought of Champagné, a Louis-le-Grand graduate and former subdeacon, who directed the school through most of the Revolution and empire. The book closes with Champagné's "Ideas on Public Education" (1790) and a large segment of his "Views on the Organization of Public Instruction . . ." (1800); the two essays provide clear statements—the first more sweeping than the second—of a conception of education as public, egalitarian, and intensely rational. Champagné's proposals of 1800 anticipated the Napoleonic educational reforms, whose effects are still evident today, in several important regards. In Palmer's presentation of the educational contribution of J.-F. Champagné, we witness a marvelous coalescence of author, subject matter, and editor.

CHARLES TILLY
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WILLEM FRIJHOFF and DOMINIQUE JULIA. *École et société dans la France d'Ancien Régime: Quatre exemples Auch, Avallon, Condom et Gisors*. (Cahiers des An-

nales, 35.) Paris: Librairie Armand Colin. 1975. Pp. 118. 32 fr.

R. D. ANDERSON. *Education in France, 1848-1870*. New York: Oxford University Press for Clarendon Press. 1975. Pp. 289. \$25.75.

PAUL RAPHAEL and MAURICE GONTARD. *Hippolyte Fortoul, 1851-1856: Un ministre de l'instruction publique sous l'Empire autoritaire*. Paris: Presses Universitaires de France. 1975. Pp. 344.

As educational systems and practices have been called upon to fulfill a wider range of functions in the modern world, historians have given more attention to the history of education. Enough searching and thorough work has appeared in the past quarter-century to make it unnecessary for the historian in other fields to rely upon myth or cliché when discussing the place of formal education in past eras. The works under review are solid examples of this increasingly significant genre.

École et société dans la France d'Ancien Régime is an essay in historical sociology which examines the data relating to students who enrolled in the secondary schools of four small French cities during the eighteenth century. The authors are concerned to show the students' social origins, the retention and completion rates, the percentage of those who continued their education beyond secondary school, the social mobility of those who attended, and other socioeconomic indicators such as the relationship of enrollment to wheat prices. The brief text is supported by fifty-two graphs, tables, and maps based upon patient and imaginative uses of baptismal, marriage, school, and tax records. The authors are not given to global statements. They support Dainville's findings that there were more lower-class students in these schools than one might have expected. But they add that the opportunities for such schooling for the sons of artisans and peasants seemed to be closing in the years immediately before the Revolution. They find no significant social mobility related to such schooling: the lower-class sons who completed the schools ended in the priesthood, and their mobility had no demographic effect. The authors conclude that the relationship of secondary schooling to social mobility is one which simply assures the social stability of those who take advantage of it, and the pattern persisted through the nineteenth century.

Education in France, 1848-1870 covers every level of formal education and its practical and theoretical relationship to the state and to society. The Ministry of Education during the Second Republic hoped to achieve the often aborted goals of equality and fraternity remembered from the Revolution of 1789. Under Napoleon III it was the fear of those goals which occasionally united liberal bourgeoisie and Catholic conservatives in their ef-

forts to retain an educational system not much different in intent and practice from that of the era of Napoleon I. Those who assume French education to have been a model of Cartesian clarity in structure and operation during the middle of the nineteenth century will be undeceived here. The ideological cross-currents, the unlikely alliances, and the paradoxes abound. All shades of opinion seemed agreed that primary education should be moralistic and Christian for the sake of a stable society. Beyond that there is division on almost every other topic. Napoleon III showed little understanding of or interest in the issues. He wished simply to assure state control while avoiding the loss of clerical support; when there were choices to be made, he valued order over liberty; and the niggardly education budgets were the best indicators of his commitment.

Un Ministre de l'Instruction Publique sous l'Empire autoritaire, Hippolyte Fortoul, 1851-1856 fills a gap noted in Anderson's bibliography. Fortoul is seen here as an impatient opportunist, whose early years in the liberal camp were spent seeking fame, first as a critic and novelist and then as a university professor of literature in the provinces. A political career beckoned in 1848, and he cultivated Prince Napoleon Bonaparte. His reward was the Ministry of Education from 1851 to his death in 1856. Fortoul's plans for the renewal of French education were limited by the parsimony of the government, the anticlericalism of the teachers in the lycées, and the classicism of the professors in the university. He hoped for a unified educational system which would be responsive to the needs of a strong and stable state. Yet the intellectual, spiritual, economic, or social interests of those who had their own intentions for the schools and the university meant that most of his programs were stillborn or short-lived. Fortoul left behind a reputation for a more rigidly centralized system of secondary education and for his political repression of the University. The authors succeed in showing the extraordinary difficulty an education minister would have had in pushing through the thickets of conflicting private and public ambitions during the authoritarian empire and the factors in Fortoul's temper and outlook which found him at his death a self-styled failure.

Anderson's book can become a point of departure for anyone who seeks a judicious and thorough survey of French education from 1848 to 1870; the other two works under review will have to be taken into account by anyone who expects to pursue further specialized study of the periods in question. All three works deserve serious study by the scholar who is not a specialist in the history of education.

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REED G. GEIGER. *The Anzin Coal Company, 1800-1833: Big Business in the Early Stages of the French Industrial Revolution*. Newark: University of Delaware Press; distrib. by Temple University Press, Philadelphia. 1975. Pp. xiii, 345. \$18.00.

Full credit is due Reed Geiger for further extending the thesis that the French Industrial Revolution was evolution. The Anzin Coal Company provides real proof of gradual growth during the 1800-1833 period. Under the leadership of the Desandrouin family and, after 1817, the Perier brothers, the highly profitable monopoly gradually introduced new methods to meet changing conditions and market demand.

The Anzin Company enjoyed a feudal monopoly on its lands in the area north of Valenciennes. A tariff helped to compete with Belgian coal. The market area expanded slowly, based on the river-canal transportation system. Although the company dominated the local and nearby markets, the distant ones, such as Paris, came later. Buyers complained about the monopolistic practices, but the company evidently tried to provide the proper types of coal to the particular industry.

The analysis of the company's operations is effectively expressed in chapters on technical development, geological and operational problems, costs, productivity, and profits, as well as the expansion of mineral concessions. The rich continental coal fields often yielded good-quality coal ranging from semianthracite through volatile bituminous. Thin and fractured seams, explosive gases, and excessive water led, however, to high-cost operations. In turn, this pushed the company to innovations, such as steam pumps and winding machines to raise water and coal from greater depths. Circumstances often forced the company, not always willingly, to divert profits to improvements, with the result that Anzin became a leader in ventilation systems, water control, and safety lamps in the ever-deepening shafts.

The maps are indispensable for locating mining areas, pits, and the river-canal systems for marketing coal. Footnotes, bibliography, and indexing are adequate.

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THOMAS D. BECK. *French Legislators, 1800-1834: A Study in Quantitative History*. Berkeley and Los Angeles: University of California Press. 1974. Pp. x, 202. \$13.75.

Thomas Beck's ambitious study of all the lower houses of the French legislatures from Napoleon's *Corps Législatif* to the Chamber of Deputies elected in 1834 quantifies and analyzes significant aspects

of the relationship between French politics and society during a historically crucial and historiographically controversial period. The Revolution of July 1830 is the "focal center" of the work. Although the quantitative analysis of the 2,862 members of the various Chambers and the attempt to correlate the attributes from which they were elected do not fulfill Beck's ambition to recapture the "true social reality of the period," they do illuminate salient issues in French political and social history.

The major attributes of the legislators include age, "social class" (based on the prerevolutionary definition of Estates), occupation, experience in government, party (Beck understands that contemporary "parties" were political factions rather than disciplined organizations), and percentage of vote obtained. The departmental characteristics are computed from a set of social, economic, and demographic variables that distinguish the more urbanized, wealthier, and "modernizing" departments from the more rural, poorer and "backward" departments.

The major burden of the argument is borne by relating the electoral results, in percentages of deputies returned for each political group, to the various characteristics of the deputies. For example, Beck's examination of Napoleon's housebroken legislators (for whom of course there were no party labels) confirms the view that Napoleon relied at first on what might be called a revolutionary bourgeoisie, but gradually turned to products of the imperial system and to elites with political and social roots in the Old Regime. The moderate royalist deputies who dominated the legislatures of 1816-1820 and 1827-1830 were characteristically, though not exclusively, employed in business and the professions, while the ultraroyalists, notably in the conservative legislature of 1824-1827, were dominated by "traditional elites"—those descended from the Second Estate and attached to the land. By 1834, as far as the electorate was concerned, "The Old France, with its traditional elite, was gone."

Put so baldly these observations may seem commonplace, but as they are worked out in Beck's painstaking analysis they do shed new light on issues that are by no means resolved in the current literature. The more sophisticated manipulation of the departmental data, however—Beck's social, economic and demographic variables—contributes less interesting results. The reader does learn that the political Left, at least in the elections of 1824 and 1827, drew its strength from "the more advanced and wealthy areas," and that the fate of the Bourbons might be read in their loss of support in the more progressive parts of the country. This suggestive observation is diluted by the fact that

there are no such correlations for the elections of 1819, 1820, 1831, and 1834. Beck concludes that the departments "voted in a random manner" in those elections. Presumably, he means that there was a random distribution of votes in relation to his variables. More refined explanations, based on a closer analysis of particular departments, are not in principle precluded, but are not contributed by Beck.

Some of the additional insights and generalizations drawn from Beck's imaginative manipulation of the political and social data are persuasive and illuminating. Others are shallow or debatable, and none can be adequately discussed in the space allotted here. One insight—"... it is obvious something fundamental changed because of the Revolution of July 1830," reassures the reader that realities available to the plain sense of contemporaries, but no longer self-evident to historians, might be recovered through techniques of systematic analysis.

ALAN B. SPITZER
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CHRISTOPHER DUFFY. *Borodino and the War of 1812*. New York: Charles Scribner's Sons. 1973. Pp. 208. \$10.00.

RICHARD HUMBLE. *Napoleon's Peninsular Marshals: A Reassessment*. New York: Taplinger Publishing Company. 1975. Pp. 228. \$10.95.

Christopher Duffy's book presents a general account of Napoleon's invasion of Russia and a more specific analysis of the battle of Borodino and its consequence upon both the French and Russian armies. Although the narrative of the battle is necessarily complex as a result of the multiple battlefield operations, the author has produced well-organized, precise, and clear accounts of the diverse actions at the village of Borodino, at the Raevsky Redoubt, at the Bagration *fleches*, beyond the Kolocha River, etc., explaining the interrelationship and effects of each action upon the other.

The author introduces little new information regarding the battle since his study is based primarily on published Russian collections of eyewitness accounts, supplemented by a limited number of accounts from French participants. He does, however, present the English reader with valuable details and interpretations of the battle, which heretofore have been available only in the Russian language. Duffy has skillfully woven numerous eyewitness accounts, especially Russian, into his narrative to depict accurately the attitudes of the protagonists. The cavalry charges, the assaults along the Russian lines, the field hospitals, the

battlefield carnage, etc., are described with meticulous detail and give the reader a sense of involvement. The character sketches of the Russian generals are perceptive and accurate, and Kutuzov's inactivity at Borodino is carefully presented. The author seems to overemphasize, however, the abilities and contributions of General Barclay de Tolly, considering the latter's inept withdrawal and disorientation beyond Smolensk which would have resulted in disaster if Junot had been equal to the task. No doubt readers will be pleased with the valuable and provocative concluding chapter—an analytical discussion of Russian historiography and, more specifically, current Soviet scholarship on Borodino and the 1812 campaign.

Regarding emphasis, it is unfortunate the author did not devote more than two pages to the numerous and varied causes of the war and reduce the material in the three chapters and appendix pertaining to a discussion of the weapons, tactics, and general composition of the armies. Nevertheless, Duffy's book is a well-written general study of the battle of Borodino; it should serve as a good introduction for further work on the topic.

The second book under review is Richard Humble's reassessment of Napoleon's Peninsular marshals, which is, in fact, a general narrative of the entire Peninsular War. In addition to the biographical sketches of the sixteen marshals who served in the Peninsula, the author discusses each of their campaigns and achievements against the backdrop of Allied opposition. He concludes his study with an evaluation of the marshals and their contributions to the French cause. It is no surprise that Masséna is regarded as "far and away the best field commander... in the Peninsula," followed by Soult, but it is indeed a shock to see that Ney, whose brilliant rear-guard actions saved Masséna's army during the retreat from Portugal, ranked low among the Peninsular marshals. Certainly Wellington, Picton, Napier, etc., had a different opinion of Ney.

The author describes the many personal, logistical, administrative, topographic, and strategic problems encountered by the French commanders in the Peninsula, and he re-evaluates their diverse and often contradictory operations against the Anglo-Portuguese and Spanish armies. Despite the vast scope of this topic and its many ramifications, Humble has written a clear, compact, and lively narrative of the war.

Although the prologue claims the book was written to provide "a look at the Peninsular War from the French side," it does not appear as though any of the French archival sources or many of the basic primary sources published in French or English have been consulted. Moreover, there is little evidence that the information in this volume cannot

be found in other more authoritative works. Occasionally, this study includes errors and misrepresentations in fact or emphasis. Humble's volume can, however, provide an interpretative overview of the Peninsular War and the commanders who served there.

DONALD D. HORWARD
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FRÉDÉRIC BLUCHE. *Le plébiscite des Cent-jours (avril-mai 1815)*. (Centre de Recherches d'Histoire et de Philologie de la IV^e Section de l'École pratique des Hautes Études. Fifth Series, Hautes études médiévales et modernes, 21.) Geneva: Librairie Droz. 1974. Pp. xvi, 149.

Official but undated returns from this plebiscite recorded civilians' votes of 1,303,073 yes and 5,077 no. Frédéric Bluche's research enables him to give corrected totals of 1,308,842 yes and 5,145 no. His recount of all the votes in one-fifth of the departments reveals that the official returns understated their yes vote by .9 percent and their no vote by 6 percent; applying these corrective percentages to the other sixty-nine departments suggests that possibly even Bluche's corrected totals are understated, and the real totals may have been about 1,315,000 yes and about 5,565 no. Clearly, however, there was not in 1815 anything like the massive fraud perpetrated in 1800 by the Ministry of the Interior and analyzed in 1972 by Claude Langlois (*Annales Historiques de la Révolution Française* [XLIV]).

Interpreting political options in any of the Napoleonic plebiscites is a delicate task. The majority response was always abstention. Bluche compares the plebiscites of 1800, 1802, and 1804 with this one, examines large-scale and small-scale geographic differences, with seven maps, and studies the attitudes of influential men—priests, notaries, and mayors. Illuminating though his discussion is, it is not exempt from criticism. For instance, a table (p. 108) gives the percentage of mayors voting yes and the percentage of eligible voters voting yes in twenty-seven departments. Bluche could have calculated the correlation coefficient ($r = 0.434$) which puts his data in a clearer perspective.

There are still discoveries to be made in the Napoleonic plebiscites, but Bluche's is now the best study of any of them and the starting point for further research.

PHILIP DAWSON
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FRANK RICHARDSON. *Napoleon's Death: An Inquest*. With a foreword by JAMES A. ROSS. London: William Kimber. 1975. Pp. 271. £4.95.

This book is an exercise in historical malpractice by a retired British Army surgeon. It is a sequel to his *Napoleon: Bisexual Emperor*, though not so blatantly designed to appeal to iconoclasts and the Gay Liberation Front. The medical opinions that follow have been validated by my friend, Dr. W. N. Adams Smith, Dean of the Medical School of the University of South Carolina.

General Frank Richardson makes a convincing case for Napoleon's having contracted amoebic hepatitis on Saint Helena. If readers accept the fact that he did, however, it remains to be proved that it killed him. Stomach cancer, which the author rejects, still seems the more probable cause of death. All retrospective diagnoses, as Richardson candidly admits, must be based on the findings of the eight physicians present at the autopsy, and the consensus was for cancer.

Nevertheless, if the author had confined himself to the causes of Napoleon's death, he would have had a fair little book of the genre he fancies. Instead, he has included introductory chapters which picture Napoleon as a Hitlerian ogre, selecting his evidence with all the objectivity of a Sir Walter Scott. Then in his epilogue he concludes that Napoleon is immortal. (The love-hate syndrome of a Chateaubriand?) Throughout, he reveals that he has read little on Napoleon published since World War II.

Richardson further spoils his work by rehashing the bisexual emperor business. Napoleon, he says, suffered in later life from pituitary dysplasia, and thus got fat, had unmasculine breasts, etc. *Ergo*, he was bisexual. Nonsense. Probably he did have the malady, but there is *no* evidence to show that it produces abnormal sexual appetites or behavior.

As a historian, Richardson is an irresponsible sensationalist; but it is hard to condemn him personally. He detests tyranny and despotism and places a high value on human life. He praises Wellington's magnanimity toward Napoleon and damns Blücher, who wanted him shot. Richardson is, withal, a man of principle and noble sentiments. A curious mixture, the general. Perhaps indeed there'll always be an England.

OWEN CONNELLY
University of South Carolina

ROBERT J. BEZUCHA. *The Lyon Uprising of 1834: Social and Political Conflict in the Early July Monarchy*. (Harvard Studies in Urban History.) Cambridge, Mass.: Harvard University Press. 1974. Pp. xv, 271. \$12.50.

This is a superb study of the most literate and autonomous community of workingmen in post-revolutionary France, examined at the point where

its social goals came into sharp conflict with the political liberalism, antirepublican fears, and mobilized interests of the Orleanist monarchy. The uprising of 1834 was, Robert Bezucha argues, the last instance of social radicalism within the worker community of Lyons. Its leadership, aroused by the threat that mutualist organizations would be destroyed by the new law on associations, and that a livelihood would be denied them, struck in April of 1834 as they had two months earlier. This time, however, the government was ready to terminate its ambiguous relationship with the social movement. In Lyons, Paris, and a few other centers, the organizations and personnel of the social and political opposition were crushed by massive arrests and a manipulated trial. The Orleanist government insisted that the uprising was manipulated by a republican conspiracy against the constitutional government of the July Monarchy. Neither the Orleanists nor the republicans had a direct interest in presenting the case of the *canuts* (silk weavers) in its own terms.

The radicalism of the Lyons silk workers had its origins under the Old Regime in the tension between silk merchants and weavers. The struggle took form in the demand for a fixed minimum rate for piece-work and adequate representation of the weavers in whatever councils were erected to regulate the industry. Neither demand was satisfied under the Old Regime or the Revolution, and both surfaced again in the grievances of the early 1830s. By then the *canuts*, representing one-quarter of the working force in Lyons, had their own tightly controlled neighborhoods, voluntary associations, newspapers, and community goals.

The silk workers, nonetheless, were politically fragmented—divided among Bonapartist, Carlist, and republican factions. In this condition, their own social efforts were overwhelmed by the larger struggle of Orleanist elites for political and social control of the urban centers. By 1846–48, when the social question was raised again, the *canuts*, weakened by technological changes within the industry and the decline of the international market for silk, no longer constituted the same community with the same demands. 1834 was a watershed in the social history of Lyons.

Within the area of its most obvious social and regional concerns, Bezucha's study clearly complements and merits an unchallenged place beside the work of Fernand Rude and Maurice Garden, who studied Lyons and the silk workers at other moments in their history. For historians concerned with social structure, it commands attention for its analysis of fiscal and census data, along with its use of administrative, arrest, and trial records. Beyond this, however, the author's didactic interest in social theory and comparative history makes

this work an indispensable volume for anyone broadly interested in the social and political processes of industrialization.

DANIEL P. RESNICK
Carnegie-Mellon University

SANFORD ELWITT. *The Making of the Third Republic: Class and Politics in France, 1868–1884*. Baton Rouge: Louisiana State University Press. 1975. Pp. xii, 329. \$17.50.

This book, the result of extensive reading in both the printed and manuscript sources, is a good example of present-day Marxist history which rejects pure economic and money-interest determinism. Sanford Elwitt's thesis is that republican bourgeois politicians founded the Third Republic, shaping it to conform to their class interests. In itself this is not an original conceptualization; it does not go beyond the age-old assertion that most of the revolutions and governments following them since 1789 were "bourgeois."

Where he more decidedly departs from traditional studies is in his perception of class conflict and in his use of local history to discover the areas of bourgeois republican strength. These chapters fit into his main thesis in that they reveal the great effort made by republicans to win over peasants and workers to the ideals of private property, individual freedom, social order, and "solidarity." This latter ideal was, in Elwitt's view, a myth used by bourgeois politicians to deaden class consciousness among workers. These chapters, although informative, are only partly satisfactory since they hardly go beyond electoral geography, failing to explain why republicans succeeded or failed in various locales.

More challenging is the section detailing the connections between republican politicians and industrialists. Elwitt finds the republican leadership coming mainly from a group of manufacturers. As such, they set out to defend industry from the financial oligarchy; they also sought to keep the working class in check lest the peasant and the petty producer, attracted by the oligarchy's moderation, swing to the right. By the late 1870s and early 1880s these republicans had not only achieved their main goal, they had founded a school system to inculcate the young in middle-class ideals, and they had expanded the empire to promote the economic interests of their class. They had transformed France into a bourgeois republic.

But now, what is a bourgeois republic? This expression, often used but rarely explained, is not sufficiently clarified by Elwitt. His idea of the Third Republic seems to identify it chiefly with industrial capitalists. Yet, he insists, they were

only part of the entire bourgeois class which comprised wealthy financiers as well as farmers and "petty producers" such as tailors, carpenters, shoe-makers, and corner retailers. Such a bourgeoisie in the 1870s would have included more than three-fourths of the active population. A bourgeois republic, therefore, was nearly France itself, save for wage workers. And the latter hardly emerge in the book.

For this reason the conflict involved in shaping the Republic was an "intra-class" affair, not one between different classes. Industrialists and their political representatives, supported by capitalist farmers, opposed financiers over issues such as railroads and secular education; over other issues such as tariffs they were in conflict with farmers and merchants. The only real issue on which they could all unite was expansion of the empire. El-witt's insistence on unifying such diverse social and economic strata into one class has disadvantages. It has blinded him to the variety of republican movements after the *seize Mai* crisis of 1877, and his views on the Radical followers of Clemenceau are simply wrong. These weaknesses notwithstanding, his book is both challenging and an important contribution.

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J. NÉRÉ. *The Foreign Policy of France from 1914 to 1945*. (Foreign Policies of the Great Powers.) Boston: Routledge and Kegan Paul. 1975. Pp. ix, 366. \$24.95.

KALERVO HOVI. *Cordon sanitaire or barrière de l'est: The Emergence of the New French Eastern European Alliance Policy, 1917-1919*. (Turun Yliopiston Julkaisuja; Annales Universitatis Turkuensis. Series B, volume 135.) Turku: Turun Yliopisto. 1975. Pp. 244.

Jacques Néré's book, which emphasizes the period from 1919 to 1940, presents a workmanlike analysis of the problems France faced, some of which developed initially when the United States refused to ratify the Treaty of Versailles and the Treaty of Guarantee and when differences arose between Britain and France over German reparations and disarmament. Although he presents a French point of view, Néré indicates the errors of French ways, some committed by diplomats at the Disarmament Conference and in the Ethiopian crisis, others by military men who were too attached to defensive principles based on obsolete concepts, as during the Rhineland crisis, and yet were too rash in moving, without proper protection, into Belgium in 1940. To him, French commitments in Eastern Europe are understandable, but he de-

plores both the blindness of Poland in keeping up conflicts with all of its neighbors, thus making an Eastern Locarno impossible, and the refusal of the Czechs to cooperate with Italy in solving Danubian problems, thereby making a Franco-Italian rapprochement difficult. He regards the Ethiopian crisis as the first confrontation in France between supporters of collective security and those favoring an alliance with Italy. He deprecates putting the blame on Britain for French inaction in March 1936; since the French public was essentially pacifist, and since the military men never intended to cross the German frontier, many in France regarded the reoccupation as of little consequence.

Though the early part of Néré's study is based on memoirs and the major monographic literature, he uses systematically the volumes in *Documents Diplomatiques Français* (1932-37) available when he wrote his book. The appendix of fifty documents, twenty-six of them from D.D.F., will be useful to students who lack access to a first-rate library, or are unable to read French.

Kalervo Hovi's research for his dissertation has covered extensive files in the archives at the Quai d'Orsay and the Public Record Office, in Brussels and Bonn, as well as in Vienna, Copenhagen, and Stockholm. He carefully explores the development of French policy in Eastern Europe from October 1917, when a very moderate view of the Bolshevik Revolution prevailed, until the autumn of 1919. During most of this period France was more concerned about creating a barrier against Germany than preventing the spread of Bolshevism. Although French policies were grounded on cooperation with her Western Allies, decisions about Eastern Europe were largely taken independently. France disliked the Bolsheviks initially because they had destroyed the Franco-Russian alliance and were thereby benefiting Germany; French intervention in southern Russia was undertaken partly to secure French economic interests, but more to eliminate German influence. France disliked British policy in the Baltic, which, on a short-term basis, countenanced the use of German troops to repel Bolshevism; France regarded the former as the greater danger. French aims in establishing a *cordon sanitaire* were more tactical and short-term, as compared with her anti-German stance, which was seen as a long-term necessity. France's whole policy toward the emerging nationalist movements was centered on encouraging them to make alliances with France or among themselves to counterbalance Germany's "structural hegemony" and prevent her further eastward expansion. Poland, and to a lesser degree, Czechoslovakia and Rumania were central to all French plans, but the Baltic states and Ukraine were intended to serve as a secondary line of defense.

France's attitude toward Bela Kun in 1919 was reserved and was dictated by strategic considerations that arose from fears that in opposing him, Hungary's neighbors would be deflected from carrying out the role assigned to them in the *barrière de l'est* system. Hovi's balancing of the various parts of French policy is convincing.

Together these two books add in both substance and interpretation to the historiography on French foreign policy.

MARION C. SINEY
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D. BRUCE MARSHALL. *The French Colonial Myth and Constitution-Making in the Fourth Republic*. New Haven: Yale University Press. 1973. Pp. xii, 363. \$12.50.

Historians who may have looked only at the title of D. Bruce Marshall's book and shunned it as yet another attempt to recapture ephemeral mythology or another dull essay into endless French constitutional conflict should know that the title is misleading. The volume is a lucid and enlightening contribution to French history—colonial, political, intellectual as well as constitutional. The author's purpose is to show how the leaders of the early Fourth Republic tried to hold together the empire threatened by the rising forces of native nationalism and to explain why their efforts failed. He focuses on the two years from the Liberation to the adoption of the new constitution in 1946, but his approach is historical, and his book is a sound introduction to French colonial history, especially the ideologies and policies underlying it in the nineteenth and twentieth centuries.

Marshall defines "the colonial myth" as the belief that France and her colonies were bound together by an indissoluble link based on shared universal ideals of the Rights of Man, that France's *grandeur* depended on retention of the colonies, and that France's responsibilities to raise native peoples to French levels of civilization required maintenance of the Metropole's pre-dominance in the governing of the colonies. The acceptance of this myth by Frenchmen of all parties blinded them to the realities of native nationalisms and, more than any other single influence, caused the delegates to the second constitutional assembly to declare their commitment to liberty, equality, and fraternity for all peoples of the French Union. But the myth also caused them to write into the constitution of the Union both inequality of states (the Metropole remained dominant) and inequality of peoples (metropolitan and colonial French enjoyed more rights than native peoples). The ideals of the myth encouraged aspi-

ration to self-government; the institutions of the Union denied it. This combination assured that native leaders would look to other myths—African socialism and Arab unity—and resort to violence. Perhaps no set of institutions could have held together a nineteenth-century colonial empire in the mid-twentieth century, but the French commitment to their myth made their divestiture of empire particularly bitter and bloody.

Marshall's thesis on the influence of the myth is undergirded by firm knowledge of French political and colonial history, and it is convincingly argued. He does, however, give due weight to other influences on the decisions of the constituent assembly—party politics, De Gaulle's ambitions, the Communist party's complex maneuvers, and the war in Indo-China.

Readers will regret that Marshall did not continue his study to the final agony of empire in the 1960s and consider the fate of the colonial myth as the nation adjusted ideologically to the end of empire. But that would be another book.

DAVID H. PINKNEY
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PEDRO DE MEDINA. *A Navigator's Universe: The Libro de Cosmographia of 1538*. Translated and with an introduction by URSULA LAMB. (Studies in the History of Discoveries.) Chicago: University of Chicago Press for the Newberry Library. 1972. Pp. 224. \$18.50.

Pedro de Medina is best known as the author of the *Arte de Navegar*, a textbook on navigation first published in 1545, with many subsequent editions in Spanish and foreign languages. He wrote also on various other subjects, but his chief interest lay in cosmography and its relevance to nautical practice. This was in fact the subject matter of the manuscript "Libro de cosmographia," which Medina submitted to the king of Spain in 1538, gaining thereby the right to produce charts and instruments under the authority of the Casa de la Contratación in Seville and, eventually, to examine pilots. A copy of this work was discovered in the Bodleian Library in 1959 and is now published for the first time. The handsome facsimile is edited by Ursula Lamb, who contributes an erudite account of Medina's life and writings, a translation of the text, and some notes thereon. The volume is one of the monograph series published under the auspices of the Society for the History of Discoveries.

In the *Libro*, cosmography is presented by way of question and answer, the questions being put by a graduate (*licenciado*) and a pilot and answered by a cosmographer. The result has the tone as well as the content of an elementary manual of instruc-

tion; indeed Medina makes it plain in the dedication that he "wished to aid those who want to know something of that science, and in particular to advise sailors upon the seas." Unlike the *Arte*, however, the present work was presumably never used for teaching, and though doubtless it reflects to some extent what was taught by Medina himself and more generally by other instructors at the Casa at that time, it is largely a theoretical work, lacking most of the features of a typical navigational manual. As a theoretical work, it is of special interest because it attempts to set out in very simple language, and with reference to everyday experience, the collection of concepts a well-educated navigator might reasonably be expected to know. It can therefore tell the reader much about the state of contemporary cosmographical thinking that could not be learned from, say, Copernicus's *De Revolutionibus*, which appeared in 1543. The editor has little to say about this aspect of the work, but one must be grateful to her for placing so interesting a document on view and for describing so well the circumstances in which it was written.

K. R. ANDREWS
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HEINZ GOERKE. *Linnaeus*. Translated from the German by DENVER LINDLEY. New York: Charles Scribner's Sons. 1973. Pp. xi, 178. \$9.95.

Among eighteenth-century scientists none had so great an impact on the field of natural history as the Swede, Carl Linnaeus (1717-78). Book-length biographies of him number more than seventy. Articles about him and his contributions number in excess of 4,000. In this small volume Heinz Goerke provides a readable overview of the man and the scientist: a work written by a physician fluent in Swedish, intimate with primary source materials, and a recognized authority on European medical history.

The work presents the many-faceted Linnaeus: teacher, botanist, zoologist, physician, pharmacologist, and sociologist. The impact of his major publications is analyzed and evaluated. It offers one of the best appraisals of Linnaeus the physician. Linnaeus' contributions to diet as a factor for good health, his classification of diseases, and his studies of disease diagnosis and therapy are well developed. The student of Linnaeus will welcome the abundant citation of reference notes, the accurate chronology of the man's life, and the annotated list of supportive biographical works.

It is regrettable that the volume is reduced substantially from the original German edition (Stuttgart, 1966), and that some of Goerke's erudition as a writer is lost in translation. The illustrations (about half of the number in the German edition)

are reproduced so poorly as to bring discredit to the publisher. The originals which they represent are not out of focus.

The brevity of the biography precluded inclusion of much available anecdotal material so essential to add life to the subject. For this, one must consult the cited works by Blunt, Gourlie, Hagberg, and Jackson. This volume is to be consulted for its accuracy and balance, for its careful analyses of the man and his works, and as an introduction to reliable source materials.

GEORGE H. M. LAWRENCE
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RICHARD MARIUS. *Luther*. Philadelphia: J. B. Lippincott Company. 1974. Pp. 269. \$8.95.

Yet another Luther study appears in the already saturated English market. Why was this book written? Apparently in order to demythologize the image of a hero and bring out the sharp edges—the provincial intolerance, the claim to infallibility as Bible interpreter—in short, the less amiable aspects of the Reformer. Moreover, Luther has "no word to speak to our time" (p. 11). Why then concern oneself with Luther at all? "We study him because he is studied"—a great "bulk of a name heaving itself out of our past with the insistent demand that we pay attention to him" (p. 254). Although there is nothing substantially new in the book, it provides a readable narrative with a penetrating, sometimes superb, if a bit lengthy, analysis of certain key writings. Marius is basically sympathetic to Luther. He dates Luther's insight of justification by faith with Bizer at 1518, thus disagreeing with a majority of Luther scholars who date it to 1513-16 (though this issue has been blurred almost hopelessly by semantics). Like Lortz, Marius points to the almost universal Catholic misunderstanding of Luther's "Faith" as dead faith. On the famous issue of the "nailing" of the Ninety-five Theses he sides with a pupil of Lortz, Iserloh, who rightly doubts the fact.

Criticisms, *inter alia*: Only the literal sense was the basis of dogma in the Middle Ages (Thomas and others). Already Gratian preferred truth to custom. Oberman has rightly doubted that there was Catholic "orthodoxy" in Luther's time, given the many contradictory concepts of justification (Thomist predestinarianism and Nominalist Pelagianism), but also the conflicting theories on infallibility, including the denial of any institutional infallibility by "orthodox" writers. Luther's position at Leipzig in 1519 was objectively nothing novel. The thesis that Luther's anti-Semitic tracts of 1543, contrasting with his tolerant attitude in the 1520s, were a direct consequence of his use of the Bible seems debatable in view of many other

inconsistencies, some even at the core of his message. Could not Luther's propensity to write not systematically but situationally be responsible?

Apparently the author's intellectual roots lie in the Enlightenment, as his appeal to Holbach in connection with the alternative "Auschwitz or Jehovah" or the reference to the Bible as an "absurd collection of folklore and phantasy" shows (ch. 28). He stops, however, where the task should begin. Respectable scholars are trying to determine the historical truth behind that "folklore." The question of Christ is essential to any attempt to make Luther relevant today. Ebeling's respective efforts are dismissed without any serious discussion. Is Marius' attitude reminiscent of the intolerance of the Enlightenment toward other than enlightened views? His book suffers from such one-sidedness. Nevertheless, it is a sobering corrective of an equally one-sided glorification of Luther.

HERMANN SCHUESSLER
University of Maryland,
College Park

MARTIN KITCHEN. *A Military History of Germany: From the Eighteenth Century to the Present Day*. Bloomington: Indiana University Press. 1975. Pp. 384. \$12.50.

The title of Martin Kitchen's new book gives little inkling of its nature and scope. It is not a history of operations or of the evolution of tactical and strategic doctrine. Like Hintze and Delbrück, Kitchen is interested principally in the intimate relationship that exists between a nation's economic, social, and political arrangements and its military institutions and policies. His success in demonstrating this—he shows, for instance, how the failure to reform the Frederician military system before 1806 or to expand the army as the General Staff wished in 1912–13 resulted from fear of the political and social changes that would follow—gives this book greater interest to the general historian than to the military specialist.

This is not to say that the latter will not find good things in it. Kitchen's accounts of the operations against France in 1870 and of the two world wars are concise, but filled with interesting detail; and his analyses of Frederician warfare, of Scharnhorst's reforms, of Helmuth von Moltke's political and military philosophy, of the deficiencies of German strategy in the age of Wilhelm II, and of Hitler's talents as *Feldherr* are shrewd and illuminating. But the most satisfying and original parts of the book are those that deal with the army's role in society: its conflict and accommodation with middle-class liberalism in the nineteenth century; the variety of weapons that it em-

ployed in its fruitless attempt to destroy the militancy of the working class (a subject treated with authority in Kitchen's book on the officer corps in Wilhelmine Germany); its ascendancy in state policy before and during the First World War and the tragic results that flowed from this; the baleful political activities of its leaders Seeckt and Schleicher during the Weimar period; and its debauching and destruction by Hitler, for whose rise it was largely responsible.

Kitchen's "Aftermath" chapter, which deals with the military history of the Federal Republic, is the least successful part of his book. He is convinced that German rearmament was a mistake, caused by the need of the Western military-industrial complex to sustain itself, and that the Bundeswehr's "strategic thinking is based on an anachronistic and manichean world view, [its] structure and armaments are inappropriate for its professed role, [its] ideological basis is suspect, and [its] functional role in post-war German society is open to severe criticism" (pp. 352–53). The arguments that he uses to support these views are not, to this reader, persuasive and seem to be based on questionable assumptions about Soviet intentions, inaccurate estimates of the relation between the strength of the NATO and Warsaw Pact powers, and unjustified intimations concerning the threat posed by the Bundeswehr to West German democracy. The best that can be said of this essay is that it is vigorous and provocative and does not detract from the great merits of the book as a whole.

GORDON A. CRAIG
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WOLFGANG HARDTWIG. *Geschichtsschreibung zwischen Alleuropa und moderner Welt: Jacob Burckhardt in seiner Zeit*. (Schriftenreihe der Historischen Kommission bei der Bayerischen Akademie der Wissenschaften, Schrift 11.) Göttingen: Vandenhoeck & Ruprecht. 1974. Pp. 405.

This significant monograph sets out to discover why Burckhardt's credit as critic of his times stands higher today than that of Ranke, Treitschke, or any other nineteenth-century German historian. The answer is found convincingly in Burckhardt's singular, "Old European" point of view. The author, however, could well have chosen as title, "Between Historicism and Humanism," for he devotes disproportionate space (four of the book's five sections) to basic questions of history's meaning and value. With remarkable analytical precision he shows that Burckhardt accepts the full secularity and historicity of human experience, while recognizing man's cultural continuity, especially in art, as a source of eternally

valid self-knowledge. For Burckhardt, art offers a sacred revelation of man's inwardness. A second creation, transcending history's ordinary limitations, art serves in all ages to purify and elevate man. The author contends, however, that this is not estheticism. Burckhardt's ultimate norm is ethical: *humanitas* or self-determination.

When the author finally turns to the central issue, Burckhardt's perspective on the crisis of his times, he fails to link it securely to the preceding analysis. Nonetheless, he demonstrates that Burckhardt's perspicacity was due to "Old European" political outlook, drawn from the patriarchal, socio-political order of Basel and, above all, the "classical" or "Aristotelian" tradition of natural law. These sources combined to form in Burckhardt an "anachronistic and idealistic" image of a community of citizens conscious of their duty as well as their rights, which he juxtaposed to nineteenth-century reality.

Although filled with insights, this part of the monograph seems less complete than those dealing with historicism. The first topic sentence on page 304, for example, is garbled. Moreover, the author argues ineffectively (p. 290) for "essential development," even "radical changes," in the historian's life and thought. The example given, Burckhardt's liberalism at Berlin, 1841-44, reflects a romantic attachment to his friends' liberated style of life more than to their political outlook. The author a few pages later weakens his argument by citing Burckhardt's critique of French politics in the *Bourgeois Monarchy*, 1843, anticipating most of the points he would make in the 1870s.

Despite its diffused focus and abbreviated treatment of Burckhardt's political outlook, the monograph is the most illuminating single-volume interpretation in at least a generation of the great Swiss historian. Hardtwig's "hermeneutical" method copes expertly with the baffling antinomies of Burckhardt's thought.

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JOHANN JACOBY. *Briefwechsel, 1816-1849*. Edited with introduction by EDMUND SILBERNER. (Veröffentlichungen des Instituts für Sozialgeschichte Braunschweig.) Hanover: Fackelträger Verlag. 1974. Pp. 668.

As one of the leaders of German democracy in the nineteenth century, the Königsberg physician, Johann Jacoby, had to be content with a secondary role in politics as well as in historiography. Publication of his correspondence, of which this is the first of two volumes, is long overdue.

In an introductory essay, Edmund Silberner describes the status of sources on Jacoby as "not at

all favorable," since his legacy has been all but lost because of the war. Most of the letters available to Silberner were copies, not originals. The core of the present edition (material collected by Gustav Mayer and donated to the Jewish National and University Library, Jerusalem) even consists of copies of letter drafts, not actual letters. Given these difficulties, Silberner has produced an impressive edition of documents, most of them previously unpublished.

The collection includes letters to and from Jacoby, letters from his circle of friends, public declarations, poems, and other documents in which Jacoby plays a part. The material's chronological distribution is necessarily uneven. Few documents exist on Jacoby's youth. The bulk of the material covers the revolutionary period. Of the 645 documents assembled in this volume, 167 date from 1848, which Jacoby himself regarded as the zenith of his political life.

Editorial annotations indicate the location of each document and, generally, deviations among various sources. Yet, a comparison of the letters in this edition with those at the *Bundesarchiv*, Frankfurt, reveals quite a few—though insubstantial—discrepancies, mostly limited to interpunctuation and underlining. Other Jacoby material available to the reviewer showed no such discrepancies. Silberner demonstrates in his footnotes patient attention to the minutiae of editing. Extensive biographical explanations deserve special mention. Bibliographical notes are kept deliberately at a bare minimum. Copious annotations pertaining to Jewish history reveal the editor's interest and expertise in this field.

While working on this edition, Silberner published several lengthy biographical articles on Jacoby based on the letters and other unpublished material, some of which—like Lotte Esau's notes on the Jacoby diary—this reviewer had hoped to find included in the present volume. Thus, the edition of letters itself reveals no new startling information about Jacoby. It does, however, provide valuable additional information on a wide variety of problems pertaining to the democratic and liberal opposition of the prerevolutionary and the revolutionary years. The book is without question an important, well-done addition to our printed sources on nineteenth-century German history.

ARNOLD SCHUETZ
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ALBIN GLADEN. *Geschichte der Sozialpolitik in Deutschland: Eine Analyse ihrer Bedingungen, Formen, Zielsetzungen und Auswirkungen*. (Wissenschaftliche Paperbacks, 5; Sozial- und Wirtschafts-geschichte.)

Wiesbaden: Franz Steiner Verlag GmbH. 1974. Pp. x, 207.

One of a series of broad syntheses intended for students of German social and economic history, this volume may be useful to professional historians who want a brief introduction to both standard and recent works on the development of German social policy. The author takes pains to relate this development to "structural" changes in economic and social life and, where his sources are adequate, does so with admirable clarity. His first chapter, dealing with the origins of social policy, is limited to Prussia in the early nineteenth century. Subsequent chapters are uneven. The one on the Hohenzollern Empire is best; those on the Weimar and Nazi periods are thin and disappointing. The book, nevertheless, succeeds as a whole in demonstrating that in spite of the revolutions and political catastrophes which have struck modern Germany, the country's social policy, with its paternalistic tradition, its emphasis on self-help, and its concern for social welfare in the interest of national strength, has a continuity of its own. The book's major defect is its superficial treatment of the results of social welfare measures. The fault is hardly the author's for it is clear from his text and his bibliography that German historians and social scientists have not produced sophisticated studies of this subject comparable to those in which British scholars have examined the achievements and shortcomings of their welfare state.

DONALD G. ROHR
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URIEL TAL. *Christians and Jews in Germany: Religion, Politics, and Ideology in the Second Reich, 1870-1914*. Translated by NOAH JONATHAN JACOBS. Ithaca: Cornell University Press. 1975. Pp. 359. \$19.50.

This is an important book for an understanding of both German history and Jewish history in the modern period. The study of the roots of twentieth-century anti-Semitism in Germany has hitherto concentrated on the *Völkisch* movement, with some attention to the economic and social aspects of anti-Semitism. This study, dealing with a broader range of intellectual attitudes, examines how all major parties (except the Socialists) in the new German state looked upon the efforts of Jews to find a place within it. The majority of German Jews relied on the deceptively simple formula of integration and equality to enable them to be both Germans and Jews. This, the author argues, was unworkable, since those groups which welcomed Jews as individuals (the Liberals) demanded that they renounce Jewish particularism, while those who were willing to accord a certain status at least

to Orthodox Judaism (the Conservatives) did so on the condition that it remain outside the Christian state. Liberal Judaism—and that meant most German Jews—had few friends either among Catholics, who identified it with their enemies in the *Kulturkampf* as well as with a variety of menacing aspects of modern life, or among Liberal Protestants. The latter, though opposed to anti-Semitism, stressed the ethical superiority of Christianity over Judaism; the surprisingly vigorous Jewish attempts to show that the reverse was true were greatly resented.

Christian anti-Semitism was paralleled by the emergence of a more intense and more explicitly racist version with overtly anti-Christian elements as well. To some degree the greater responsibility of the former kept the latter from becoming a mass movement, at least before 1914. It is the great merit of this book that this complex of attitudes is seen not only in terms of the "Jewish Question," but as part of the general problem of German identity and self-definition. In this context the importance of Jewish-Christian relationships for the history of Wilhelmine Germany is made clear. The impressive erudition, the richness of detail, and the lucid style (rendered in a singularly felicitous translation from Hebrew) all contribute to the significance of the book. It will, perhaps, be a difficult work for those not overly familiar with the historical setting and background, both German and Jewish. But then this is not a book for the specialist only, and whatever effort is required to grasp its complexities and subtleties will be well worth the trouble for a wide range of readers.

OLON BEINFELD
Washington University

ROBERT GELLATELY. *The Politics of Economic Despair: Shopkeepers and German Politics, 1890-1914*. (Sage Studies in 20th Century History, volume 1.) Beverly Hills: Sage Publications. 1974. Pp. xvi, 317. \$15.00.

The social and political evolution of the German *Mittelstand* has recently been the subject of several detailed studies. Robert Gellately has added a well-researched account of the social circumstances and political mobilization of German shopkeepers before 1914. An epilogue traces their further radicalization during the Weimar era and links this book to recent regional histories of the early Nazi party.

Gellately warns against treating the *Mittelstand* as a single social entity. Whereas artisans and handicraftsmen suffered greatly from the cyclical economic downswings of the 1880s, retailers mostly enjoyed favorable economic conditions and thrived on cheap factory-produced consumer goods which

threatened the very livelihood of craftsmen. Transportation improvements, rapid urbanization, and increases in workers' disposable incomes greatly expanded the volume of retail trade and encouraged the proliferation of small retail outlets. In the 1890s, however, while the overall economy experienced an upswing, small shopkeepers faced severe competition from each other and from larger department and chain stores, consumer cooperatives, and installment purchase and mail-order firms. More effective organization of shop employees, together with government insurance and other legislation, also squeezed the profits of the small independent retailer.

Shopkeepers defended themselves against these threats. They formed local purchasing cooperatives and discount unions, called for boycotts of factories supplying consumer coops, demanded the establishment of independent chambers of retailers like the Prussian *Handwerkskammern*, and founded regional and national organizations. After 1900, especially, more overtly political organizations tried to mobilize support for legislative protection against "unfair competition." But no single organization managed to unite all shopkeepers. Local evidence from Hamburg reveals the great diversity of political and social views within their ranks. Gellately gives an interesting account of their divisions when Conservative finance proposals caused the dissolution of the Bülow bloc. He also demonstrates the importance of the Saxon organization, led by Theodor Fritsch and others, in steering most shopkeepers into the right-wing "Cartel of Productive Classes" in 1913. Sworn enemies of social democracy, most retailers also believed that their best hope to obtain greater social protection lay in supporting the efforts of big industry and agriculture to preserve the status quo. World War I and the early Weimar years further politicized and radicalized shopkeepers, making them vulnerable to extremists like Hitler.

Although an important contribution, this carefully researched book leaves a few gaps to be filled. Little evidence is given of the impoverishment of retailers or of their social mobility; no distinction is made between the impact of economic fluctuations upon larger and smaller retailers. While, as Gellately shows, anti-Semitism was rife among shopkeepers, no information is given about the numbers and role of Jews in retailing or the relationship between economically motivated anti-Semitism and the biological racism espoused by a man like Fritsch. Finally, the book assumes a fairly simple relationship between economic "despair" and political mobilization. Though convincing for the Weimar period a lot more evidence of retailers' incomes and bankruptcies (in addition to their claims of distress when seeking protective legisla-

tion) is needed to prove the connection for the pre-1914 era. The 1890s saw a veritable mania for political and social organization in Germany, and retailers' activities should be set within this context. Their growing group solidarity might perhaps be viewed as another indication of the emergence after 1895 of an "organized capitalism" in Germany, where traditions of state protection and customary notions cut across free-market ideas in a complicated way. More evidence is needed to show that economic despair was the chief motivating factor in the organizational efforts of shopkeepers before 1914.

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SUSANNE MILLER. *Burgfrieden und Klassenkampf: Die deutsche Sozialdemokratie im Ersten Weltkrieg*. (Beiträge zur Geschichte des Parlamentarismus und der politischen Parteien, number 53.) Düsseldorf: Droste Verlag. 1974. Pp. 440. DM 72.

This useful book describes the development of German Social Democratic policy from the war credits vote in August 1914, to the eve of Germany's military collapse in the fall of 1918. The growth of opposition within the party against the support of the war effort and the resulting split between Majority Socialists and Independent Socialists is the main theme. Most available printed sources and a great many manuscripts and oral testimonies are used. For this reason alone, Miller has made a great contribution to the history of socialism; her book will serve as a guide to further research. Although she apparently has more sympathy for the Independents than for the party majority, her judgment is as well-balanced as one can expect of any historian writing on a controversial subject. In contrast to other writers, she fortunately gives much space to the struggle of the Majority Socialists against annexationism and to their support of the Reichstag peace resolution of July 1917, although in this reviewer's opinion she still underrates the positive significance of that resolution and of the cooperation of the Majority Socialists with liberals and Catholics in the *Interfraktionelle Ausschuss*.

On some points, however, the reader might wish Miller's analysis had gone deeper. She might have explained the fundamental, as distinguished from the mere tactical, differences between the pacifist core of the Independent Socialists and the left radicals—between such men as Hugo Haase on the one hand and Karl Liebknecht and Julian Borchardt on the other. Miller speaks extensively of the differences in the attitude of the two groups to the issues of the day, but except for a few brief

hints the reader does not learn the nature of the philosophical antagonism—that Haase and his friends rejected violence in principle and that the people around Liebknecht and the other radical splinter groups asserted the legitimacy of revolutionary violence, leading to dictatorship. Yet it was this difference which eventually caused the Haase wing to rejoin the Majority Socialists and the other to form the Communist party.

In many ways German party politics during World War I was the prelude to the Weimar Republic, and the actions of parties during the war can be fully evaluated only in light of their consequences during the Weimar period. Susanne Miller announces in her *Nachwort* that she intends to write a history of German Social Democracy in the first postwar years. Aside from the valuable insights the reader will probably gain from this sequel, work on the continuation project may lead the author to a revision of her rather adverse judgment on the merits of the collaboration of the Majority Socialists with liberals and Catholics during the second part of the war. Without the forging of these links in 1917, the Weimar Republic would have lacked a political foundation; specifically, the *Erfüllungspolitik* under Chancellor Wirth, which was supported by both the Independents and the Majority Socialists, would probably never have materialized; and without the rapprochement of the two social democratic parties during the Wirth episode, their unification would hardly have been feasible.

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ALFRED D. LOW. *The Anschluss Movement, 1918–1919 and the Paris Peace Conference*. (Memoirs of the American Philosophical Society, volume 103.) Philadelphia: the Society. 1974. Pp. xiii, 495. \$8.00.

In the past, historians of the Austro-German *Anschluss* movement have concentrated their efforts on the events of the late 1930s and on Nazi Germany's annexation of Austria. Alfred D. Low examines fully and in great detail what might be called the "background to the *Anschluss*," namely the events of the year following the November armistice in 1918. During the first year after the war the waves of a reawakened *grossdeutsch* longing played a major role in the politics of rump Austria—German Austria—and, to a somewhat lesser degree, in the politics of Germany. The Germans, particularly Austrian Germans, took a naive view of Wilson's concept of national self-determination by believing that it would overrule all economic and strategic considerations in the postwar territorial settlements. This German concentration

on the possibility of an Austro-German merger was, as Low points out, an exercise in futility and national frustration for the Germans of both states, particularly for the Austrians. This frustration might have been avoided, along with the resultant irredentism, had the Allies spelled out clearly and quickly the reasons for their determined opposition to this national merger, which, after all, would have amounted to compensation for the territorial losses Germany was to suffer elsewhere. In retrospect, it seems quite clear that the ultimate treaty prohibition of the union was a foregone conclusion, but the author does not as clearly demonstrate that Allied policy was not united enough, nor were the Allies sure enough of their own position and strength to have issued an early public proclamation of their opposition.

Low presents a thoroughly balanced study of this topic, though an introductory chapter tracing the history of the *grossdeutsch* idea during the nineteenth century and the developments of World War I would have provided a clearer setting for the issues that arose after the collapse of the two German monarchies in late 1918. A more detailed examination of German and Austrian newspapers for the 1918–19 period would have further demonstrated the shifting fortunes of the *Anschluss* movement in public opinion. Such additions would have provided support for, but would not have altered, Low's conclusions.

Low brings to the study of this topic considerable background in the history of Central and Eastern Europe and of the history of the succession states in particular. The book under review will soon appear in a German version.

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MODRIS EKSTEINS. *The Limits of Reason: The German Democratic Press and the Collapse of Weimar Democracy*. (Oxford Historical Monographs.) New York: Oxford University Press. 1975. Pp. xi, 337. \$22.50.

Modris Eksteins' book, *The Limits of Reason*, traces the decline and collapse of Weimar Germany not, as has often been done from the viewpoint of politicians and parties, but through the eyes of Germany's prestigious and articulate liberal-democratic press. The work focuses on the history of the three great publishing houses of Sonnemann, Ullstein, and Mosse and their respective newspapers—the venerable and scholarly *Frankfurter Zeitung*, the *Berliner Morgenpost*, *Vossische Zeitung*, and *Berliner Tageblatt*, among others. The family history of these publishing empires, from their inception in pre-imperial Germany to their decline in the Weimar Republic and their death under Hitler, would, in itself, form an engrossing tale. The au-

thor has provided scholars with much new information on the Ullstein family quarrel during the 1920s.

The family fortunes of the Sonnemanns, Ullsteins, and Mosses are only incidental, however, to the main theme of the book. This is a thoughtful and thought-provoking study of German intellectual and political liberalism in the empire, the revolution of 1918-19, and the Weimar Republic. Because of the nature of the German constitutional system of 1871 and the failure of the German liberal revolution of 1848-49, the liberal press in Germany, more so than in Western Europe, consciously cast itself in the role of spearhead and propagator of liberal-democratic reform. This remained the common creed of the great liberal publishing houses of Germany, reaching its noblest definition in Sonnemann's testament of 1909. The heirs of the *Frankfurter Zeitung*, he said, were to remain "politically liberal in social-political matters, always . . . friendly to reform, always inclined to support the economically weak. . . ." The author gives an intriguing account of the union and ultimate divorce of intellectual liberalism, as represented by the metropolitan liberal press, and political liberalism, as organized in the liberal parties. Perhaps the most challenging portion of the work is the question of liberalism's own responsibility in the political disaster of the republic. Impeccable in scholarship and often absorbing in its narrative, *The Limits of Reason* stands out as an important contribution to the understanding of twentieth-century Germany history.

ALEXANDER RUDHART
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THEODOR SCHIEDER. *Hermann Rauschnings "Gespräche mit Hitler" als Geschichtsquelle.* (Rheinisch-Westfälische Akademie der Wissenschaften. Geisteswissenschaften: Vorträge, G-178.) Opladen: Westdeutscher Verlag. 1972. Pp. 91. DM 11.70.

Hermann Rauschnig, a German nationalist turned Nazi (1931), served as president of the free city of Danzig in 1933-34. Because of the city's importance, Rauschnig was allowed to participate in a number of discussions with Hitler in the early 1930s. Subsequently disillusioned with the Nazis and their political squabbling, he broke with Hitler in 1934 and went into exile. After writing a theoretical denunciation of Nazism (*The Revolution of Nihilism*), he published an account of his conversations with the Führer (*Gespräche mit Hitler*) in 1939-40. A best seller in many languages, the *Gespräche* was for long the only volume purporting to present the essence of Nazism in the Führer's own words. In the postwar period, however, as other and more readily authenticated expressions of Hit-

ler's ideas became available, historians raised serious questions about the validity of Rauschnig's account.

Theodor Schieder has now attempted to determine the reliability of Rauschnig's *Gespräche* by searching for cross-references in the German archives and also by comparing Rauschnig's Hitler with the picture of the Führer which emerges from other sources. Though his pursuit of documentary check points turned up meager results, he was more successful in showing that Rauschnig's account fits tolerably well into the sweep of documentary Hitleriana extending from 1928 (*Hitler's Second Book*) to 1944-45 (*Hitler's Table Talk*). Aside from the general reservations that must arise whenever one document or source is compared with another, however, Schieder's vindication of Rauschnig is not beyond specific criticism. He fails to compare Rauschnig's version of Hitler's views with a recently published account that comes from approximately the same period (Edouard Calic, *Unmasked: Two Confidential Interviews with Hitler in 1931*). Furthermore, Schieder readily accepts at face value Rauschnig's explanations of his own motives and actions. Yet here and there where it is possible to check these assertions, the results do not always enhance one's confidence in Rauschnig. For example, Rauschnig repeatedly claimed, and Schieder accepts his assertions as valid, that in the postwar period he refused to aid the Allied war-crimes prosecutors because he held that the Nuremberg trial concept was faulty. In fact, however, during the summer of 1945 Rauschnig declared his readiness to testify and in a twelve-page summary specified the points he would be willing to make on behalf of the prosecution. Some may be ready to dismiss this as a trifling inconsistency, but if we are to extend much credence to Rauschnig's accuracy and his memory regarding Hitler's statements, it is difficult not to be troubled by errors or half-truths in the assertions he made about himself.

Schieder concludes that although Rauschnig's *Gespräche* does not constitute a verbatim record of Hitler's views, it may serve as a general summing up of those views in the early 1930s. This is a fairly guarded assessment, but in the opinion of this reviewer, a historian might do well to exercise even more caution than Schieder.

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ERNST NOLTE. *Deutschland und der Kalte Krieg.* Munich: R. Piper & Co. Verlag. 1974. Pp. 755. DM 64.

Miracles do happen. Before reading Ernst Nolte's book, this reviewer would have considered it im-

possible still to write a Hegelian history in which events are determined by the logic of concepts. Such, however, is the underlying assumption of this book. According to Nolte, for example, the Bolshevik Revolution was neither regularly Marxist, because it did not happen in a highly industrialized country, nor entirely irregular because Russia was no longer an exclusively agricultural economy. For this reason, the Russian Revolution had to arouse a reaction that also was impure, a mixture of non-Marxist elements. "This answer, when it originated, received first accidentally the name Fascism, and if it had not originated . . . it could have been constructed in thought" (p. 130). Because of this Hegelian approach, the gist of Nolte's book, despite its awesome length of 755 pages, is a simple thesis: the Cold War began because at the end of the Second World War, Germany found itself split into two parts, and the Cold War ended when the German governments acknowledged this situation, that is, they accepted the existence of two German states—when the fact of partition had been raised into consciousness.

This spiritualized notion of history gives Nolte's book peculiar features. He places the events from 1945 to 1972 in a broad framework: the war of the Greeks against the Persians; the creation of an Eastern and a Western Empire through the coronation of Charlemagne; the origin of a Left and a Right in Europe in the eighteenth century, accentuated by the establishment of the United States. All these events, according to Nolte, fed into the Cold War. The chronological and spatial extension of the content, however, also serves the purpose of terminological clarification. The widening of the historical horizon permits Nolte to compare similar phenomena of different times, to distinguish among them, and, particularly, to separate the Cold War from other warlike struggles and tensions. Nolte is concerned with a clear definition of his concepts; he is aware that the concatenation of ideas, which he sees behind the factual course of events, requires terminological precision.

The book is concerned with ideas and the change of ideas during the Cold War. A reader expecting a detailed treatment of diplomatic negotiations and governmental actions will be disappointed. Nolte aims at describing the "atmosphere" in which events develop and not the events. Furthermore, when he discusses diplomatic documents, the accent is on discovering whether, in their formulation, they reflect a new "self-understanding," rather than on their practical or tactical purposes. Nolte's preferred sources are materials which document the development of public opinion: newspapers, periodicals, books, speeches, movements in political parties and at universities. The

value of Nolte's analyses is sharply diminished, however, by his method, or his lack of method, in the use of these sources. There is no systematic, certainly no quantitative, research about the treatment given to Cold War issues in leading organs of public opinion—an approach that might not only have demonstrated a gradual evolution in thinking about the Cold War, but also indicated the forces determining this thinking. He is selective and gives attention only to those statements that link the unfolding of the logical process to the final outcome. Walter Lippmann, George Kennan, Eugene Rostow, Paul Sweezy, Stanley Hoffmann, and Sidney Hook are some of the writers and publicists of this country whose statements Nolte uses to indicate the various stages in thinking about the Cold War. Because quotations are taken very much out of context, the reader gets no clear notion whether the quoted statements are representative of politically important groups or express merely a personal conviction. For instance, statements from Kennan's writings are cited to document a variety of divergent positions and are not connected with the events which influenced the evolution of his thought.

Nolte's method of basing his story on expressions of public opinion meets particular difficulties when he begins to analyze Russian policy. The few published manifestations of Russian political thinking are overinterpreted, and Nolte's procedure reminds one of the excesses of our own "Kremlinology." As can be expected, his method is most successful in outlining the development of political thinking in Western Germany during the Cold War period; here too, the lack of systematic analysis and an impressionistic approach placing too much emphasis on the author's own experiences at the Universities of Marburg and Berlin make the results of Nolte's discussion tentative. But Nolte suggests connections which students of post-war Germany will find useful to follow up. German affairs also produce Nolte's one excursion into institutional history—an informative description of the bureaucratic apparatus created in Western and Eastern Germany for the conduct of the Cold War.

It is not without purpose that Nolte has placed the events of the Cold War in a broad framework, including the ancient world, and stressed only those facts which were steps toward the war's logical conclusion. This is contemporary history, written *sub specie aeternitatis*; Nolte tries to isolate and emphasize those facts on which, he assumes, historians of later centuries will focus for a comprehension of the history of our time. But the domination of the entire story by its outcome creates the impression that political leaders were confronted by a chess problem which, at the end,

they were able to solve. Nolte eliminates the moral element from political actions and describes them as if they were pure technics. Events are not bound up with personalities, with economic interests, with different modes of thinking or political traditions and values; everything is interchangeable if external circumstances are the same. If the Communist party in the United States had been as large as that of Germany, Roosevelt would have sympathized with anti-Semitism (p. 160); the only difference between Auschwitz and the American conduct in Vietnam was that the Americans could protest, the Germans could not (p. 528); anti-Semitism can be regarded as a by-product of capitalism and can develop anywhere. Nolte does not justify crimes and vices, but in considering all happenings in the political area from a functional point of view, he denies the impact of moral considerations, emotions, and values in political life. It may be praiseworthy to be objective, to "extinguish one's self," but when, as in Nolte's case, the fanaticism for objectivity goes so far as to look upon things from a height which makes differences between icebergs and volcanoes no longer discernible, the picture is wrong. One may take a revisionist attitude on the Cold War, as this reviewer does. Nevertheless, it cannot be denied that moral convictions, fears, and economic interests were involved in the crises which punctuated the years of the Cold War and played their role in the deadlocked end. A history of the Cold War which does not consider and evaluate the importance of these factors is not true to life.

There are reasons for this distorted perspective. Nolte's book is more than another study of contemporary events; if it is not the first, it is at least one of the first German historical books after the Second World War which returns to the traditional, nationalistic pattern of pre-World-War German historical writing. The book deals with "Germany and the Cold War," not only because from the German angle some new light might be thrown on the events, but also because Nolte considers Germany the center of the Cold War and the German acceptance of their country's partition as the decisive event concluding the Cold War. The message of the book is that Germany has become again an important center of the political world. But the decisiveness of Germany's role in the Cold War, which, according to Nolte, previous writers have not recognized, but which to him is evident and clearly a determining factor in his writing this book, actually is a most doubtful assumption. At least, two questions must be raised. The one is whether, in the events of the Cold War, and particularly in the struggle for Berlin, Germany and Berlin were really the objects of the struggle or only symbols of a much wider

global conflict. No serious discussion of this question will be found in Nolte's book. Moreover, as the strange mixture of names which he considers representative of American public opinion shows, Nolte has only foggy ideas about the manner in which policy is made in the United States. He is remarkably vague and unsatisfactory about the global interests and involvements of the United States; without serious examination, he subordinates them to the struggle for Germany.

But there is another question closely connected with the previous one. When did the notion of the need to accept a partition of Germany take hold in the West? Nolte suggests that the recognition of the partition by the two German governments was the crucial event, from which the acceptance of the partition by the other powers followed. But voices—influential voices—willing to accept a divided Germany, were heard among the Western powers since 1946, and one could probably make a good case that, in accepting the division, the two German states, and particularly Western Germany, did not initiate a new development, but gave up a position lost long before.

It is no accident that these questions do not receive the treatment they deserve; the history which would have taken such considerations into account would not have been a logical process. It would have been a very different history into which interests and personalities, emotions and miscalculations, passions and moral considerations would have entered, and in which the varieties of possibilities which existed in these years would have been appropriately weighed. This would have been a book in which the "atmosphere" of these post-war decades would have come much more to life than in the obfuscating dispassionateness of Nolte's book.

FELIX GILBERT

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FRANCESCO CARACCIOLO. *Uffici, difesa e corpi rappresentativi nel Mezzogiorno in età spagnola*. Reggio Calabria: Editori Meridionali Riuniti. 1974. Pp. 264. L. 6,500.

For two hundred years Spain ruled the Kingdom of Naples as a viceroyalty. Gradually, during this period the ancient rights of the Neapolitan people, guaranteed by Ferrante of Aragón in 1483 and confirmed in two edicts by Charles V, were whittled away. Offices that were to be granted only in consultation with the Neapolitan *Consiglio Collaterale* and other local advisory and judicial bodies were disposed of in Madrid. Taxes eroded the wealth of the kingdom and went to fill the seemingly unending demands of Spanish imperial ambitions. What in the 1500s had been the attempt

of an increasingly absolutist government to create an efficient administration by closely controlling local officials became, in the following century, a venal, corrupt, and cumbersome state bureaucracy.

Working almost exclusively with the rich depository of sources in the Simancas archives, as well as with the collections of Naples, Reggio Calabria, Cosenza, and various other archives and libraries, Francesco Caracciolo has given us a careful study of how Spain ruled Naples and how increasingly the interests, guaranties, and powers of the representatives of the local population were subordinated to purely Spanish policies.

Caracciolo points out that the attempt at monarchical centralization of power and the seventeenth-century crisis were not limited to Naples, but are to be considered in the light of developments throughout Western Europe in the same period. Thus, this study—a careful analysis of governmental practices or malpractices in a small state—adds an important piece to the European mosaic composed of national histories. For students both of Italy and Spain, this work belongs on their library shelf with Koenigsberger's *Government of Sicily under Philip II of Spain: A Study in the Practice of Empire* and the many other works by Italian and foreign scholars on various aspects of life, government, and politics in Naples during the centuries of Spanish rule.

EMILIANA P. NOETHER
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CARLO M. CIPOLLA. *Cristofano and the Plague: A Study in the History of Public Health in the Age of Galileo*. Berkeley and Los Angeles: University of California Press. 1973. Pp. 188. \$7.50.

Epidemic outbreaks of contagious disease—whether Biblical leprosy and medieval plague or smallpox, yellow fever, cholera, and influenza in more recent times—have repeatedly challenged the leaders of Western communities to deal with a fearful threat. Contagion was an obvious phenomenon that had to be countered, even though its mechanism was not understood until the mid-nineteenth century.

The little town of Prato in the Grand Duchy of Tuscany sprang into action at the first news of plague, on October 26, 1629. With only 6,000 inhabitants—17,000 for “greater Prato”—it boasted four physicians and three surgeons. In the emergency, the government appointed four (later eight) lay “health officers,” headed by Cristofano Ceffini. His *Libro della Sanità*, composed shortly after the epidemic subsided, has provided Carlo Cipolla with unique source material.

Prato, on orders from Florence, took the usual

preventive measures. It stationed guards at all mountain passes, fords, and city-gates, isolated the houses where patients had died, and quarantined their families for twenty-two days. A pest-house and a convalescent home, requisitioned after intensive haggling, were staffed and run at municipal expense. Cristofano supervised the feeding of the survivors, who lived in boarded-up houses, the burial of the dead, the fumigation of their homes, and the burning of their belongings. Cristofano reports that twenty-five percent of Prato's inhabitants died of the plague in 1630–31.

In Carlo Cipolla, Cristofano found an appreciative chronicler, even a friend. Economist, historian, demographer, professor at the Universities of Pisa and California at Berkeley, and increasingly intrigued by the impact of epidemic disease on European communities, Cipolla relishes Cristofano's exact figures, his parsimony, his public spiritedness, his circumspection and common sense. While Defoe or Camus may tell us more about the psychological impact of plague, and Bowsky gives us a wider purview in *The Black Death*, this fine short book, written with verve and sharp critical acumen, with its eight factual appendices and a full—mostly Italian—bibliography, should serve to tempt more investigators into the field of public health and medicine where social historians can reap rich harvests.

DORA B. WEINER
Manhattanville College

GAETANO FALZONE. *La Sicilia nella politica mediterranea delle grandi potenze*. Palermo: S. F. Flaccovio, Editore. 1974. Pp. 428. L. 8,000.

This thick octavo paperback tells how the Sicilian 1848–49 bid for separation from mainland rule fell disastrously afoul of Anglo-French rivalries in the Mediterranean. Falzone teaches at the University of Palermo, and his study, twenty-five years in the making, complements nicely two volumes edited by Federico Curato on the Sicilian revolutionaries' correspondence with London and Paris. While Curato shows the Sicilians through their own diplomatic papers, Falzone elaborates the French connection as preserved in the Quay d'Orsay files. A proper balance of 250 pages of commentary in Italian and 150 of documents in French offers both narrative and source material elucidating the episode. The author looks at public opinion in Sicily on the eve of 1848, the uprising of January 12, the mission of Lord Minto, the policy of the Second French Republic, the collapse of Neapolitan rule, the election of a Piedmontese candidate to the throne (he refused), the Bourbon Ferdinand's bombardment of Messina, the French arrange-

ments for a cease fire, and the fall of the provisional government. His conclusion is blunt: "Sicily was by fate just one element of the Mediterranean game, hence the bitter revolutionary lesson warned that only through rejoining the peninsula could the island hope to preserve itself from the very rude injuries of international rivalry." Falzone is recommended.

DUANE KOENIG
University of Miami

SIDNEY SONNINO. *Diario*. Volume 1, 1866-1912, edited by BENJAMIN F. BROWN, introduction by GIORGIO SPINI; volume 2, 1914-1916, edited by PIETRO PASTORELLI; volume 3, 1916-1922, edited by PIETRO PASTORELLI. Bari: Editori Laterza; Lawrence: University Press of Kansas. 1972. Pp. xlv, 534; xii, 374; xi, 407. \$35.00 the set.

SIDNEY SONNINO. *Scritti e discorsi extraparlamentari*. Volume 1, 1870-1902; volume 2, 1903-1920. Edited by BENJAMIN F. BROWN. Bari: Editore Laterza; Lawrence: University Press of Kansas. 1972. Pp. xxii, 932; xv, 936-1716. \$45.00 the set. *Diario* and *Scritti*, \$75.00 the set.

Sidney Sonnino had charge of the Italian foreign office from November 1914 to June 1920. So long a tenure, preceded by almost half a century of active participation in Italian politics—he was twice briefly prime minister—makes him a personage of unusual importance to whom surprisingly little attention has been given. This has been partly due to the fact that, except for the collection of his parliamentary speeches, few of Sonnino's writings could be found. That the present collection should be the product of American scholarship, points up the increasing role played by Americans in European historiography. The finding of Sonnino's papers in a hidden backroom of his villa at Montespertoli near Florence makes a fascinating tale of sleuthing, which is attractively related in Spini's introduction. The five volumes under review consist of two distinct parts: three of them the diary, covering the period 1866-1922; the other two some seventeen hundred pages of his writings and speeches other than those delivered in parliament. Through the latter, one can relive virtually the entire period of prefascist united Italy. Sonnino's interests were broad, and he was a cultured man. His two essays on the *Divine Comedy*, especially the one on Beatrice, reveal a facet of sensitivity not usually associated with him.

But Sonnino was above all a political animal. Although often regarded as a rigid conservative, his direct experience with the Paris Commune of 1871 made a lasting and conditioning impression on him. His concern with the "social question" remained ever alive; very early he advocated extending the franchise, even to illiterates, a very

radical position at the time. The problem of the South and the issue of land tenure in general loomed large among his preoccupations.

On the score of state finance, however, he definitely believed in the orthodoxy of balanced budgets. Wedded to all aspects of free enterprise, he saw in socialism a great potential menace. Interestingly, he would also have subscribed to the Gambettan slogan, *le cléricisme voilà l'ennemi*, and he felt that the existing ambiguity of relations with the Vatican, embodied in the Law of Guarantees, was the best possible arrangement, far preferable to a formal agreement.

As Minister of Foreign Affairs, Sonnino's views on foreign policy are of greatest interest. His earlier position on the score of Italy's place among the powers forecast his subsequent activity. Like most other directors of Italian foreign policy, Sonnino's appreciation was a moderate one based on a realistic appraisal of Italian capabilities. He had believed in the Austro-German alliance but, once at the foreign office, he became convinced that the Allied camp offered the better prospects.

Sonnino was cautious, believing in the letter of the law. The American intervention in World War I and the Wilsonian program for peace raised some obvious problems for Italy. Even before that intervention he saw Wilson's activity as designed "already to secure a place at the peace congress" (*Diario*, 3: 103). One can only regret the absence of comments on the Fourteen Points speech and of any mention of the discussions between the Allies and Colonel House in connection with the German armistice. It was on that occasion that the ambiguity arose concerning Point IX—"a readjustment of the frontiers of Italy should be effected along clearly recognizable lines of nationality"—that was to bedevil the subsequent peacemaking and lead to the Italo-American clash in April 1919. Nor is there any mention of Wilson's visit to Italy in December 1918, and surprisingly little on the peace conference. Sonnino would have taken his stand on the 1915 Treaty of London, a more solid position than Orlando's bungled attempt at compromise, though little more likely to have proved successful. Sonnino himself signed the Treaty of Versailles, but by that time the Orlando ministry had already fallen, an event that virtually ended Sonnino's political activity.

RENÉ ALBRECHT-CARRIÉ
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C. J. LOWE and F. MARZARI. *Italian Foreign Policy, 1870-1940*. (Foreign Policies of the Great Powers.) Boston: Routledge and Kegan Paul. 1975. Pp. xi, 476.

This is a contribution to a series which already includes volumes on British, French, and Austro-

Hungarian diplomacy. Each book offers a synthesis of the latest research supplemented by the results of the authors' own forays into the archives, and also contains an English-language appendix of the most important documents.

Certain shortcomings inherent in the series format, which have been noted by reviewers of earlier volumes, are present here. First, it is not immediately apparent for whom *Italian Foreign Policy* is intended. Too detailed to attract the layman or undergraduate, the book does not, however, tell the specialist much that he does not already know. To be fair, the graduate student, aspirant to become a specialist, will find it an invaluable reference work; hence, an audience does exist, albeit a limited one. Second, the rationale behind the choice of some topics to receive documentation from primary sources and others not remains unclear. Sometimes a monograph is cited; sometimes documentary material which can be found in the footnotes of the same monograph. Fundamentally this work, and series, are synoptic; it might be better to admit it more openly.

These caveats aside, this volume merits high praise. Starting from the conventional description of Italy as the "sixth wheel" of European diplomacy, Lowe and Marzari trace the interplay of two Italian schools of thought. One, cognizant of Italy's economic and strategic deficiencies, strongest in the north and preoccupied with the German problem whether Hapsburg or Nazi, urged diplomatic caution. The other, hypernationalist and radical, dreamed of glory and empire to the south in Africa regardless of Italian capacities. The former, in the persons of Visconti Venosta, Robilant, San Giuliano, and the permanent staff of the foreign ministry, tended to hold sway before 1914, as they sought and secured maximum "flexibility" within the Triple Alliance (p. 71). The latter, heralded by Crispi, found its true champion in Mussolini who "substituted a strategy for a policy . . . , until the means, the acquisition of strength, became an end in itself" (p. 209).

The skill with which these two themes are handled reveals the loss to the historical profession occasioned by the premature death of the authors—both Canadian scholars in mid-career who have died recently just as their joint work was reaching fruition. This sad coincidence must be held to account for some rough edges on an otherwise commendable book: an abrupt conclusion, misspellings in the notes, some missing German titles in the bibliography. Secondary characters are not always identified in the text; and the index, surely the publisher's responsibility, omits many of the *dramatis personae*.

ALAN CASSELS
McMaster University

DORA MARUCCO. *Arturo Labriola e il sindacalismo rivoluzionario in Italia*. (Studi, number 10.) Turin: Fondazione Luigi Einaudi. 1970. Pp. 350. L. 4,000.

This is the first full-length biography of an interesting, amazingly contradictory, and ultimately not very important man. Consider the following information: born in Naples in 1873; already a member of the Neapolitan anarcho-socialistic left in the mid-1890s; forced into exile after the *fatti* of 1898; student of Pareto in Switzerland; a major leader of the revolutionary syndicalist movement to 1908; a passionate proponent of the Libyan War of 1911; and, of course, an interventionist in the Great War.

And this is only the beginning! This veteran deputy of four legislatures—as an "independent" socialist, 1913–21—became for a time a champion of the Russian Revolution, defined as an "interesting experiment in a government exclusively of the working classes" (p. 249). Then, to the astonishment of all, in 1920 he became the Minister of Labor in the last Giolitti government.

A characteristic example of Arturo Labriola's tergiversations was his attitude toward fascism and the fascist regime. In early 1922, straining to emphasize what was "positive" in fascism, he insisted that "fascism does not fight socialism, but the socialist party; not democracy, but its institutions and the traditional democratic party. It would itself like to be a new democracy and a new socialism" (p. 280). The presence within fascism of his old syndicalist comrades (Orano, Lanzillo, Rossoni, Panunzio) undoubtedly influenced this judgment. On the other hand, Dora Marucco's research has revealed that Labriola made one of the most eloquent and timely protests against fascism. On November 12, 1921, he wrote to V. E. Orlando: "I've been in Rome during these last tumultuous days and I left with a sensation of horror which still oppresses me. . . . To sum it up: why pay taxes, have recourse to justice, participate in elections and have a government when we permit the construction of an armed power, fascism, in the country. . . . If you do not decide to disarm fascism, then fascism will destroy the State: either directly by ruining the State, or indirectly by provoking an armed resistance of the socialists and communists which has now become even a question of the right to life" (pp. 328–29). Several times during the next couple of years, Labriola stressed to Giolitti and others that fascism as a "private army" could be defeated only by armed force. That this was not done is a measure of the bankruptcy of the liberal state of that time.

And yet by 1926 he is negotiating with the fascists to exchange his promise of a withdrawal from politics for either a legitimate passport or permission to accept an offer to teach in Italy. The

government declined the bargain, and Labriola escaped to France. There and in Belgium he quarreled incessantly with antifascists of all colors, openly defended the Regime with regard to the Abyssinian War, and returned to Italy in 1936.

After the Second World War, he became a member of the Constituent Assembly and a Senator by right because of his four terms as a deputy in the prefascist parliament. He was one of the very few Italian politicians not belonging to the "extreme left" who vigorously attacked the Atlantic Pact. On this ground, in 1956, he ended his political career as an "independent" in the Communist list for the municipal council of Naples!

There are many fine things in this biography of that paradoxical figure. Marucco's work is strongest in its exposition of Labriola's career to 1908 as a revolutionary syndicalist, especially in her demonstration of that movement's weakness as an independent force in Italian labor. Unfortunately, however, she does not explain satisfactorily (perhaps no one could!) the chameleon-like character of his life. Ultimately, it is hard to improve on Filippo Turati's judgment of 1925 that Arturo Labriola "will always be a headache to whatever party he belongs."

JOHN M. GAMMETT
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ORNELLA CONFESSORE. *Conservatorismo politico e riformismo religioso: La "Rassegna Nazionale" dal 1898 al 1908*. (Saggi, number 101.) Bologna: Il Mulino, 1971. Pp. xii, 466. L. 6,000.

ANGELO GAMBASIN. *Parroci e contadini nel Veneto alla fine dell'Ottocento*. (Biblioteca di storia sociale, 1.) Rome: Edizioni di Storia e Letteratura, 1973. Pp. 316. L. 9,000.

No one has ever doubted the importance of the Catholic Church in modern Italian society; yet until a few years ago, only a handful of books had explored the subject seriously. Recently, scores of useful studies have treated particular groups of Catholics, the Church and politics, and religious life in various regions. Each of these books represents an important aspect of that historiographical vitality. Confessore studies the ideas of an influential group and with finesse assesses their political and religious attitudes (appropriately, Fausto Fonzi wrote the introduction). Gambasin uses records of episcopal surveys to build a pioneering analysis of the relation between religious behavior and social structure in the Veneto (appropriately, Gabriele de Rosa wrote the introduction to this volume, the first in a new series).

Both books are based on impressive original re-

search; both have footnotes nearly as extensive as the text, connecting each point with other studies of every sort; both are methodologically self-conscious, and both deal with the critical period around the turn of the century when Italy's liberal regime was rocked by growing social and political conflict.

Confessore studies the *Rassegna Nazionale* in its most vital period, 1898-1908 (it was published from 1879 to 1943 and again from 1950 to 1953), describing both its public positions and the voluminous correspondence of its small group of supporters. Catholic patriots who dreamed of reconciling a conservative Risorgimento with a Church attuned to modern ideas, theirs is a history of loyalties baffled and frustrated. They were tainted with "Americanism" when that was condemned by Leo XIII, tarnished and frightened by Pius X's campaign against modernism ten years later. Consistently drawn to a strong state that would combat socialism, strikes, and the vaguer threat of democracy, they could comprehend neither the attacks on Catholics launched by Di Rudini and Pelloux nor the rising Christian Democratic movements. By 1908 they attempted to launch a Conservative Reform party, just as Giolitti was widening the scope of Italian politics. Despite reassuring connections with the royal family, the men of the *Rassegna Nazionale* were increasingly isolated. Their social connections made them important to contemporaries (their subscribers never numbered more than 600), but it is their religious and political isolation that is historically significant. That, for all her balanced and careful exposition, Ornella Confessore does not adequately explain.

Gambasin on the other hand, who has written about Catholics in the Veneto before, writes history from below (de Rosa's introduction even includes a bow to the *Annales* school). He constructs maps of the percentages of the population making regular confession (a range from 40 to 96 percent) and from these identifies three zones in addition to urban areas, and then describes each in terms of geography, land-holding, and religious practice. He depicts a lower clergy active, anti-liberal, and opposed to the aristocracy, battling local superstitions and dangerous ideas spread by returning migrants and by freemasons and socialists in dance halls and bars. A mine of insights, this painstakingly assembled book shows much about both the Church's vitality and the universal conviction that it was mortally threatened. Fundamentally, however, the connections made between religious practice and social conditions are those the priests perceived, and the emphasis is therefore on the Church's response rather than Catholicism as a social agent. Social reality and the views of the clergy become so entangled that Gambasin's in-

telligent and quite credible descriptions, region by region, remain un compelling and impressionistic. An appendix rich in statistics and documents even offers a basis for alternative explanations.

Thus both books provide new information about and important insights into the complicated responses of Italian Catholics (from the preoccupation with migration to the influence of Rosminian ideas) to a society experiencing disruptive change. But neither work is clear as to how its findings should be applied. Although sophisticated and informed, they fall short of the kinds of argument that could lead to the generalizations that historians still lack.

RAYMOND GREW
University of Michigan

OTTAVIO BARIÉ. *Luigi Albertini*. (La vita sociale della nuova Italia, volume 21.) Turin: Unione Tipografico-Editrice Torinese. 1972. Pp. x, 569. L. 7,000.

The Albertini family allowed Ottavio Barié sole access to the Albertini Archives in Milan to facilitate the writing of this biography. Barié subsequently edited a four-volume set of Albertini's correspondence. The work currently under review helps place Albertini on the center stage of Italian history where he belongs. English-speaking scholars recognize Luigi Albertini as the author of the highly acclaimed *Origins of the War of 1914*, but not enough is known of his tremendously influential role as co-owner and director of Milan's *Corriere della Sera*, nor of his stature as one of Italy's leading liberals. This book is valuable not only because of the light it sheds upon Albertini, but also because it adds to the continuing debate on the Giolittian era and the origins of fascism, although the author misses the opportunity to place Albertini and fascism in a broader European context.

It is Albertini's opposition to the democratic liberalism of Giovanni Giolitti and his position in the crisis of liberalism and the rise of fascism that chiefly merit Barié's attention. He overemphasizes Albertini's "abstract moralism" as the basis of the ever-growing hostility to Giolitti, concluding that it was a question of methods more than the threatened consequences. This leads to the related contention that the *Corriere* was "an organ of information" rather than a political organ. In fact from 1898 to the triumph of fascism in 1922, Albertini and his prestigious daily called not only for law and order and the preservation of the Liberal State of the Risorgimento, but also for the continued exclusion of "subversive forces" outside the *Statuto*, including both socialist and Catholic masses. The *Corriere* consistently reflected the conservative lib-

eral tendency of the Italian ruling class. Giolittian policies of reconciliation with the laboring classes represented for Albertini the death of liberal Italy.

In the author's chapter on the fascist period, he has marshalled the historical facts and attempts to deal objectively with a complex phenomenon—an ambitious undertaking which is exhaustive, but not definitive. The analysis is flawed not so much because it is more or less an apology for his protagonist's initial support of fascist reaction (after all, Albertini redeemed himself in the eyes of his most vociferous critics for his courageous antifascist opposition from 1923 to 1925), but because it fails to deal with Albertinian conservative liberalism along lines indicated in the evidence contained in preceding pages. Albertini tragically witnessed the fruits of his rigid adherence to classical nineteenth-century liberalism. Always antiapocalyptic in nature, his liberalism opposed democratic liberalism, and opted for authority and order to resolve Italy's woes. This was evident during the Libyan conflict and the nationalistic effusion of the period. It was heightened during the Great War. And when Mussolini offered what seemed to be a viable alternative to Giolittianism, Albertini, who underwent another turn to the right after the peace, welcomed it as an antidote.

Barié spends nearly one hundred pages explaining the reasons for Albertini's "genuine myopia"—his familiarity with only the Milanese "official" fascism of Mussolini and *Il Popolo*, rather than the violent agrarian fascism of Emilia; Albertini's preoccupation with the Adriatic Question, and his absence as a delegate to the Washington Naval Conference; the failure of the ruling class to oppose fascism at first, although Albertini's position was more guarded and limited than, for example, Salandra's; and the excuse that no one could have known what a disastrous path fascism would take in the future. Albertini, Barié concludes, opposed belligerent nationalism, but he saw fascism as the extreme of a national party emerging during the interventionist struggle of 1914-15. He also had to direct himself to his middle-class constituency (referring, of course, to the "santa reazione" of middle-class public opinion to the fascists). Finally, Albertini thought fascism could be normalized and constitutionalized and save Italy from the Red menace.

There is lacking in this interpretation a sense of the organic crisis of Italian liberalism in this era, the incapacity of the governing class and its institutions to deal effectively with either democracy or fascism. With reference to Albertini specifically, the nationalist rhetoric he published in the *Corriere* (especially that of his dear friend, Gabriele D'Annunzio, a relationship treated superficially by Barié) during the Libyan conflict and the Great

War helped create a climate of brute force and cannot be cancelled out by his later renunciation of irredentism. His postponed attacks against Mussolini cannot erase his responsibility in preaching for strong countermeasures to rule by Giolitti or collaboration with socialists. He always took what he considered the moral position, based on faith in political formulas that had succeeded in the past for the historical Right, and he was forced to deal with the results in the turbulent first quarter of the twentieth century when those formulas simply did not work.

It is hoped that Barié's vast study will lead to further research on Albertini and to a better understanding of fascism and of the Giolittian era, a rich and complex period in Italian history, which was once, like Luigi Albertini, a neglected child of historical scholarship.

PAUL J. DEVENDITTIS
Nassau Community College

MELTON S. DAVIS. *Who Defends Rome? The Forty-five Days, July 25-September 8, 1943*. New York: Dial Press, 1972. Pp. xii, 560. \$12.50.

This is an overlong, popularized account of the period from the coup d'état that overthrew Benito Mussolini's Fascist dictatorship on July 25, 1943, to the announcement of the Italian armistice with the Allies on September 8, the eve of the invasion at Salerno. During those forty-five days two aged Machiavellians, Victor Emmanuel III and Marshal Pietro Badoglio, headed a royal dictatorship which sought ineptly to negotiate a *volte-face* with the Anglo-Americans while trying to reassure Hitler that Italy remained loyal to the Axis. A few hours after Badoglio broadcast news of the armistice, he and the panic-stricken royal family fled from Rome in a motor caravan to the Adriatic coast, leaving behind no coherent orders for defending the capital from a German take-over.

The day-by-day events of this period are recounted in cinematographic fashion by the author, an American journalist who has spent many years in Rome as a correspondent for the Mutual Broadcasting System. Davis has also written *All Rome Trembled* (New York, 1957), a journalistic account of the Wilma Montesi scandal that rocked Italy's Christian Democratic government in 1954. In his most recent book, Davis spares no criticism of the stupidity, duplicity, and even cowardice of many of Italy's leaders during the summer of 1943. His technique is to set forth in successive chapters accounts of what was going on concurrently in Italian government and opposition circles as well as in Allied and German headquarters. With an eye for colorful conversation and interesting though often inconsequential details, Davis has

pieced together his book from an impressive array of printed sources and, in certain cases, personal interviews. Numerous photographs are included.

Apart from its excessive length, the book's chief shortcoming is a failure to place the "forty-five days" in the larger context of Italian history. One finishes the book without having gained a clear picture of why Italy found herself in such a mess in mid-1943 or what the political and military repercussions of this interlude were going to be. Although Davis's account may interest the general reader who has some acquaintance with Italy, it does not replace the more scholarly studies of F. W. Deakin, *The Brutal Friendship* (New York, rev. ed., 1966); Col. Albert N. Garland and Howard McGaw Smyth, *Sicily and the Surrender of Italy* (Washington, 1965); and Luigi Ganapini and Massimo Legnani, *L'Italia dei quarantacinque giorni* (Milan, 1969).

CHARLES F. DELZELL
Vanderbilt University

I. B. GREKOV. *Vostochnaia evropa i upadok zolotoi ordy (na rubezhe XIV-XV vv.)* [Eastern Europe and the Downfall of the Golden Horde (at the Turn of the Fourteenth and Fifteenth Centuries)]. (Akademiia Nauk SSSR, Institut Slavianovedeniia i Balkanistiki.) Moscow: Izdatel'stvo "Nauka." 1975. Pp. 519. 1 r. 93 k.

I. B. Grekov, the son of Academician B. D. Grekov, is a senior fellow (*starshii nauchnyi sotrudnik*) of the *Institut slavianovedeniia*. His ambitious monograph can be divided into two parts. The first (pp. 13-310, with the brief Conclusion, pp. 483-87) is a detailed and analytic political history of Eastern Europe—including Poland and the Golden Horde—emphasizing the parallel evolution of the Grand Principalities of Vladimir and Lithuania in the late fourteenth and early fifteenth centuries. The second part (pp. 311-482) is a wide-ranging discussion of Muscovite ideology during the same period. Both sections (occasionally verbatim) derive from and expand upon Grekov's earlier works, the former on his *Ocherki po istorii mezhdunarodnykh otnoshenii Vostochnoi Evropy XIV-XVI vv.* (1963), pp. 13-118, and the latter from his articles in *Sovetskoe slavianovedenie* (1970, number 6) and in *Pol'sha i Rus'* (1974).

Grekov emphasizes the impact of the complex interaction of all East European states and principalities. He shows how the changing configuration of East European politics influenced the centrifugal and centripetal forces and tendencies toward multi-ethnic or mono-ethnic polities which were everywhere at work. Grekov's forte is reinterpretation of seemingly minor events so that they reflect

and validate much larger processes and international relations; the events leading up to and following the battle of Kulikovo (1380) are an excellent case in point. He sometimes minimizes internal causation, however, and over-rationalizes, as, for example, his assertion that every "Tatar" raid was a result of conscious Horde policy based on the balance of power in Eastern Europe. Moreover, Grekov sometimes slights the full picture of major events, such as the Union of Kiew, or processes, such as the weakening of the Golden Horde alluded to in his title. There are obviously no surprises in the outcome—the ascension of Moscow, the decline of Lithuania and the Golden Horde—but since Grekov had earlier dated the fragmentation of the Golden Horde to the mid-fifteenth century, 1420 is not a propitious point to end this narrative. In addition, specialists will quite properly raise innumerable questions of detail, since Grekov's willingness to force the evidence and to rely on highly indirect substantiation, is well-known.

The inclusion in the book of a discussion of Muscovite ideology is justified by Grekov's demonstration of the intimate connection between ideological texts and political history. His premise that differing presentations of the same event, e.g. the battle of Kulikovo, in different sources embody alternative ideological programs or phases reacting to a rapidly changing political reality, are fully warranted. His datings and interpretations, however, of various monuments will arouse controversy. Besides, Grekov does not discuss several issues that require more attention, such as the Russian appreciation of the Chingissid blood legitimacy. But Grekov's close textual reading and fertile imagination often permit him to draw hitherto unnoticed convincing and illuminating connections.

The monograph is based upon comprehensive use of published sources and an extensive bibliography, although there are a few curious and inexplicable omissions. Most importantly, Grekov writes with flair. The book makes stimulating reading.

CHARLES J. HALPERIN
Indiana University

E. PAMLÉNYI, editor. *Social-Economic Researches on the History of East-Central Europe*. (Studia Historica Academiae Scientiarum Hungaricae, number 62.) Budapest: Akadémiai Kiadó. 1970. Pp. 231. \$7.20.

This collection of four essays deserves recognition, however belated, because of the remarkable contribution in English by Laszlo Katus. While among the other three essays there are two coauthored by

Hungary's justly renowned economic historians, Iván Berend and György Ránki, the findings of their useful essay in German on Hungarian national income and capital accumulation, 1867–1914, are available, along with much more, in their *Hungary, A Century of Economic Development* (1974). Their essay in Russian on the Industrial Revolution in Eastern Europe likewise appears in a more expanded form in their *Economic Development in East-Central Europe in the 19th and 20th Centuries* (1974).

The great strength of Katus' essay is its scrupulous use of statistics, with 36 pages of invaluable tables and graphs appended, to measure the extent and nature of "modern economic growth," precisely as Simon Kuznets defined it, in dualist Hungary between 1867 and 1914. Finding an annual growth rate over this long period of 2.2 percent in real per capita terms for a near equivalent of GNP, Katus has established that the Hungarian economy achieved a Rostowian "take-off" before World War I, comparable in Eastern Europe only to the Russian and Czech experiences. Gains in productivity accounted for fully half of this average increase, principally through more intensive agriculture, in contrast to Russia, and through the rise of railway-related heavy industry. These impressive gains in productivity lead beyond physical increases in labor and capital to the improved quality of labor and the size of the market as well as the introduction of better machinery, as Katus demonstrates. He emphasizes the pre-1914 improvement in literacy and technical training and goes so far as to stress the economic advantages of Hungarian inclusion in the Habsburg Dual Monarchy, until recently an unthinkable position among Hungarian scholars. Among these advantages, the author argues convincingly, were free movement of factors of production, including skilled labor, and the free market of 50 million people furnished by the monarchy's customs union.

A concluding section compares pre-1914 Hungarian growth to that of the rest of Europe and places the author's work in Marxist focus. Katus suggests that continuing Hungarian backwardness in 1914, relative to the developed economies, should not be blamed on the nineteenth-century experience within the Habsburg monarchy, but rather on the Hungarian role of agricultural supplier to Western and Central Europe. This role evolved during the early modern period, according to post-1945 Hungarian historiography and to the recent and much less original than advertised works of Immanuel Wallerstein.

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JOSEPH ROTHSCHILD. *East Central Europe between the Two World Wars*. (A History of East Central Europe, volume 9.) Seattle: University of Washington Press. 1975. Pp. xvii, 420. Cloth \$14.95, paper \$7.95.

This is the ninth volume in a projected eleven-volume multi-author series entitled "A History of East Central Europe."

The term "East Central Europe" is apt to bring an uncomprehending frown to the faces of even many professional historians, for it is quite new. It has come into use only since World War II, and has not gained general acceptance yet. The late Oscar Halecki first promoted it (see, for example, his book *The Limits and Divisions of European History* [New York, 1950]) to designate the area of Europe, which the editors of this series define as having "the eastern linguistic frontier of German- and Italian-speaking peoples on the west, and the political borders of Rus/Russia/the USSR on the east." To designate the same area, some historians still use the term "Central Europe," others the term "Eastern Europe," while journalists, economists, and political scientists often prefer the terminology of the cold war and call it the "Soviet Bloc." Central Europe implies, however, the inclusion of Germany, and Eastern Europe the inclusion of Russia, while the Soviet Bloc implies an area homogeneous in character and internally solid. In reality, East Central Europe has never had any unity until that which was imposed on it by the Soviets after World War II. The solidity of the Soviet Bloc was more apparent than real even in the heyday of Stalinism, and since the death of the Soviet dictator in 1953, all of the countries of East Central Europe have to a varying degree reasserted their individuality.

Historically, East Central Europe has witnessed a great mingling of Slavic, Germanic, Latin, and other peoples. Roman Catholicism and Greek Orthodoxy, but also Protestantism and Islam, have won adherents in the area. Parts of the area for centuries were under the rule of the Ottoman empire, the Habsburg empire, the Russian empire, the Polish-Lithuanian commonwealth, Prussia, and the mercantile republic of Venice, all of which have left a permanent impress on it. The peoples of the area have reached very different levels of social, cultural, and economic development, from the high level of the industrialized and urbanized Czechs to the modest level of the pastoral and tribal Albanians.

This great diversity of East Central Europe creates particularly difficult organizational problems for the historian trying to write the history of the entire area. Many historians will question the organization of the series as a whole, as well as of its individual volumes. Despite the wish of the editors to avoid the national approach to the his-

tory of East Central Europe, Rothschild finds it impossible to present an integrated history of the area between the two World Wars and treats it country by country. This reviewer says this, not in criticism but in recognition of the difficulty of the task, as he has taught and written on the history of East Central Europe, and has also found it impossible to abandon the national approach. Rothschild covers in his work Poland, Czechoslovakia, Hungary, Yugoslavia, Romania, Bulgaria, Albania, and the Baltic states. His exclusion of Greece on the ground that it is culturally, economically, and politically more a Mediterranean than a Balkan country is more convincing than his exclusion of Austria on the ground that it is not culturally, economically, and politically part of East Central Europe and that it was absorbed in the problems of *Anschluss* between the World Wars. This is a case of projecting Austria's outlook since World War II when it resolutely turned its face westward back to the interwar period. At that time, however, Austria still had more cultural and economic ties to other successor states, notably Hungary and Czechoslovakia, than, let us say, Prussia, and politically was not completely absorbed in the problems of *Anschluss*, but oscillated in Hamlet-like indecision between *Anschluss* and Habsburg restoration. This reviewer recognizes, however, that a certain amount of arbitrariness is inevitable in delimitating area history.

Rothschild has avoided the pitfall of trying to write a perfectly symmetrical history of the countries of East Central Europe. Instead, he presents a well-balanced treatment, emphasizing those aspects of each country which were most important. Sometimes he sidesteps rather than resolves the difficulties of his disparate subject by use of clichés and fancy terminology (for example, "problematics," when problems would have done just as well). On the whole, however, this book represents a massive achievement and a great advance over the brilliant pioneering work of Hugh Seton-Watson, *Eastern Europe between the Wars, 1918-1941* (London, 1945), and *Independent Eastern Europe: A History* (London, 1962) by C. A. Macartney and A. W. Palmer, which is marred by the blatant bias of its senior author.

VICTOR S. MAMATEY
University of Georgia

EMIL BIEDRZYCKI. *Historia Polaków na Bukowinie* [History of the Poles in Bukovina]. (Zeszyty Naukowe Uniwersytetu Jagiellońskiego, 300. Prace historyczne, 38.) Cracow: the University. 1973. Pp. 260. Zł. 55.

The author, Emil Biedrzycki, is eminently qualified to write this history of Poles in Bukovina. A native of that region, he has long been a specialist

in Romanian literature and language and an interpreter of that culture to Poles at the University of Lwów and later at the Jagiellonian University.

This monograph belongs among those books dealing with closely defined regional and local studies, obviously written for the specialist of East Central Europe and historians of that phenomenon called nationalism. It is a work that treats with only one of the medley of national minorities inhabiting this area known by its historical name (since 1392) of Bukowina (Polish), Bucovina (Romanian), Buchenland (German), and Bukovyna (Ukrainian).

The role of Poles as colonizers in this area dates from the distant past and for some years the connection with Poland has been more than intimate. But it is obviously not the purpose of the author to write the full history of this relationship, as he devotes only eight pages to that period before the First Partition of Poland. He concentrates, rather, on that period between 1869 and 1918 when, under Austrian rule, Poles in Bukovina formed a compact community solidly conscious of its national roots. These feelings were especially strong in the urban area of Czerniowce.

Romanian and German historians have treated Polish contributions to the cultural and economic history of this area only marginally, while Ukrainians have, on the whole, been negative in their assessment. Biedrzycki, for his part, has a tendency to exaggerate the Polish legacy when he affirms the active role of the Poles, and the result frequently is a welter of names on a single page that only serves to distract the reader.

In view of the fact that Bukovina as an entity no longer exists (Northern Bukovina has been annexed by the Ukrainian SSR and Southern Bukovina is now part of Romania) and the Polish minority had long been dispersed, this fault (?) can be excused. Yet what the author intended, and has accomplished admirably, is to write a positive history of this minority before the archival material (scattered through Austria, USSR, and Romania) has perished. There is much of value here to scholars of East Central Europe as Biedrzycki has brought together a multitude of rich source material and given to the specialist an impressive piece of research on a topic of limited scope.

LEO JOHN HACZYNSKI
University of Prince Edward Island

JOZEF PILSUDSKI. *Year 1920 and Its Climax. Battle of Warsaw During the Polish-Soviet War 1919-1920. With the Addition of Soviet Marshal Tukhachevski's March Beyond the Vistula*. London: New York: Pilsudski Institute, 1972. Pp. 283.

This volume was conceived and published by the Pilsudski Institute as a centennial memorial to one

of the principal founders of the post-World War I Polish state. While the memorial is modest, the subject is superb: Pilsudski's finest hour—the victory of his forces over Soviet armies in the battle of Warsaw in August 1920, a triumph that has been called one of the decisive battles of the world. This description is quite accurate, for had they brought Poland under their control, Soviet armies might have gained Germany and the rest of Western Europe, which was then in the midst of a painful economic, political, social, and financial adjustment.

The volume consists of a lengthy introduction that sets the Polish-Soviet struggle in proper perspective; a brief analysis of that struggle by Pilsudski's adversary, Soviet Marshal M. I. Tukhachevsky, delivered early in February 1923, as a lecture at the Moscow Military Academy; and Pilsudski's lengthy response written between April and June 1924. As one might expect, the loser and the victor differ in their assessments of the outcome of the operation. Tukhachevsky attributes early Soviet successes to good reconnaissance, effective propaganda, division among Soviet adversaries, and foreign assistance (from Lithuania, Germany, and pro-Soviet groups in Western Europe), and imputes the final defeat of the operation (whose aim was to sovietize Europe) "not to political but to strategic factors" (p. 263), and to such additional elements as lack of technical equipment, poor communications, overextension, and Western assistance to Poland.

In response, Pilsudski corrects many of Tukhachevsky's facts, figures, and errors; criticizes him for intentional exaggerations or omissions; and presents his own version of Polish distribution of forces, tactical operations, reasons for advances as well as retreats, and numerous other clarifications of events that culminated in the "miracle along the Vistula," which, Pilsudski maintains, was prepared and executed by Poles in behalf of Poland. While Pilsudski's commentary is marred by frequent, but under the circumstances inevitable, elations over Soviet defeat and Polish triumph, the English translation of the battle of Warsaw will be welcomed by all students of military history.

BASIL DMYTRYSHYN
Portland State University

ANDRZEJ PILCH. *Studencki ruch polityczny w Polsce w latach 1932-1939* [The Student Political Movement in Poland, 1932-1939]. (*Zeszyty Naukowe Uniwersytetu Jagiellońskiego*, 299. *Prace historyczne*, 37.) Cracow: the University, 1972. Pp. 207. Zł. 33.

Students in interwar Poland, unlike those in America, were involved in lively, often violent, politics. Pilch's valuable monograph clearly illus-

trates their ideological pluralism even during the seven undemocratic years of prewar Poland. The first part chronologically presents the main political youth movements; the second deals with three issues: the students' stand in defense of academic autonomy against the encroachments of the *sanacja* regime, nationalistic conflicts which culminated in the tragic antisemitism led by the followers of Roman Dmowski, and the antituition struggle in the public schools of higher learning.

Like most works on recent history published in present-day Poland, Pilch's book somewhat distorts the true picture by its emphasis on the allegedly major role of the rather insignificant Communist student movement, which, according to his own figures, never exceeded 200 members, mostly of non-Polish ethnic origin (p. 67). Disregarding the unpopularity among all classes of prewar Poland of Marxism-Leninism and its Muscovite protectors, Pilch asserts that a substantial segment of the population in the Eastern half of Poland strove for unification of West Ukraine and West Byelorussia with the Soviet Republics (p. 150).

Though personally grateful for the author's recognition of this reviewer's role in organizing opposition to the fascistic and antisemitic trend among Lwów students, I can assure him that the "two" leaders of the PMSD (Polish Social Democratic Youth), Lerski and Sołtysik, are but one person since I then used the double name of Lerski-Sołtysik (pp. 125-126). Similarly, Rowmund Piłsudski is not a nephew, but a distant relative of Marshal Piłsudski (p. 32).

In view of his scrupulous listing of all possible Marxist publications and obscure Communist personalities, the author's omission of the seminal role played by such youth periodicals as *Polityka*, which that towering figure of the Polish emigration Jerzy Giedroyc (editor of Paris' *Kultura*) edited, and *Orka na Ugorze* ("Plowing the Fallow"), which the late Jaxa-Rozen and Herling-Grudziński edited, seems ominous. The same applies to his overlooking the important former leaders now active in exile: Gierat, Wierzbiański, Wilk, and Żenczykowski. Such shortcomings have a somewhat marring effect on an otherwise thoroughly researched contribution to explain the turmoil at Polish universities prior to the students' enthusiastic effort to defend their country in September 1939.

GEORGE J. LERSKI
University of San Francisco

BARBARA BOJARSKA. *Eksterminacja inteligencji polskiej na Pomorzu Gdańskim (Wrzesień-grudzień 1939)* [Extermination of the Polish Intelligentsia in the Gdańsk-Pomerania Region (September-December 1939)]. (Badania nad Okupacją Niemiecką w Polsce, number 12.) Poznań: Instytut Zachodni. 1972. Pp. 186. Zł. 35.

Nazi policies and practices in the occupied countries are probably the best-known chapters in the history of World War II. Records of thousands of war-crime trials, innumerable publications based on both scholarly research and personal experiences of survivors, and German secret archives, seized by the Allies and made available to the public, seemingly exhausted the subject. Still, thirty years since the end of the war, the influx of the authenticated data does not stop. It is as if humanity refuses to put its conscience at rest.

Barbara Bojarska limited her research territorially and concentrated on the relatively small area adjacent to the prewar Free City of Danzig. She also limited her study in scope by focusing on Nazi extermination of the Polish leadership: political, cultural, educational, religious, as well as economic. This extermination was not only a result of the corruption of the Nazi officials, but also represented the implementation of a long-range policy, formulated by Hitler himself. This policy aimed at reducing the Polish nation to the status of slaves in his Third Reich. "Only a nation deprived of its leading strata can be lowered to the rank of slaves," he stated. Already on September 12, 1939, while the Polish campaign was still going on, he issued pertinent orders to those of his subordinates who were to deal with the Poles.

The author demonstrates research in depth, undeviating methodology, careful verification of sources and statistical data, and avoidance of side-issues. Her work, if translated into English, would be appreciated by the international academic community. The book's revealing content notwithstanding, the reader will find in it a *vade mecum* which will acquaint him with not only the Polish literature on World War II but also the variety of archival material, both governmental and private, available in Poland for scholarly research.

JAN KARSKI
Georgetown University

ULDIS GĒRMANIS. *Oberst Vācietis und die lettischen Schützen im Weltkrieg und in der Oktoberrevolution.* (Acta Universitatis Stockholmiensis: Stockholm Studies in History, 20.) Stockholm: Almqvist & Wiksell. 1974. Pp. 336.

The much awaited study of the Latvian *strelki* (which first appeared in Swedish as a thesis for a licentiate degree and was then serialized in a Latvian magazine in Canada) has finally appeared in German. The topic of this technically well-presented book is important for all students interested in the Russian Revolution in particular, and Russian history in general. Though the involvement of the Latvian *strelki* in the Russian Revolution is well known in Latvian and Soviet historiography, no book or article about them has ever appeared in a

Western language (with the exception of two chapters devoted to the topic in A. Ezergailis, *The 1917 Revolution in Latvia*).

The story of the *strelki* begins in the summer of 1915 when the high command decided to organize Latvian national troops for use in the Northern Front. The *strelki* began their association with the Bolsheviks soon after the March Revolution and continued it until the end of the Civil War in 1921. Ģērmanis' work, though, takes the story only to the Bolshevik uprising in 1917. He has promised to write a second volume covering the Civil War. In part the title of the work is misleading: though it purports to be a monograph on Colonel Jukums Vācietis (a commander of a *strelki* regiment and later the first commander of the Red Army), the author has very little to say about him. No substantive motivations for Vācietis' involvement with the Bolsheviks are provided. Moreover, the author failed to consult the lengthy autobiographical sketch that Vācietis wrote for the *Deiateli Soiuza Sovetskikh Sotsialisticheskikh Respublik i Oktiabr'skoi Revoliutsii*. Because the author summarizes the events of October in such a schematic manner in the final chapter, he would have been better advised to leave that subject to the next volume. The main contribution of Ģērmanis' work is to summarize the secondary literature that exists in Latvian historiography on the *strelki*. Though the author has done a thorough job of researching memoir literature and some document collections, he has done little other work in primary sources.

Though this book (put in very respectable German by Andrejs Kubulins) is very important to Western scholars as an introductory text, it leaves much to be desired for experts who are acquainted with the original sources. One wishes that Ģērmanis' effort had surpassed the Soviet historian J. Kaimiņš' work *Latvishkiye strelki v bor'be za pobedu Velikoi Oktiabr'skoi Revoliutsii*.

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TARAS HUNCZAK, editor. *Russian Imperialism from Ivan the Great to the Revolution*. With an introduction by HANS KOHN. New Brunswick: Rutgers University Press. 1974. Pp. xi, 396. \$17.50.

The articles in this collection—devoted to an examination of the motives, character, and results of Russian imperialism—are of uneven quality. The contributions of Henry Huttenbach and Walter Leitsch, concerned with Russian policies toward Ukraine and Poland respectively, avoid raising questions about the imperialist nature of these policies. The former does so by stressing, rather, the mutuality of religious and ethnocultural interests between the Ukrainians and the Russians in the seventeenth century and the role played by

some Ukrainians in integrating Ukraine into Russia; the latter does so by viewing Russo-Polish relations up to the late eighteenth century as essentially a clash between two rival empires, with the consequent annihilation of the one by the other. Neither deals with Russian policies in Poland and Ukraine in the nineteenth and twentieth centuries when Russian rule became synonymous with political, economic, and religio-cultural oppression.

Huttenbach, in a second article, again demonstrates his penchant for avoiding the question of imperialism by concerning himself solely with the origins—rather than with the motives—of Muscovite expansion. He limits the discussion of the consequences of this expansion for the subject nationalities of the empire to several superficial and baseless observations, offered almost as afterthoughts—that the “cultural and social autonomy allowed them [the Tatar and Mongol peoples] made the Muscovite yoke bearable.” An article by Ragnhild Hatton, on Russia's expansion to the Baltic from the late fifteenth to the early eighteenth centuries likewise avoids the issue of the imperialist nature of Russia's occupation of the eastern coast of the Baltic, although it mentions in passing Russia's commercial gains from involvement in the Baltic trade. The real value of her contribution, rather, is to be found in the detailed study she presents of the diplomacy and wars involving Russia, Poland, and Sweden for control of the Baltic Sea.

The essay by Emanuel Sarkisyanz, while admitting the existence of Russian imperialism, seeks to discount its deleterious effects on the non-Russian peoples of the empire by noting that other countries were equally imperialistic (an irrelevant point also made by Huttenbach in his article on Muscovy), that not all the non-Russian peoples were exploited to an equal degree, and that economics played a minor role in the “driving motivation” of Russian imperialism, all by way of noting that the true villain in the story is Stalin's “neo-Muscovite and totalitarian imperialism.”

An article by Traian Stoianovich, concerning Russia's involvement in the Balkans, is somewhat confusing. When he discusses Russia's policies there and in the Straits, he sees concrete political-diplomatic, military, and socioeconomic factors at work, the evidence for which constitutes the bulk of the article. But he then goes on to assert—in complete contrast to the prevailing spirit of the otherwise highly informative study—that “Russian domination of the Balkans was primarily ideological.” Only two articles, Firuz Kazemzadeh's “Russian Penetration of the Caucasus” and Geoffrey Wheeler's “Russian Conquest and Colonization of Central Asia,” deal directly and systematically with the subject at hand. Each

recounts the history of Russian military, political, economic, and religio-cultural penetration of the territories under discussion. Each discusses the techniques of direct and indirect control, the manipulation of local peoples and conditions in furtherance of this control, and the subsequent failure of local religio-nationalist elements to free their respective peoples from alien rule. For the peoples of the Caucasus, according to Kazemzadeh, this failure led to either physical extermination or mass deportation. In Central Asia, according to Wheeler, it resulted in the conditioning of the native peoples to submit to a new and presumably equally alien Bolshevik regime. An article by Taras Hunczak on pan-Slav ideology and one by Sung-Hwan Chang on Russian involvement in China and Korea round out the collection.

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L. E. SHEPELEV. *Aksionernye kompanii v Rossii* [Joint Stock Companies in Russia]. (Akademiia Nauk SSSR. Institut Istorii SSSR, Leningradskoe Otdelenie.) Leningrad: Izdatel'stvo "Nauka." 1973. Pp. 346.

Although a definitive history of Russian capitalism has yet to be written, a substantial foundation has been built recently, particularly in the United States, France, and the Soviet Union. Many historians have focused on Russian capitalism's social, entrepreneurial, and institutional history. One may cite the works of I. F. Gindin, P. G. Ryndziunskii, and V. Ia. Laverychev, Roger Portal, Alexander Gerschenkron, and this reviewer. John McKay and René Girault have also treated at length foreign enterprise and investment in Russia. But much of the economic, political and cultural history of Russian capitalism remains to be elucidated.

Shepelev's study provides a unique Soviet contribution to this endeavor. His book is based on exhaustive research in Soviet archives. He has utilized the papers of the St. Petersburg Stock Exchange, as well as dozens of state agencies, industrial enterprises, and banks, and other documents. The author provides a wealth of detail that would be difficult for most Western scholars to come by. Shepelev's stated purpose is essentially descriptive. He explores a subject about which little has previously been written: the centralization of capital in industrializing Russia of the late Tsarist period and the economic policy toward stock companies of the imperial government and of its successor regimes.

The book's chapters can be divided into two groups: those that focus on quantitative aspects of

the stock companies' growth and those that deal with stock corporation and stock exchange regulation.

The first Russian stock corporation, established in 1755, was a diving company operating in the Gulf of Finland. Other stock companies appeared in the early nineteenth century, but the tempo of growth did not accelerate substantially until after the Crimean War, with the founding of hundreds of companies each decade, and then in the boom of the 1890s, when hundreds appeared each year. The early companies were involved with commerce, insurance, textile manufacturing, and food processing, but by the 1870s, banks and railroads had achieved primary importance. By 1914, Russia had 2,263 stock corporations with a capital of over 4.5 billion rubles, compared to 5,488 in Germany, amassing the equivalent of over 8 billion rubles.

The 1890s also witnessed the growth of a large-scale speculative stock market in Russia. An outgrowth of the merchants' exchanges of the early nineteenth century, a sophisticated stock exchange, similar to those of London and Wall Street, had developed in St. Petersburg by the end of the 1890s, with twenty other active exchanges in various cities of the empire. With the stock exchange came the scenes of speculative activity, so familiar in Europe and America.

Nicholas I and Count Kankrin were suspicious of stock companies, which they saw as nests of dishonest speculation. Yet they wished to stimulate industrial growth. This was reflected in the first legislation of 1836, which affirmed the institution of the stock corporation and the limited liability of shareholders, but sought rigorous government control. This legislative and administrative surveillance continued throughout the Tsarist period, and much of Shepelev's book is devoted to an analysis of the most important projects and laws.

The climactic episode of the history of Russian stock companies came in 1917 and 1918, and is briefly described in the final chapters. There was an outburst of financial activity during the period of the Provisional Government, during which 734 stock companies with a capital of almost 2 billion rubles were formed, twice to quadruple the level of stock-company formation in 1913. Beginning with the decree of December 14, 1917, the Soviet government began the seizure of large stock corporations and confiscation of all capital which earned more than 500 rubles per month. Within a year, the whole system had been destroyed.

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UWE LISZKOWSKI, editor. *Russland und Deutschland*. (Kieler historische Studien, number 22.) Stuttgart: Ernst Klett Verlag. 1974. Pp. 334. DM 98.

This *Festschrift* was dedicated to Georg von Rauch on his seventieth birthday. Von Rauch is one of the Federal Republic's leading specialists on the Baltic area and Russia from the seventeenth to the twentieth century. Though best known in this country for the English version of his short history of Soviet Russia, von Rauch's wide-ranging scholarship has touched numerous aspects of pre-1917 history as well. The theme of German-Russian interactions and perceptions provides a common thread for the contributions to this volume.

Many of the pieces are of minor interest. Worth noting are Günter Wiegand's report on Eastern Europe in the late-medieval chronicles of Lübeck; Norbert Angermann's report on German merchants in the trade of seventeenth-century Pskov; and Erik Amburger's discussion of the Wogau-Konzern's role in the economy of Moscow and Russia from the early nineteenth century to 1917. There is food for thought in Klaus Zernack's review of "negative policy toward Poland" in German-Russian diplomacy in the eighteenth century. Michael Garleff and the editor, Uwe Liszkowski, deal with interesting aspects of the change from a positive to a negative perception of Poland in late nineteenth- and early twentieth-century Germany and Russia. The relations of radical political parties are usefully represented by Wolfgang Geierhos' discussion of the German Social Democratic Party's attitude toward Russian revolutionaries in the early 1880s and by Albrecht Buchholtz's sidelights on the career of the important Soviet diplomat, Leonid Krassin.

All these, like the twelve other contributions, a short biography of von Rauch, a list of his publications, and a register of the dissertations prepared under his direction, will be assured a decent burial by the price of the book.

GERHARD L. WEINBERG
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V. V. KABANOV. *Oktiabr' skaia revoliutsiia i kooperatsiia (1917 g. - mart 1919 g.)* [The October Revolution and Cooperative Societies (1917-March 1919)]. (Akademiia Nauk SSSR, Institut Istorii SSSR.) Moscow: Izdatel'stvo "Nauka." 1973. Pp. 293.

Although the October Revolution is generally associated with discontinuity in Russian history, the development of the USSR can be fully understood only if one takes into account the important social and cultural institutions which survived. One of the more significant and neglected of these was the cooperative movement—the producers', consumers', and credit institutions which developed rapidly in the decade before 1917—and came at the height of War Communism to serve some 100 million people through more than 53,000 separate as-

sociations. This small but detailed volume documents the cooperatives' incorporation into the Soviet state system and argues their importance, despite "bourgeois origins," as institutional and cultural precursors of Soviet socialism.

The author has a broad and sympathetic vision. He sees the cooperatives before October as instruments in the struggle against monopoly capitalism and argues they cultivated both an attitude of cooperation among workers and peasants and a spirit of economic independence. Appreciating the administrative expertise of their staffs, he insists on the invaluable service they performed during the Civil War. One gains an insight into the extensiveness of cooperative operations, the humanitarian, non-political commitment of their administrative personnel (many of whom continued to work for the new government), and the complex process by which the Bolsheviks absorbed their distribution and credit apparatus.

This latter process is the author's central focus. After detailed reviews of historiography and sources, he concentrates on cooperative activity in the immediate post-October period and on the Bolshevik policy of "compromise" in the spring of 1918. He then examines the party's "struggle for control," presenting a variety of statistics, noting the problem of political dissent, and discussing the struggle between cooperatives and groups like the committees of poor peasants (*kombedy*). His emphasis is on the consumers' cooperatives, the most extensive cooperative form. But he examines as well the role of cooperative credit institutions, arguing that the level of deposits increased between 1917 and 1918, "evidencing the trust of the petty bourgeois in the proletarian state" (p. 135). Most interesting are figures of overall cooperative growth in this period and the indication they give of the enormous role these institutions played in distributing goods and providing urgently needed funds for production.

Only 1700 copies of this book were published, but it is an informative and suggestive work. Some may be bothered by the author's inflexible class categories, his failure to penetrate the inner workings of cooperatives, his neglect of producers' associations, his failure to follow closely the fate of cooperative staffs, and his excessive concern for Lenin's views. But those interested in either the cooperatives themselves or the process of institutional transformation after October will find it serious, scholarly, and useful. Of special note is the inclusion of an index, rare in Soviet books.

WILLIAM G. ROSENBERG
University of Michigan

A. AVTORKHANOV. *Proiskhozhdenie partokratii* [The Origin of Partocracy]. Volume 1, *TsK i Lenin* [The

Central Committee and Lenin]; volume 2, *Tsk'i Stalin* [The Central Committee and Stalin]. Frankfurt/Main: Possev-Verlag. 1973. Pp. 728; 534. DM 32; DM 23.

In this mammoth narrative of the Bolshevik leadership in Russia, Abdurakhman Avtorkhanov has analyzed the nature of the Soviet regime and the contributions made by Lenin and Stalin, to whom the two volumes are respectively devoted, in creating the system of totalitarian rule that has persisted since the Great Purge.

Serving for the past two decades as a political analyst for Radio Liberty and the Institute for the Study of the USSR in Munich, Avtorkhanov brings the perspective of an eyewitness to these events, for he was a Communist party functionary in the thirties until he became a victim himself of the Great Purge. (He was released from prison in 1942 only to be captured by the Germans shortly thereafter.) His previous works, both drawing on this experience, include *The Reign of Stalin* (1953), under the pen name of Alexander Uralov, focusing on the purges, and *Stalin and the Soviet Communist Party* (1959), centering on Stalin's fight with the Right Opposition and his consolidation of power up to the purges.

Avtorkhanov's stated aims are to explain Lenin's political role, the nature of the post-Lenin succession struggle, and the origins of Stalin's "criminal Bolshevism." In detailing these developments he provides a useful congress-by-congress guide to the debates between the party leadership and the long sequence of opposition factions up to the point when Stalin put an end to all meaningful discussion.

Avtorkhanov credits Lenin, thanks to his "will to personal power" (1:610) and his use of opportunists like Stalin to curb the idealists in the party, with laying down the basic principles of the "partocracy"—"a hierarchical system of absolute political, economic, and ideological power and rule of the party within the party—the apparatus of the CPSU" (1:31). On the other hand, he holds Stalin responsible for a well-planned campaign to destroy the Leninist old guard and the collectively led party in favor of a new system of personalistic terror. A "Leninist more than Lenin himself," Stalin carried the principle of "partocracy" to its logical extreme and to the "ruin of the Leninist Central Committee" (1:62). "That which Lenin started off in embryo," Avtorkhanov observes, "Stalin raised up as a monster" (2:477).

The most interesting portion of Avtorkhanov's work is his detailed description of Stalin's struggle to consolidate his power against the Right Opposition led by Bukharin. This, of course, is the period where the author's account begins to be enlivened

with his observations as a lower-level participant in the actual events. Fortunately, this material appears in his works that were previously published in English.

Avtorkhanov makes few factual slips for a work of this scope. It was Stalin, not Lenin, who said "Marxism is not a dogma but a guide to action" (2:158); the 1927 Platform of the Fifteen was issued not by the Trotskyists but by the separate Democratic Centralist group. Certain statements by the author will be controversial. He alleges that Lenin engineered the assassination of the German ambassador, Count Wilhelm von Mirbach, in 1918 as an excuse to suppress the Left Socialist Revolutionaries (1:506-67). In the maneuvers to suppress Lenin's "Testament" in 1923-24, Avtorkhanov contends that Stalin falsified Lenin's instructions to excuse withholding the document until after Lenin's death (2:47-52).

A much broader question of interpretation is raised by Avtorkhanov's view of Stalin as an almost superhuman political strategist, shrewdly calculating his moves years ahead, according to his "principle of unprincipledness" (2:384), in pursuit of a preconceived goal of total personal power. Such a view is retrospectively persuasive, as many biographical works since Trotsky's *Stalin* testify. It underrates, however, the unpredictable, unfolding nature of politics anywhere and the unforeseeable good fortune Stalin enjoyed in the political naiveté and disunity of his opponents. It also leaves unanswered the question of the extent to which Stalin's principles of "partocracy" are still imbedded in the Soviet system of rule.

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ALBERT S. LINDEMANN. *The 'Red Years': European Socialism versus Bolshevism, 1919-1921*. Berkeley and Los Angeles: University of California Press. 1974. Pp. xviii, 349. \$15.75.

The 'Red Years' is an ambitious undertaking aimed at providing an overview and synthesis of the history of the relationship between the three major Western European socialist parties (German, French, and Italian) and the Bolsheviks between 1919 and 1921.

Given this objective, the book provides not only a great deal of specific information on the connection between the parties but also about larger issues. The latter include how the relationship between Western European socialists and the Bolsheviks influenced the development of the Soviet regime, how the October Revolution affected the socialist parties of Western Europe, and, most important, how in turn the demise of revolutionary hopes in Western Europe shaped Lenin's policies.

While the October Revolution provided inspiration and enthusiasm for socialists in Western Europe, its ambiguities and peculiarly Russian characteristics did not make it easy for Western socialists to develop policies helpful for the cause of the proletarian revolution in their own countries. As the author makes clear, "... the bolsheviks had not made a proletarian socialist revolution in any full or Marxian sense of the term. The only plausible rationale for their takeover in Russia was that they were starting something that necessarily had to be completed in the highly industrialized West, where the material conditions existed upon which to build socialism. . . . They were able to hold power not so much because of the strength of their proletarian backing, but because opposition to them was divided. Russian society was so atomized that a ruthless and terroristic minority could maintain power over it. This was not an auspicious beginning to world socialist revolution" (p. 33).

Deviating somewhat from the conventional assessments of the role of the Bolsheviks, the author also stresses that World War I brought to the surface the schisms and factionalism among Western socialists. These were, in the final analysis, decisive factors in obstructing the development of strong, united socialist movements which could have had a revolutionary potential. With appropriate detachment, Lindemann concludes that the conflict between Western socialists and the Bolsheviks produced "few unsullied heroes," and that the postwar divisions among Western socialists had deep historical roots independent of Bolshevik policies.

The valuable contributions of this work, besides documenting and elaborating its main thesis, include the detailed portrayal of the personalities and policies of various Western socialist leaders, a reassessment of Lenin's attitudes toward Western socialism and world revolution, and the examination of the social and historical conditions which created the particular characteristics of each major Western European socialist party and their various factions. (The author distinguishes among "sycophants," "syndicalists," "opportunists," "independents," and "old Marxists" in regard to attitudes toward the Bolsheviks.)

Given the scope and complexity of the topic and the author's determination to weave together many divergent historical threads, it should not be surprising if the reader is at times left with the impression that certain subtopics could have been more fully discussed and the book as a whole better focused.

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Kritika burzhuaiznoi istoriografii Sovetskogo obshchestva [A Criticism of Bourgeois Historiography of Soviet Society]. Moscow: Izdatel'stvo "Politicheskaya Literatura." 1972. Pp. 411.

This volume of seven chapters, written by nine authors, analyzes the writings of Western scholars, public figures, and journalists about major problems of Soviet history, ranging from the October Revolution to collectivization, the Second World War, and the Soviet post-war era. To a large degree the views and conclusions expressed here will come as no surprise to those already familiar with Soviet impatience with foreign criticism. The footnote citations of Western literature show broad acquaintance with journal literature, memoirs of public figures, and scholarly monographs, but this is also characteristic of much Soviet writing on other, less sensitive topics. While there is a reduction in the use of the harsher terms of the Soviet lexicon such as "falsification" to describe Western views, there are still enough examples to make this a dreary volume to read. In addition, while the West is sharply criticized for its errors of judgment about the Soviet situation, there is an equally extreme approval of the acts and opinions of the USSR.

Nevertheless, Western analyses have, indeed, had their flaws of haste, prejudice, and incomplete use of sources, and at times the authors of this volume have made some telling points. Trying to find them, however, in a book that lacks index and bibliography, and that is, one feels, so insufferably sure of everything as the rest of us wish we were of anything, is perhaps not worth the labor.

ROBERT V. ALLEN
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IWAN KOSZELIWEK. *Mykola Skrypnyk*. New York: Suchasnist Publishers. 1972. Pp. 342.

MYKOLA SKRYPNYK. *Statti i promovy z natsional'noho pytannia*. [Articles and Speeches on the Nationality Question]. Compiled by IWAN KOSZELIWEK. Munich: Suchasnist Publishers. 1974. Pp. 370.

Skrypnyk, an old Bolshevik and close associate of Lenin, was to a great degree responsible for the formulation of the Soviet nationality policy in the 1920s, for the creation of the federative structure of the USSR, for the establishment of the bicameral legislative assembly of the Soviet Union and for the compromise solution of preserving the unity of the Party, but with territorial subdivisions coinciding with the boundaries of the constituent republics.

Skrypnyk had been a ruthless member of the Cheka, and an effective prosecutor in the Ukraine. But he was also firmly convinced—as the result of Bolshevik defeats in the Ukraine—that the Com-

munist regime could survive in territories which were not ethnically Great Russian only if it identified itself with the national and social strivings of the minorities. Fearful of the reactionary tendencies of the members of the Tsarist bureaucracy, many of whom remained in administrative positions during the Bolshevik regime, Skrypnyk argued against the "petit bourgeois" characteristics of the various shades of "Russian imperialists" inside and outside the Communist party. When he became responsible for the educational network in the Ukraine, Skrypnyk started to implement the policy of Ukrainization, trained Party cadres and encouraged, with some success, emigré Ukrainians to return. These policies, which he was always careful to label, with some justification, as Leninist, brought Skrypnyk into conflict with Stalin. Skrypnyk, however, allegedly was not familiar with or chose to disregard Lenin's privately expressed sentiments vis-à-vis the national minorities. In the power struggle which took place after Lenin's death, Stalin won by playing faction against faction. But Skrypnyk, by committing suicide in 1933, foiled plans for a larger show trial which Stalin was preparing. Koszeliwec argues, admittedly with no direct evidence, that Skrypnyk committed suicide as a sign of protest against his own policies. Having become a disillusioned Marxist, Skrypnyk saw suicide not only as a way out, but also as a telling argument against himself.

In the destruction of millions of Ukrainian peasants and thousands of members of the intelligentsia, including the leading members of the Communist party, which followed in the wake of his suicide, Skrypnyk himself became a posthumous enemy of the people. Although he has been officially rehabilitated after de-Stalinization, little has been written about him in the Soviet Union. The Koszeliwec study, based on published sources and a few personal interviews, at times calls the reader's attention to the contemporary situation in the USSR, whenever such parallels seem warranted. But despite the author's comment that his writing might be considered polemical, the study on Skrypnyk is generally free of such overtones. Moreover, the book includes a full bibliography of Skrypnyk's works and an autobiographical sketch.

The companion volume of Skrypnyk's speeches and articles, edited by Koszeliwec, is useful because the material has not been readily available. Skrypnyk was an extremely prolific writer, and Koszeliwec wisely limited the collection to Skrypnyk's contributions on the nationality question. It is a crucial, and still largely overlooked, issue in the study of the USSR and one not likely to be tackled by official Soviet scholars. Moreover,

Skrypnyk is at his oratorical and polemical best when he deals with the nationality question.

MARTHA BOHACHEVSKY-CHOMIAK
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EDUARD WINTER. *Russland und das Papsttum. Part 3, Die Sowjetunion und der Vatikan.* (Akademie der Wissenschaften der DDR. Zentralinstitut für Geschichte, Quellen und Studien zur Geschichte Osteuropas, volume 6.) Berlin: Akademie-Verlag. 1972. Pp. x, 338.

This is the final volume of Eduard Winter's history of relations between Russia and the Papacy from the conversion of the East Slavs to the Soviet period.

Published under the auspices of the Academy of Sciences of the German Democratic Republic, this volume is understandingly inimical to the Vatican. It is worth reading, however, because of the wealth of research materials used by the author, especially the publications by the Holy See and other Catholic institutions, German, Czechoslovak, and Soviet diplomatic documents, various learned journals and pertinent books. Whatever the interpretation, facts are truthfully reported.

Winter sees the Vatican's attitude toward Soviet Russia as guided by hostility toward Communism and the wish to achieve a union with the Russian Greek Orthodox Church. He does not conceal Soviet persecutions (they are not persecutions for him) of Catholics in the Soviet Union, but stresses the resulting enmity of the Vatican. According to Winter, Pius XII considered the USSR as the Vatican's main enemy, while Nazi Germany was the lesser evil. Interestingly enough, while he describes the horrors of Nazi occupation of Eastern Europe and the Soviet Union, he never mentions the Jews.

John XXIII is the only pope whom he praises, because of his conciliatory attitude toward the Soviet Union. Paul VI's policy is characterized as hesitant and contradictory, though he is searching for a *modus vivendi* with the Soviet Union.

The weakness of the book is the author's neglect of the problems of the Church in Eastern Europe, notably in predominantly Catholic Poland, where Paul VI strives to achieve a *modus vivendi* between the Church and the Communist government, often to the displeasure of the Polish hierarchy.

The author is right, however, in arguing that recently both the Soviet government and the Vatican have followed conciliatory policies. Official visits to the Vatican by Foreign Minister A. Gromyko (1965 and 1966) and the chairman of the Presidium of the Supreme Soviet, N. Podgornyi (1967), as well as by representatives of the Russian Greek Orthodox Church (1967, 1968, and 1969)

provide the evidence that Moscow sees better relations with the Vatican as part of its détente policy, while the Vatican hopes to alleviate the situation of Catholics in the Soviet Union, particularly in Lithuania and to reach better relations with the Greek Orthodox Church in the ecumenical spirit.

A historian interested in the Vatican's foreign policy would be well advised to include this book in his research materials.

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M. LEWIN. *Russian Peasants and Soviet Power: A Study of Collectivization*. Translated by IRENE NOVE with the assistance of JOHN BIGGART. With a preface by ALEC NOVE. (The Norton Library.) New York: W. W. Norton and Company. 1975. Pp. 539. \$4.95.

This is an intelligent, lively, and in some respects persuasive book concerning the political, social, and economic background of the Soviet "great turn" toward massive collectivization of peasant agriculture, initiated in the USSR in 1929. On a number of issues—notably on the political conflicts among the left, right, and center factions of the Communist party, which started in 1923/24 and culminated in 1928 with the crushing of the left and right factions—Lewin goes over well-worn terrain. The main interest of this book is in the suggestion that collectivization was the "culmination of a chain reaction" set off by the 1928 crisis in state procurement of agricultural deliveries from the peasants, and that the result of Stalin's forceful, "emergency" solution of this crisis consolidated his own personal power under the form of the "Stalinist totalitarian dictatorship" (pp. 516–17).

The long-standing conflict among the party's factions concerned *precisely* the twin issues of how to industrialize (that is what priorities should be set and what pace should be selected) and what to do with the peasants and private agriculture (that is when, how, and to what extent should agriculture be tapped in order to fuel industrialization). The procurement crisis may or may not have triggered the collectivization drive, but the question had been "in the air." Indeed, the establishment of market relations at the periphery of the state complex, and the acceptance of a privately owned agriculture, inaugurated in 1921 under the name of New Economic Policy (NEP), was not considered by many party leaders as a *permanent* fixture. It was rather a *retreat to be overcome*. Undoubtedly, however, once the collectivization started, Stalin pushed it heedlessly and brutally to its end.

Certainly Stalin's dictatorship came out strengthened from this process. But Lewin entertains disarmingly naive ideas as to what the party

was *before* Stalin fastened his own grip and his own "personality cult." According to Lewin, because of the 1929 turn "the principle of personal loyalty came to take the place of programme, criteria, and analysis, and . . . a Marxist, rationalist, revolutionary Party ended up, by a peculiar twist, with a personality cult" (p. 452). This is also tied to the idea that the party became increasingly bureaucratic, losing as it were its pristine revolutionary character.

Actually, the "bureaucratization" of the party had started from the very first day it grasped power. A highly centralized machine, intensely committed to Lenin's leadership and accustomed to operate through secret committees, the party and the soviets spawned immediately committees and subcommittees completely removed from any "mass" control. Certainly, Lenin was more modest and less crude, though not less brutal with his adversaries; but he ran the party machine in the same highly personal fashion as Stalin, who only accented certain features. These were part and parcel of the party's structure, of its methods, and of its basic policy of imposing its will on the state and on the economy.

Lewin is particularly successful in describing the situation and the mood in the countryside on the eve of the "great turn." He has not taken full advantage, however, of the relatively abundant statistical data available on the socioeconomic structure of the countryside. The *Statistical Yearbook of the USSR for 1928* (Moscow, 1929) for instance, contains some 218 pages on various interesting samples concerning the villages. Through an inspection of these tables, one can readily visualize the smallness of the peasant holdings, the limitation of available capital, the arbitrariness of the division into "poor," "medium," and "wealthy" farmers, and the unbelievable poverty out of which the party squeezed the wherewithal for the great industrialization drive.

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K. ROKOSSOVSKY. *A Soldier's Duty*. Translated from the Russian by VLADIMIR TALMY. Edited by ROBERT DAGLISH. Moscow: Progress Publishers. 1970. Pp. 340.

A. EREMENKO. *The Arduous Beginning*. Translated from the Russian by VIC SCHNEIERSON. Moscow: Progress Publishers. 1966. Pp. 328.

K. A. MERETSKOV. *City Invincible*. Moscow: Progress Publishers. 1970. Pp. 94.

K. A. MERETSKOV. *Serving the People*. Translated from the Russian by DAVID FIDLON. Moscow: Progress Publishers. 1971. Pp. 377.

S. M. SHTEMENKO. *The Soviet General Staff at War, 1941-1945*. Translated from the Russian by ROBERT DAGLISH. Moscow: Progress Publishers. 1970. Pp. 399.

ANDREI GRECHKO. *Battle for the Caucasus*. Translated from the Russian by DAVID FIDLON. Moscow: Progress Publishers. 1971. Pp. 367.

M. V. ZAKHAROV, editor. *Finale: A Retrospective Review of Imperialist Japan's Defeat in 1945*. Translated from the Russian by DAVID SKVIRSKY. Moscow: Progress Publishers. 1972. Pp. 247.

I. S. KONEV. *The Great March of Liberation*. Translated from the Russian by DAVID FIDLON. Moscow: Progress Publishers. 1972. Pp. 286.

A. VASSILEVSKY et al. *Moscow 1941-1942 Stalingrad: Recollections, Stories, Reports*. Compiled by VLADIMIR SEVRUK. Edited by BRYAN BEAN. Moscow: Progress Publishers. 1970.

MANFRED KEHRIG. *Stalingrad, Analyse und Dokumentation einer Schlacht*. Stuttgart: Deutsche Verlags-Anstalt. 1974. Pp. 680.

JÜRGEN FÖRSTER. *Stalingrad Risse im Bündnis, 1942-43*. Freiburg: Verlag Rombach. 1975. Pp. 172.

The many armies that returned home in 1945 marched directly into print, where they have remained since World War II became the most written-about war of all time. Two of the main participants, however, the Soviet Union and Germany, joined the march late. Although the Soviet Army played a major role from 1941 on, nothing of consequence was published while Stalin lived, in part owing to the personality cult and in part to an exaggerated security consciousness. Khrushchev eliminated the first obstacle in his 1956 Twentieth Party Congress speech and at least mitigated the second. The wave of publication that followed was capped by the six-volume *Istoriia Velikoi Otechestvennoi Voyny Sovetskogo Soiuza*. (Moscow, 1960-63). Since the middle 1960s, the Brezhnev-Kosygin regime has allowed the Khrushchev wave to become a flood in volume of print but a mere ripple in terms of new information. While the German Federal Republic did not restrict or control publication of World War II history, German historians lacked access to records until the early 1960s when the captured military documents held in England and the United States were returned to Germany. War history research also had to do without official support before the *Bundeswehr* established the Military History Research Office in the early 1960s. The works considered here represent some of the recent Soviet and German output.

Of the Soviet works, four are memoirs, currently a popular form in the Soviet Union. All were published after 1965. By then, most western generals of memorist caliber had long before banked their

advances and collected their royalties. But the delay was not necessarily a disadvantage for the Soviet generals. Among the present four, only Rokossovsky had "a name" during the war. The others did not achieve real stature until after Stalin died. For the Soviet government the belated publication has, moreover, a positive advantage: information that would have been made public elsewhere twenty-five years or more ago can now be presented as "new."

Out of date by Western standards, the memoirs are nevertheless important to current Soviet World War II historiography. Besides recognizing individual military prominence attained during the war or after, they allow apparently open discussion on a conveniently compartmentalized basis. Each author is free to confess his own shortcomings, which few generals anywhere are inclined to do in excess, and praise or blame his subordinates. He can also take occasional issue with his superiors but only on matters specifically related to his own command. Consequently, the generals write extended vignettes—Eremenko on Mogilev, Smolensk, Briansk, and the Fourth Shock Army; Meretskov on Karelia, the Volkhov, Karelia again, and the Far East; Rokossovsky on a succession of corps, army, and army group commands up and down the front. Shtemenko offers a novelty, a veiled but, particularly in the latter chapters, instructive picture (the only one extant) of the workings of the General Staff.

Not only writers of memoirs, the generals are, for several reasons, also historians. For one, the war is still too important an event in the Soviet Union to be left to mere academicians. For another, not every major figure had a role in the war commensurate with his status in the 1960s. Then too, in the Soviet Union the past must be kept in tune with the present; and, lastly, no doubt there is just the simple pleasure of authorship. Marshal Grechko, for instance, finished the war in command of the First Guards Army, not an unimportant post, but by 1967 he was Minister of Defense of the Soviet Union. In *Battle for the Caucasus* he deals with a whole theater of the war—as a historian rather than as a memoirist, although he commanded armies there. Actually the fighting in the Caucasus was not crucial in the war, but Colonel L. I. Brezhnev was there as a political officer. In Grechko's hands the Caucasus campaign emerges as a model of military operations with implications for the present and the future. *Finale*, edited by Marshal Zakharov, who was Chief of Staff of the Trans-Baikal Front during the war, performs a similar service for the August 1945 offensive against the Japanese in Manchuria. The contributors—one marshal (Malinovsky), two major generals, and three colonels—conclude that the oper-

ation against the Kwantung Army was "the most devastating defeat suffered by the Japanese Army during the Second World War" and that "this was the decisive factor in bringing about Japan's collapse and hastening her final defeat and unconditional surrender." On the atomic bomb they cite a Soviet-sponsored "Japanese" history of the Pacific war which states, "Even the atomic bomb had no effect on [Japanese] state policy as laid down by the Supreme War Council." It is of passing interest that this statement, frequently made in Soviet publications, is always attributed to "Japanese historians." *The Great March of Liberation* features three marshals—Grechko, Konev, and Zakharov—two colonel generals—Sharokin and Zheltov—and a lieutenant general—Telegin—writing on the 1944-45 campaigns in east-central Europe and the Balkans.

Moscow-Stalingrad is an anthology published to commemorate the twenty-fifth anniversary of the victory in Europe. The fifteen selections include memoirs of Marshals Rokossovsky, Vassilevsky, and Zhukov. The rest are a mixture of fiction and reportage by such writers as Simonov, Bergholtz, and Yuri Zhukov. The quality of the writing, translations, and illustrations is markedly higher than in the other books, but, except possibly for color, its usefulness as history is slight.

The two German works are productions of the *Bundeswehr's* Military History Research Office and reflect its approach to World War II history. Both are parts of series that will not, as far as can be seen, add up to comprehensive war histories in the way that the U.S., British, and Soviet official series do. They are elaborately documented, astutely objective, detailed analyses of meticulously excised parts of the war. The authors, apparently have seen and made note of every available document and account in German or Russian, ignoring works in other languages.

Kehrig's *Stalingrad* is a logistical and tactical study of the battle from October 6, 1942, to February 2, 1943, in 545 pages of text with 68 (93 pp.) documentary appendices. Although it is the most extensive and intensive work on the subject to date, it stops without a conclusion at the collapse of the XI Corps on the morning of February 2. The Foreword promises "a historical evaluation . . . when the fundamentals have been mastered."

In the effort to come to terms with the meaning of the defeat, Förster's *Stalingrad* investigates the effects on Germany's allies—Italy, Romania, and Hungary—and on neutral Turkey. Passing lightly over the defeats the allied armies suffered in and around Stalingrad, Förster deals with the friction with the allies, peace feelers, the projected Balkan alliance with Turkey, and Hitler's attempts to

patch up relations in the conferences at the Berghof and Schloss Klessheim in April 1943.

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M. M. KRAVETS'. *Ivan Franko—Istoryk Ukraïny* [Ivan Franko—Historian of the Ukraine]. L'viv: Vydavnytstvo L'vivskoho Universytetu. 1971. Pp. 202.

Ivan Franko (1856–1916), the great Ukrainian poet, was a noted scholar of the history of Ukrainian literature and a writer whose fictional works are not without value for the study of Ukrainian history. This volume, however, is the first to recognize Franko as a historian, though he never presented himself as such and had no formal training in the field. The author, a professor of history at L'viv University, discusses in five chapters Franko's approach to history, his research on feudalism in Ukrainian history, his study of the Ukrainian people in the capitalist epoch, his views on Ukrainian historiography, and, finally, Franko as an editor of Ukrainian historical sources.

In the opening chapter, Kravets' attempts to prove that Franko based his writings on the works of Marx and Engels—with a few admitted deviations (p. 14). He erroneously derives this conclusion from Franko's translation of one chapter of *Kapital* into Ukrainian and from undocumented claims, for example, that Franko saw Ukrainian national aspirations as part of the class struggle (p. 16). True, Franko as a humanitarian was an internationalist, and as a Ukrainian living under Austro-Hungarian rule, he was a revolutionary democrat. Yet in his autobiography, *V Poti Chola* [In the Sweat of the Brow] (1890), Franko wrote of himself at age 21: "I was a socialist merely by sympathy like any peasant, but far from understanding what scientific socialism was." From the same source comes: "I never belonged to that sect of the faithful who founded their socialistic program on . . . class warfare." Dr. Okhrymovych, a colleague and Marxist enthusiast, wrote of the poet at middle age: "Franko took a critical attitude toward Drahomaniv and Marxism and tended to revisionism and Fabianism" (see Percival Cundy, *Ivan Franko* [1948], p. 39).

In the chapters that follow, the author finds Franko's writings essentially in accord with Soviet views and, despite Hrushevsky's studies, the most profound treatment of pre-Soviet Ukrainian history. Kravets', a historian of recent Ukrainian labor and peasant history, while lauding Franko's efforts, notes errors in his writings on the "feudal" and "capitalist" eras and in his editing of ancient historical sources. (Interestingly, Franko did not even allude to feudalism, much less use the word,

in his writings.) In the chapter on Ukrainian historiography Kravets' attempts to show Franko as a historian in dispute with well-known Ukrainian historians, such as Hrushevsky, but the points raised by Kravets' are more or less insignificant (e.g., nonutilization of a journal, p. 181). Finally, though Kravets' could hardly be expected to go against the line of Soviet Academician Pashkov, his undocumented assertion that Franko saw "the bright future of his people" as "union with the Russian people" is simply false. Among other proofs Franko's poem, "Ne Pora" (1880), alone refutes this: "No longer, no longer should we the Russian or Pole meekly serve! . . . Let love be for the Ukraine alone."

As trying as publishing in the USSR of the history of non-Russian peoples is, Kravets' deserves credit for this book. Perhaps the curiosity he will raise among his Ukrainian readers about certain controversies in that republic's past will be merit enough.

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AFRICA

NORMAN R. BENNETT. *Africa and Europe from Roman Times to the Present*. New York: Africana Publishing Company. 1975. Pp. 246. Cloth \$12.50, paper \$5.95.

To compile a coherent history of Africa from Roman times to the present is a massive task; to complete such a work in one usable survey would seem to be impossible. Yet Norman R. Bennett has written such a work and has made an excellent contribution to African history. According to Bennett, his purpose was to use the most recent scholarship to present a short account of the relationship between Africa and Europe over several thousand years of history. He has not only fulfilled this purpose, but has gone beyond it to shed light on recent African developments.

In a work of this sweep, the reviewer naturally looks for basic errors in fact. That there are surprisingly few reflects the depth of Bennett's scholarship and research. He has taken the threads of both North and sub-Saharan African history and intertwined them into a reasonable fabric. At times it seems that the portion pertaining to the area south of the Sahara is stronger than that to the Maghrib. This, of course, seems to reflect Bennett's interests. The brief analysis of British and French colonial policy is an excellent summary of the two systems; the chapter, "African Responses," which outlines the often neglected African reaction to the European colonizer, is also valuable.

Free from nagging errors, well written, the product of the best of recent research, *Africa and Europe*

should serve the needs of those wanting a succinct overview of African history.

JAMES J. COOKE
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VICTOR N. LOW. *Three Nigerian Emirates: A Study in Oral History*. Evanston: Northwestern University Press. 1972. Pp. xv, 296. \$15.00.

The history and development of state systems in the Western Sudan has generally focused on the "greats." For centuries the Hausa States—Bornu, Wadai, and Dafur—were the polities best known to outsiders. Monographs on Jukun and Nupe have placed these lesser polities into the literature. Nevertheless, this leaves a plethora of smaller kingdoms out of the comparative roster. Low has chosen to do this job for the so-called nineteenth-century border states of Gombe, Katagum, and Hadejia, which lay between Bornu on the east and the rising expansionist power of the Sokoto Caliphate on the west. These states were induced into existence by the Fulani *jihad*, and throughout the nineteenth century they referred to Sokoto as the ultimate authority: "The Emirs and their successors continued periodically to furnish Sokoto with martial aid and a share of state revenue and attended on the Sultan whenever summoned" (p. 12).

After summarizing the political organization of these states, the author discusses his methods and launches a historical account of the border emirates divided into early and later nineteenth century. The author also attempts in a major way to reconstruct the population sizes, ethnic distributions, fief holdings, and boundaries as these were about 1900 when the British took over the area.

There is no other book that covers this subject or puts together the archival materials with local oral traditions. In this sense Victor N. Low's work is certainly a contribution. Like it or dislike it, scholars will be forced to consult it. Low's understanding, however, of the kind of data necessary to document political history in the Sudan seems extremely limited. Why, or even how, Gombe, Katagum, and Hadejia have differentiated as political systems from the accounts and models provided by Smith, Nadel, and others escapes him as a problem of central importance. Instead, he seems to pay great attention to estimates—mostly by early British colonial officials—of population and fief boundaries. The reliability and validity of such demographic data create a problem well-beyond the capabilities of the usual African historian. So much political advantage rests, and always has, with the distortion of data, and the effective use of such data requires special training, which Low clearly does not have.

Where the historian can contribute in describing and explaining particular events, the book also falls short. For example, Rabeh is said to have given up "his apparent intention" of invading the eastern Fulani emirates (p. 174). A close reading of Low's sources does not sustain the argument that Rabeh wished to continue his conquests eastward, nor that he gave up such ambitions. Low neither documents nor explains such assertions. And Buba Yero (Gombe) is said to have withdrawn from operations east of the Hawal River and "then or afterwards abandoned any claim on Biu . . ." (p. 94). This information is utterly crucial to someone working on the development of the Biu political system, but he would look in vain for some explanation of this event. Local traditions in Biu suggest that Buba Yero's incursions led to coordination and organization of intersettlement. This same Gombe leader is said to have conquered the Kanakuru area of Shellen and had it "supervised in some vague manner" (p. 152). Low's writing is frustrating to scholars working on just such problems.

Nevertheless, the book is a start. Scholars now need more detailed political histories of each of these areas. Low has stretched the canvas; others must now paint the picture.

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HELMUTH HEISLER. *Urbanisation and the Government of Migration: The Inter-relation of Urban and Rural Life in Zambia*. Foreword by R. MANSELL PROTHERO. With an appendix on some migration histories by M. G. MARWICK. New York: St. Martin's Press. 1974. Pp. xi, 166. \$15.95.

In 1931 fewer than 25,000 Africans in Zambia (formerly Northern Rhodesia) lived in urban areas; by 1969 the number had risen to some 1,100,000. Helmuth Heisler points out that until after World War II this urbanization occurred despite the opposition of the Provincial Administration. The dominant viewpoint, associated with Lord Lugard's philosophy of indirect rule, emphasized the importance of maintaining the traditional cultural patterns of the village and agricultural ways of life. Urbanization, colonial officials feared, would undermine traditional value systems and structures of authority, disrupt rural life and agricultural enterprise, create racial and labor unrest, and deprive Africans of their social and economic security. Moreover, the cost of housing, policing, feeding, educating, and providing for the health and welfare of a large urban population was more than most European administrators, politicians, and capitalists were willing to bear.

Gradually, the needs of employers in the grow-

ing mining industry and commercial agriculture prevailed over those of administrators. Initially employers used a "Recruited Labor System," monetary inducements, and coercive taxation. But this gave way in the 1930s to a "Casual Labor System," under which Africans went to and from the urban-industrial areas largely on their own initiative in response to social and economic motivations. Yet until the end of World War II Europeans were only willing to provide the Africans primitive "labor camps," consisting largely of dormitories for temporary workers. With growing labor unrest and the need for more highly motivated, skilled, and experienced manpower, "Towns for Africans" were built, making possible a stabilized family life for large numbers of Africans. Nevertheless, it was not until political independence in 1963 that Africans could become committed to "urbanism" as a way of life, with less need to look to their rural homelands for social security.

While Heisler aptly describes and analyzes these developments, certain inadequacies are apparent. Because of the emphasis on patterns of migration, the absence of maps is disconcerting. Occasionally, particularly in the chapter "Women for Urbanization," Heisler goes beyond his sources to speculate about the relationship of divorce, companionate marriage, distance, and nationalism to migration from the rural areas. Here and elsewhere, he might have made greater effort, using interviews, to get an African point of view, which could have increased his confidence about the impact of changing incomes on migration incentives (p. 113). As it is, African motivations for urbanization remain unclear. More serious, the author fails to explore certain policy questions he raises near the end of the book: Is there overurbanization in Zambia? Can and should rural life be made more attractive? If not, what are the consequences of a growing divergence of peasant and urban systems? Without answers to these questions, readers are left with the hope that Heisler deals with them in another book.

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ARYE ODED. *Islam in Uganda: Islamization through a Centralized State in Pre-colonial Africa*. (The Shiloah Center for Middle Eastern and African Studies, Tel Aviv University. Studies in Islamic Culture and History. A Halsted Press Book.) New York: John Wiley and Sons. 1974. Pp. x, 381. \$23.50.

In *Islam in Uganda* Ayre Oded traces the introduction of Islam into the kingdom of Buganda. The work is divided into three parts. Part one deals with the coming of Zanzibar Arabs to Bu-

ganda in search of markets for ivory and slaves during the reign of Kabaka Suna (c. 1832-56), who welcomed them. Oded covers in detail the factors that tended to promote and hinder the influence of Islam, which reached its height about 1875 in Kabaka Mwanga's time. Contrary to current opinion, Islam emerges as having made positive contribution to Buganda by introducing such skills as writing and reading and the use of different styles of clothes and other goods.

Part two deals with the first arrival of Europeans, in particular the explorer Henry Morton Stanley who initiated the effort to undermine Islam and promote Christianity. The ambitions of Khedive Ismail of Egypt (1836-79) to annex Buganda through the work of Colonel Charles Gordon and other agents are well treated, as are the reasons for their failure.

Part three covers the years (1877-84) of intense religious rivalry and intrigue between the Moslems and Christian missionaries, resulting essentially from the latter's determination to oust Islam and end the slave trade. Oded portrays vividly the dilemma and confusion Kabaka Mutesa faced when confronted with conflicting religions (Islam, Protestantism, Catholicism). The leaders of these religious groups are carefully analyzed as are intra-Christian conflicts. Mutesa emerges as a shrewd, highly intelligent ruler, capable of using religion as a tool to maintain his power.

The author then summarizes the progress of Islam after Mutesa's death, through the religious wars of the late 1880s, and brings the work to date by treating Islam in the post-independence era, characterized by division, and the current efforts of President Idi Amin Dada to unify the Moslems.

There are seven appendices, a valuable bibliography, and an index. Oded's book is a very useful, well-researched contribution to an understanding of a neglected aspect of Uganda's formative years that still influences the present.

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J.-M. FILLIOT. *La traite des esclaves vers les Mascareignes au XVIII^e siècle*. (Mémoires ORSTOM, number 72.) Paris: ORSTOM. 1974. Pp. 273. 120 fr.

PHARES M. MUTIBWA. *The Malagasy and the Europeans: Madagascar's Foreign Relations, 1861-1895*. (Ibadan History Series.) Atlantic Highlands, N.J.: Humanities Press. 1975. Pp. xvi, 411. \$17.25.

Off the southeast African coast lies the great island of Madagascar, and to the east of it the small islands of the Mascareignes, Mauritius, and Réunion. Indonesian migrants settled Madagascar during the first millennium A.D., and when Euro-

peans entered the western Indian Ocean around the beginning of the sixteenth century they found vigorous, independent, indigenous polities. The isolated Mascareignes were yet unoccupied when the Portuguese reached them during the same century. In the seventeenth century the Dutch used Mauritius as a stopping place on their route to the East, maintaining a limited presence until the beginning of the eighteenth century. Also in the seventeenth century, the French, who then were active without great result in Madagascar, began their settlement of Réunion. In 1715 they occupied Mauritius.

Slaves from Madagascar, East Africa, and elsewhere were brought to Réunion before the close of the seventeenth century, and during the succeeding century a busy slave trade developed, especially after the arrival in 1735 of the dynamic French governor, Mahé de la Bourdonnais. The magnitude of the slave trade to the Mascareignes long has been a question of interest to scholars concerned with both Madagascar and the East African coast. Using archives in Britain, France, Réunion, Mauritius, Madagascar, and South Africa, as well as the abundant published literature relating to contacts between Europeans, Africans, and the Malagasy, Filliot attempts to answer it. The fruits of his labors make available for the restricted area of his interest a result similar to that of Philip Curtin in *The Atlantic Slave Trade* (Madison, 1969): we now possess a base number to serve as a starting point for all future studies of the slave trade in this region. During the period from about 1670 to 1810 160,000 slaves reached the Mascareignes. Filliot breaks down the imports periodically, as follows: c. 1670-1717, 1000; 1715-26, 10,000; 1727-51, 25,000; 1752-66, 14,000; 1767-1810, 110,000. He calculates that 45 percent of the total Mascareignes slave imports came from Madagascar, 40 percent from East Africa, 13 percent from India, and 2 percent from West Africa. Admittedly, the figures must be treated with caution, but Filliot has carefully analyzed his sources and explained his methodology, so that his readers can reasonably assess his judgments. His figures provide a valuable addition to other recent studies of the slave trade of the western Indian Ocean by C. S. Nicholls, Edward Alpers, Fred Cooper, and Abdul Sheriff. Filliot's scholarship is impressive, and his statistics will have to be accepted as the best available estimate for the foreseeable future. In the remainder of his monograph Filliot provides data on the details of the process of the trade in slaves, including region-by-region analyses of the areas from which slaves were drawn. The bulk of the data, however, relates to Madagascar.

In *The Malagasy and the Europeans* Phares Mutibwa studies the people who dominated much of

Madagascar during the nineteenth century, the Merina of the central plateau. Mutibwa is primarily concerned with the relations between the Merina and the European world from the 1860s to the French conquest in 1895. The Merina and their great leader, Rainilaiarivony, had to deal extensively with Britain and France. For these relations Mutibwa presents a useful account drawn from the major British and French archival collections and from the Malagasy National Archives. The volume adds to our knowledge of this period of Malagasy history when the Merina, by adapting European influences and techniques to their needs, strove to remain free of European rule. But with his focus on the Merina, Mutibwa often over-identifies with their diplomacy; for example, he accepts as valid the dubious claims of the Merina to suzerainty over the Sakalava peoples of Western Madagascar. Nonetheless, since English-language studies of Madagascar are unfortunately rare, Mutibwa's volume fills a niche in the historical bibliography of the great African island.

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IAN LINDEN. With JANE LINDEN. *Catholics, Peasants, and Chewa Resistance in Nyasaland, 1889-1939*. Berkeley and Los Angeles: University of California Press. 1974. Pp. xii, 223. \$16.00.

Roman Catholic missions have been too little studied in Africa, most researchers being more comfortable with the often more accessible Protestant experience. Of the few examinations of the Catholic impact, this one succeeds because of Linden's grasp of the relevant theology and, equally, the social context in which the missionaries labored. For Linden this context included the climate of the sending countries, the constraints and opportunities of colonial rule, and the cultural and other demands of the Africans among whom the priests proselytized.

Linden's best, later chapters discuss the educational effort of the priests and nuns and their attempts, over decades, to create an indigenous Church through rigorous training in isolated seminaries. His earlier chapters narrate the pre-colonial attempt to establish Catholicism in Nyasaland (now Malawi), the postpartition starting of stations by the White Fathers as a by-product of their work in Northern Rhodesia (now Zambia), and the subsequent movement into southern Nyasaland of the Montfort Fathers: Linden focuses more upon the White than the Montfort Fathers, but there is less than expected about the Catholic role during the Chilembwe rising. In fact, it is not clear what the "resistance" of the title is meant to convey. There is a glimpse of the

battles over territory between the Catholics and the Dutch Reformed and Presbyterian mission groups, and an explanation of why—after Chilembwe—so many administrators encouraged, even assisted, the Catholic advance.

As a microstudy, this book has substantial merit. But it lacks any comparative dimension. The Catholic experience in Malawi is never contrasted to that of Zambia, East Africa, or Zaïre. Nor is Malawi Catholicism compared more than superficially with Malawi Protestantism.

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BRIAN ROBERTS. *The Zulu Kings: A Major Reassessment of Zulu History*. New York: Charles Scribner's Sons. 1975. Pp. xii, 388. \$10.95.

"The nations, Shaka, have condemned you/Yet still today they speak of you/Still today their books discuss you/But we defy them to explain you." Thus wrote B. W. Vilikazi, a Zulu poet, in the concluding lines of a literary masterpiece. Many authors have tried to provide an explanation for the Zulu phenomenon. They include writers as varied in their background and approach as Peter Becker, C. T. Binns, Max Gluckman, Donald R. Morris, J. D. Omer-Cooper, Leonard Thompson, and E. A. Ritter. Brian Roberts, who has already contributed to a variety of South African themes, now provides a reassessment of the empire that Shaka won and Cetshwayo lost. His book has earned praise from sources as different as the *Library Journal* and Chief Buthelezi, Chief Minister of Kwa Zulu. These good opinions are deserved. The author has consulted a wide range of archival sources in Britain and South Africa, as well as a considerable body of secondary literature. He uses his sources with a sense of discrimination. He writes clearly, and he has an exciting story to tell.

To Roberts, the story of Zululand is a Sophoclean tragedy. Shaka—by disrupting the age-old pattern of tribal settlements, destroying his enemies with methodical thoroughness, and laying waste the surrounding country—founded a kingdom unique in the annals of South Africa. His claim to supremacy, as Roberts sees it, owed as much to territorial aggrandizement as to military supremacy. "That land could represent power (rather than acting as grazing for cattle, the traditional symbol for wealth) was a revolutionary concept in tribal politics. It accounted not only for the rise of the Zulu nation, but for its eventual downfall: a downfall which was heralded by the coming of the first white men."

Roberts would have deserved even better of his readers had he dealt at greater length with the purely military side of Zulu history—with those

questions of tactics, strategy, and supply that played such a vital part in a kingdom founded upon war. Also, he is somewhat overconfident in his use of statistics. No historian should make much of dubious estimates of the kind that 2,000,000 people perished as a result of Shaka's wars, or that 3,000,000 died by reason of Leopold's atrocities in the Congo. Nevertheless, this is a good book. Its appearance indicates how far the popular historiography of Southern Africa has advanced since the appearance of a work like Hugh Marshall Hole, *The Passing of the Black Kings*, 40 years ago.

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ASIA AND THE EAST

FRANK SWETZ. *Mathematics Education in China: Its Growth and Development*. Cambridge, Mass.: MIT Press. 1974. Pp. 364. \$15.00.

The stated purpose of the author is "to assess the trends in mathematical education at the primary- and middle-school levels in China." Chapter 1 gives interesting examples of native opposition to foreign influence from India in the third century, the Jesuits after 1601, and Europe in the nineteenth century, among others. Chapter 2 describes the establishment, in the late nineteenth century, of a modern school system based first on European and Japanese models and then on American. Chapter 3 begins with a description of Mao's educational theories, and the difficulties—lack of teachers, buildings, books, and chalk—of setting up schools in the remote province of Yenan. In 1937 there were only 120 schools in these border regions, but by 1949 the number had increased to 1,081.

After their assumption of power in 1949, the Communists were greatly influenced by the USSR, particularly in their theories of education. Chinese students sent to Russia between 1949 and 1958 numbered 61,000, about twice as many as came to America in the forty years after 1909. But many difficulties arose from this alien influence, which was felt by Chinese political leaders to promote an "elitism" inconsistent with true Communist theory. The school system became deeply infiltrated by party cadres, who made demoralizing innovations in 1958 and again in 1960 in connection with the Great Leap Forward.

In the next five years, the fluctuations between "redness" and "expertness" finally produced those uprisings and disorders among students, particularly in the University of Peking, that constitute the beginning of the Cultural Revolution. Finally, in March 1968 army units restored some

semblance of discipline in the schools, which were now taught by teams composed of factory and agricultural workers, political cadres, and revolutionary teachers and students. Instruction had deteriorated in quality, but by 1970 the difficulties arising from Chinese literary tradition and from long-continued foreign influence had been eliminated.

The innovations in Chapter 5 can be characterized by the typically Chinese phrase "walking on two legs," or taking advantage of two kinds of learning—study of books and work in fields and factories, supplemented by psychological experimentation, social events with mathematical games and contests, inexpensive booklets for self-instruction and entertainment, handbooks for farmers, and special journals for secondary-school teachers and students.

The final chapter discusses the extent to which such flexibility could be imitated with profit by Third World countries. The closing sentence optimistically predicts that, as a result of growing mathematical sophistication among workers and farmers, "the industrial potential of the People's Republic of China will become most formidable."

The extensive documentation, in a field in which documentation is notoriously difficult, does credit to the author's industry and resourcefulness.

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JOSEPH R. LEVENSON. *Revolution and Cosmopolitanism: The Western Stage and the Chinese Stages*. With a foreword by FREDERIC E. WAKEMAN, JR. Berkeley and Los Angeles: University of California Press. 1971. Pp. xxxi, 64. \$5.00.

When Joseph Levenson died in 1969, he had completed his *Confucian China and Its Modern Fate* and was deep into a new trilogy on "provincialism and cosmopolitanism." Two volumes were partly drafted, and this brilliant posthumous work, three undelivered lectures, was to be the third. Levenson illuminates the dilemma of post-May Fourth intellectuals. Alienated from the "silk-fan attitudes" of Confucian "high culture," they fostered a new cosmopolitanism "to escape being exhibits themselves . . . for foreign delectation." By analyzing prefaces to Chinese translations of Western plays, Levenson reveals the "rootlessness" of their condition. "'Bourgeois' cosmopolitans" of the 1930s could savor the "advanced art" of Pirandello or Schnitzler, but their pleasure was ambiguous. For emergent Chinese Communism, the internationalism of Schnitzler's translator had no social purpose. "No red flag was likely to rise from this well-manicured hand."

Why, then, in the 1950s did Chinese Communists continue to condone cosmopolitan translations? Because they had developed a *class* cosmopolitanism of their own. The *jen-min* ("people" as class), which in Chinese Communist analysis virtually constituted the nation, were also the *jen-min* of all nations. In translations of non-Communist plays, a foreign "high culture" was made "people's"—an international term appropriate to the Chinese people. Shakespeare's Henry V led a new nation, but the translator's preface makes the paean abstract, extolling "not the people of England but an age of peoples,"—"and gentlemen of China will think themselves accursed upon St. Crispin's Day." Only after 1957 was "empathy" out. Class struggle went forward, while "the bourgeoisie was still abroad at home." In the "provincial" spirit of the Cultural Revolution, "the sophisticates, detached from the *jen-min* by their culture, were detached from the [nation] by their world-wide affinities with fellow cosmopolitans."

Levenson's perception of Chinese intellectuals was sensitized by his own dilemma as a believing Jew. In an elegant foreword, Frederic Wakeman shows how Levenson strove to define Judaic particularism (the history, the people) against the "spurious universalism" of Christian tradition. His outer scholarship expressed his inner faith: in his life as in his work, "a tension which prohibited the merging of opposites was dynamically creative."

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RICHARD J. SMETHURST. *A Social Basis for Prewar Japanese Militarism: The Army and the Rural Community*. (Published under the auspices of the Center for Japanese and Korean Studies, University of California, Berkeley.) Berkeley and Los Angeles: University of California Press. 1974. Pp. xxi, 202. \$14.00.

This book presents a narrowly focused but provocative interpretation of the social dynamics of rural Japan between 1910 and 1945. Richard J. Smethurst argues that in 1910 the Japanese War Ministry created a comprehensive network of Imperial Military Reserve Associations (*Teikoku zaigo gunjinkai*) in order to promote national unity and to foster grass roots support for the army. The War Ministry subsequently encouraged the formation of the Greater Japan Youth Association, youth training centers, and the Greater Japan National Defense Women's Association, all of which were controlled at the local level by the Reserve Associations.

Drawing upon local histories and detailed case

studies of several Japanese villages, Smethurst demonstrates how the Reserve Associations, in particular, merged into rural communities. Unlike the national school or police systems, the Reservists had branches in every tiny hamlet, the basic unit of rural society. Reservists served in important capacities within these communities, as police or firefighters, for example. Usually all eligible males joined the local branch, and its leadership often reflected the local economic power structure. Through the Reserve Associations, the army could indoctrinate the Japanese peasantry with patriotic and military values. The peasants became "national villagers" who supported the army, as loyalty to the hamlet became synonymous with loyalty to the nation.

Smethurst provides important evidence that there was no sudden eruption of militarism in Japan, since he shows how the army expanded its influence gradually after 1910. Yet this book offers a limited picture of prewar rural Japan, as there is no analysis of serious rural economic problems or of the relationship between the Reserve Associations and local leaders who were agents of the political parties. Also, one must question whether the Reservists' encouragement of patriotism and obedience alone led to xenophobia in Japan.

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P. HARDY. *The Muslims of British India*. (Cambridge South Asian Studies, number 13.) New York: Cambridge University Press. 1972. Pp. ix, 306. Cloth \$15.50, paper \$5.95.

FRANCIS ROBINSON. *Separatism among Indian Muslims: The Politics of the United Provinces' Muslims, 1860-1923*. (Cambridge South Asian Studies, number 16.) New York: Cambridge University Press. 1975. Pp. xiii, 468. \$22.50.

ALBERT CHRISTIAAN NIEMEIJER. *The Khilafat Movement in India, 1919-1924*. The Hague: N.V. de Nederlandsche Boek- en Steendrukkerij V/H H. L. Smits. n.d. Pp. vii, 263.

ALEXANDER F. BAILLIE. *Kurrachee: Past, Present and Future*. (Oxford in Asia Historical Reprints from Pakistan.) New York: Oxford University Press. 1975. Pp. xx, 269. \$20.75.

The books under review deal with the Muslims of India during British rule. Hardy's study is a general, readable discussion of the varying impact of British policies upon India's Muslims. Robinson's monograph is a revision of his doctoral dissertation, and as such, is much more restricted in scope, concentrating on the Muslims in one province during the era of emerging Muslim self-consciousness as a separate political group. Niemeijer's work is

also a dissertation dealing with Indian Muslims during a particularly significant political movement. The reprint of Baillie's *Kurrachee* [sic] will be commented upon later.

There are a number of points of comparison among the first three works. Each deals with British policies toward Indian Muslims and the Muslims' responses to those policies. Each looks at the evolution of the Indo-Muslim community, especially in the political context. Hardy and Robinson, in discussing British policies toward Muslims, show that there were differences according to regime and region. In this welter of contradictory policies and priorities, the myth of a carefully worked out policy of "divide and rule" is laid to rest. Hardy characterizes British policy as one of "balance and rule," an attempt to balance a number of interests, regional and professional as well as religious. Thus, the evolution of Muslim separatist politics was less the result of Muslim response to the categories perceived by their British rulers, than the unpredictable combination of British attempts to balance interests with the Muslims' evolving sense of identity. This identity was based on the legacy of political supremacy and earlier religious reform movements, in addition to the political and economic pressures of British rule.

Robinson, however, gives to successive British governmental reforms the predominant role in the emergence of Indian politics and, consequently, of Muslim separatism. Political rivalries emerged because of competition for new patronage, cast in the molds of British categories. In spite of occasional lip service to "firm religious beliefs," Robinson views religion and ideology as irrelevant in the creation of Muslim separatism; the competition for bureaucratic favor and electoral advantage was all-important. His tone is indicative of a point of view straight out of the British records. He sees the use of religious ideas and symbolism in the political context as rank opportunism, designed to hoodwink the credulous masses and to increase the circulation of the radical young Muslim press. This tone is not conducive to understanding the Muslim politicians in their own terms. Granting the importance of British policies in the political development of India, one nevertheless must take into account a variety of cultural intangibles and accept their legitimacy in the political process. Hardy seems to be attuned to this need; Robinson tends to be insensitive to all but the "hard" political and economic factors.

If the British Raj was not monolithic in its dealings with Indian Muslims, neither was the Muslim community monolithic in its response. Here again, Hardy and Robinson make useful contributions in cutting the myth of Muslim unity into its com-

ponent parts. Hardy clearly shows the different regional and class distinctions among Muslims and their varying responses to the challenges of Western education and new employment patterns. Robinson analyzes the factional divisions within Muslim leadership of the United Provinces, dubbing the factions the "Old Party" (progovernment) and the "Young Party" (pronationalist). Here, too, the differences between the two parties were based as much on differences of style and personality, and on religious and cultural variables, as on attitudes toward working with government.

Niemeijer's work, both in dealing with British policy and with the Muslim response to it, is considerably more monolithic in approach. The author occasionally shows that British bureaucrats disagreed over policy, but he seems to lose sight of this fact in his generally simplistic analysis. He spends a great deal of time trying to define "nationalism" and "communalism" and to use these definitions in his narrative. This approach leads him to ignore the conflicts among Muslim politicians, a result which is particularly disastrous for a study of the Khilafat movement. His assumptions lead him to treat the Indian Muslims as an undifferentiated group, whereas the Khilafat movement sought to create a political consensus among a number of groups and factions. Niemeijer certainly does not help increase our understanding of religio-political movements, and he betrays an insensitivity to the importance of ideological factors.

Perhaps the reason for inadequate understanding of the complexities of Muslim separatism lies in the sources each author uses. Hardy is best able to understand his subjects because of his background in Islamic scholarship and his use of Persian and Urdu sources in addition to British records. He thus provides scholars with a useful, insightful synthesis of much recent research. Robinson has done a painstaking job of research in British records, nationalist newspapers, and private papers. He has also used some Urdu sources in translation. It is unfortunate that this potentially valuable piece of research is marred by a bias which severely limits its usefulness. Niemeijer's sources are limited to British records and secondary works in Western languages, and his point of view is myopic. His work is a rehash of much that has gone before, with no new insights.

Baillie's *Kurrachee*, originally published in 1890, is a chatty, rambling, and thoroughly delightful collection of miscellany about the sleepy port he felt certain was destined for greater things. The work contains religious and ethnographic observations, as well as much that will be of interest to urban and economic historians, especially those inter-

ested in the development of ports and railways under the Raj.

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ARUN COOMER BOSE. *Indian Revolutionaries Abroad, 1905-1922: In the Background of International Developments*. Patna: Bharati Bhawan. 1971. Pp. xv, 268. Rs. 30.

EMILY C. BROWN. *Har Dayal: Hindu Revolutionary and Rationalist*. Tucson: University of Arizona Press. 1975. Pp. xiv, 321. \$7.95.

Indian revolutionaries played important roles in international politics during the last four decades of British rule in South Asia. Although their barrage of inciting literature and occasional violent acts did not bring the imperial government to its knees, the revolutionary menace did force compromise by the British and served as a reminder of alternative avenues of political expression if concessions were not made to Indian nationalist demands. In addition, militants in Europe and North America affected diplomatic relations, creating tension between England and her nominal allies. In Canada and America the revolutionaries also became involved in local politics and built alliances with respectable individuals and parties.

Both these studies explore the many dimensions of Indian revolutionaries abroad. Arun C. Bose connects activities in Asia, Germany, and North America between 1907 and 1919. Balancing archival material with printed sources, he traces how bands of Indian radicals in different locations formed links during the First World War and ultimately proved ineffective in sabotaging British administration in India. Although Bose's writing is dry, and his conclusions at times are influenced more by how revolutionaries and their sympathizers or opponents tended to view the movement rather than by facts, the assessment probably will remain the best of the half-dozen books surveying the subject.

Emily Brown, however, goes far beyond writing just another synthesis or recounting deeds and misdeeds among revolutionaries. Focussing on the life of one individual, Har Dayal, she sheds new light on what Indians were doing outside the subcontinent while providing a lively and highly readable biography of an enigmatic Indian intellectual. The product sets a new standard for careful tracing of stray documents and arranging them in meaningful fashion.

In 1905 Har Dayal came to England as a bright young scholar with a grant and much academic

promise. Within two years he changed first into a bitter critic of imperial rule, and then into a hardened propagandist and plotter. But as Brown clearly demonstrates, Har Dayal's antipathy toward colonial rule was not solely a matter of politics; it rested ultimately on a concern with the nature of Indian civilization and tradition. At each step of his career, Har Dayal mixed scholarly research and philosophical speculation with attacks against the immediate cause of national degradation, foreign rule. His tendency to move from project to project, friend to friend, and country to country, reflected psychological uncertainty and unrest growing from a preoccupation with evolving a satisfactory philosophical statement while concomitantly playing a meaningful role in everyday affairs. Har Dayal's public life thus can be broken into two major segments: 1907-19, a period of revolutionary zeal and activism; and 1919-39, a time of enforced exile when he attempted to deal with India's many problems on an intellectual plane. Thwarted from returning home by British fear, he wrote books on philosophy and Hinduism, worked toward a synthesis of Western and Indian ideas, and gradually became more conservative in politics.

Har Dayal was not a typical Indian revolutionary, but by carefully studying his life and the milieu in which he lived, Brown has broadened our understanding of organizations and individuals involved in the futile effort to overthrow the British. She and the University of Arizona Press are to be congratulated for producing a major contribution to modern Indian intellectual and political history.

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R. J. MOORE. *The Crisis of Indian Unity, 1917-1940*. New York: Oxford University Press. 1974. Pp. xii, 334. \$9.00.

In a remarkable speech in the House of Commons in 1858, John Bright argued that the united India created by the British served only their own greed and vanity, with despotism a necessity to preserve a political structure that denied the complexity of the subcontinent's historical experience. For the sake of the people of India, he urged that the area be divided into at least five separate states. R. J. Moore does not mention Bright's prescient argument, but his book is, in effect, a comment on the ironies and anomalies that became apparent as representative government and independence became political realities. When the process of devolution of power got under way with the Montagu-Chelmsford reforms at the end of the First World

War, the form of nationalism articulated by the Indian National Congress had already made it an article of faith that the imperial territorial legacy would be handed down intact to a liberal, representative state. The British, despite the conventional arguments of national historians, shared the passion for maintaining political unity, if not for democracy.

Moore traces in considerable detail the development of the strains in Indian political life from the announcement in 1917 of the British intention to move toward responsible government to the point in 1940 when the strains became deep fissures. The central chapter deals with the crucial period in 1930 when, under Gandhi's leadership, the Congress evolved new strategies to hold together a badly divided Congress. According to Moore, Gandhi concluded that creating these strategies was more vital to Indian unity and freedom than a continual and frustrating search for a constitutional formula to satisfy Muslims, Hindu liberals, British Conservatives, and Indian Princes. His decision was to mount a civil disobedience campaign to expose British rule in India as a despotism manipulating the Indian economy for its own selfish ends. He formulated eleven demands that made no reference at all to constitutional issues, but only to subtle linkages of morality and economics, such as prohibition, the salt tax, customs duties, and the exchange rate. He was thus able to secure the backing of the mercantile community for "an experiment in anarchy," with the Salt March its great symbol. Thus, unity within the Congress was purchased by ignoring the growing estrangement with the Muslims.

The India Act of 1935 was intended to take India toward semiautonomous provinces, but the Congress forestalled the Act's intentions by controlling its provincial governments through a unitary party structure. According to Moore, the program of gradual devolution, the strategy the British hoped would maintain unity, led "inexorably towards division," since the Muslim leadership was excluded from power.

AINSLIE T. EMBREE
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NICHOLAS MANSERGH and PENDERL MOON, editors. *The Transfer of Power, 1942-7. Volume 5. The Simla Conference: Background and Proceedings, 1 September 1944-28 July 1945. (Constitutional Relations between Britain and India).* London: H.M. Stationary Office; distrib. by Pendragon House, Palo Alto, Calif. 1974. Pp. xcii, 1346. £17.50.

Documents on the Simla Conference, found in the latest volume of this series (Vol. 5), show a marked

rise of political tensions. As an observer of a classic drama, one can sense the rising pitch of emotions and the tightening strain of nerves as events moved to their ultimate, tragic climax—Partition. Once again, this volume shows that each party displayed semblances of reasonableness and pacificity. A slow thaw came upon political relations in India, due chiefly, as is often and explicitly shown, to the prospect and reality of victory in the European war. With their homeland no longer threatened, British rulers could afford magnanimity. They did not withdraw the Cripps pledges of further constitutional advances, despite the Quit India Rebellion and its aftermath. In order to redeem the pledges, however, the British saw further Anglo-Indian dialogue as a necessary preliminary. That such a resumption of talks should take place earlier rather than later was of great concern to the viceroy. He felt that while at the moment the British *raj* was "sitting pretty," its long-term prospects were far from comfortable. Unless imaginative steps were taken to assure political advances by agreement among leaders of the major parties in India, the probability of serious unrest and violence would grow. Leaders in Britain were not so quickly persuaded; members of the War Cabinet and especially Churchill questioned the wisdom of such a move and the forms it should take. The debates they engendered, taken from Indian and British records, provide unusual glimpses into the workings of the "official mind" in Britain as it moved toward its penultimate decision on "the Indian problem."

At the conclusion of debate the British Cabinet agreed to call a conference of Indian political leaders, to be held at Simla on June 25, 1945. Documents in the volume relate the story of the conference in graphic detail. The difficulties of the viceroy and his advisers become glaringly apparent. The imprisoned Congress Working Committee, whose participation was so important to the conference's success, had to be released. Gandhi's role with the conference had to be defined—first, because he claimed that he was not a member of the Congress; and second, because his power over the Congress was so obvious. Deliberations followed an erratic course. Ultimately, they broke down and the conference failed. In the view of the viceroy, responsibility for the breakdown lay in "the real distrust of the Muslims, other than National Muslims, for the Congress and the Hindus."

Of course, the crucial events of the conference cannot be seen apart from other important developments—relations with other countries, plans for industrial development in India, India's representation at the San Francisco Conference, supplies of food, and questions of prospective commercial re-

lations between an independent India and Britain. Taken as a whole, these documents provide a fascinating insight into the workings of one of the great political systems of modern times.

ROBERT ERIC FRYKENBERG
University of Wisconsin,
Madison

UNITED STATES

WILCOMB E. WASHBURN. *The Indian in America*. (The New American Nation Series.) New York: Harper and Row. 1975. Pp. xix, 296. \$10.00.

This volume in the New American Nation Series must have been one of the most difficult assignments in the series. The militancy in recent years of Indians and their supporters has made it increasingly difficult to deal objectively with any aspect of the history of Indian-white relations. Wilcomb E. Washburn has retained his poise remarkably well, and there is a studied effort to see both sides of any issue. He did not shrink, however, from taking a position on the American Indian Movement, which will distress its adherents.

Overall, this is easily the best survey of Indian-white relations. Washburn was an early advocate of ethnohistory (the effort to bridge the disciplines of history and anthropology), and it is reflected in this volume. The author draws upon the most recent scholarship in history and is equally at ease in the literature of anthropology. The result is a depth and breadth to the discussions which make it invaluable as a reference work. Moreover, his clear, direct style makes for easy reading.

No survey of such a complex and controversial topic can be completely satisfactory to any reader. Some may conclude that the balance of the book gives the colonial and early national periods undue space and compare the treatment of the Pequot and King Phillip wars with the failure even to mention Custer's battle on the Little Big Horn. This reviewer differed with several specific judgments made by Washburn, for example the defense of treaties as "more often than not, the bargain struck by two autonomous forces who weighed carefully the advantages accruing to each from the agreement." Nor could this reviewer agree with the significance assigned the period 1845-55. If the western Indians were "humbled and reduced," it certainly was in the twenty years following 1855, not in the years 1845-55.

These were exceptions, however, to the generally excellent quality of the volume. As one closely involved in the preparation of the new multivolume edition of the *Handbook of North American In-*

dians, Washburn was the best-posted historian to undertake this task, and he has not disappointed us.

WILLIAM T. HAGAN
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Fredonia

LEWIS P. SIMPSON. *The Dispossessed Garden: Pastoral and History in Southern Literature*. (Mercer University Lamar Memorial Lectures, number 16.) Athens: University of Georgia Press. 1975. Pp. x, 109. \$6.00.

In *The Dispossessed Garden*, Lewis P. Simpson finds a unifying theme in the literature of the South from the seventeenth century to the present: the attempt by Southern writers to accommodate their unique history with the pastoral myth of the New World as a redemptive garden.

Arguing that Virginia was as much a covenant community as New England, Simpson asserts that Virginians from William Byrd II through Thomas Jefferson found an important place for black chattels in this society. Slaves were unobtrusive gardeners, a necessary part of a "world which the Southern imagination would unsuccessfully endeavor to represent in terms of a pastoral garden of the chattel, seeking to make a covenant between the literary mind and chattel slavery, to make slavery the condition of the independent mind" (p. 32-33).

Southern writers of the antebellum period attempted to reconcile a conflict between the institution of slavery and the pastoral image "by giving the chattel a pastoral relation to the land" (p. 48) and by producing "a literature of radical alienation from modernity" (p. 52). Writers of the early twentieth-century Southern Renaissance created a literature of alienation based upon a "covenant with memory and history" (p. 99). In the post-Faulkner South, "a covenant with the existential self" (p. 99) dispossesses the culture of alienation and offers new challenges to the survival of the literary imagination.

The author has chosen a wide selection of impressive illustrations from the literature and history of the South to support his thesis. Historians with an appreciation for the possibility of using myth and literary symbolism to understand history will find much in this small volume worthy of reflection and debate.

E. STANLY GODBOLD, JR.
Valdosta State College

PETER J. COLEMAN. *Debtors and Creditors in America: Insolvency, Imprisonment for Debt, and Bankruptcy, 1607-1900*. Madison: State Historical Society of Wisconsin. 1974. Pp. xiii, 303. \$17.50.

This is a frustrating book. It deals with subject matter of vital concern to legal and economic historians; the author spent seven years examining virtually every relevant printed source and a small but revealing sample of manuscript court records and debtors' petitions. Yet at the outset Coleman warns us that his study "does not pretend to be definitive. Rather it is a preliminary exploration" (p. ix). Unfortunately, the admonition is well taken. The book makes substantial contributions on a variety of topics. But the author's very plan for the volume foreclosed any possibility of systematically testing hypotheses regarding the impact of economic change on the public law of debtor-creditor relations.

The structure of the book is deceptively straightforward. Part One consists of three introductory chapters which survey debtor-creditor law in the colonial era, congressional experimentation with short-term bankruptcy laws during the nineteenth century, and judicial construction of the contract and bankruptcy clauses of the Constitution vis-à-vis state action in the field. After 1824, when a bitterly divided Marshall Court handed down *Ogden v. Saunders*, it was clear that state governments had ample authority to abolish debtors' prisons and, so long as Congress failed to supersede state laws, to enact prospective bankruptcy programs as well.

Parts Two through Four contain, in effect, fourteen individual monographs—one for each of the original thirteen states plus Vermont. Here Coleman digests each jurisdiction's applicable statutes, deftly supplements findings drawn from legal records with secondary materials, and painstakingly describes each state's idiosyncratic chronological and institutional development in the debtor-creditor field. These narrative chapters form the heart of the book and will be of considerable interest to state and local historians. But legal and economic historians are apt to be disappointed. We are left wondering, for example, why Massachusetts enacted a bankruptcy law as early as 1837, yet only three other states followed her lead before 1894. Rarely does Coleman adequately interpret what he reports; hence the comparative dimensions of his subject remain virtually unexploited.

Part Five contrasts sharply with the preceding sections both in substance and tone. In two chapters of "perspectives," one each on the debtors' prisons and bankruptcy, Coleman suddenly plunges into business history and offers some exciting observations on the interaction between debtor-creditor law and changes in the very style of lending. "In the long run," he concludes, "the debtors' prison disappeared because it was obsolete" (p. 268). Beginning in the mid-nineteenth century lenders developed more reliable in-

stitutional mechanisms for securing loans; creditors discovered that the power to imprison was superfluous in a marketplace served by monetized commercial paper, crop liens, pawnshops, garnishment, and conditional sales contracts. Bankruptcy relief was a more difficult matter. Policy-makers had to balance the demand for laws to encourage risk-bearing entrepreneurship with the nation's lack of fluid capital and the consequent need to maintain Americans' credit rating in national and international money markets. Moreover, advocates of bankruptcy laws had to overcome their fellow citizens' ingrained belief that discharging debtors was immoral. For many nineteenth-century Americans it was one thing to abolish debtors' prisons or to enact temporary stay laws in times of economic distress, and quite another to accord insolvents the right to discharge their obligations altogether. Finally, Coleman suggests that the slow acceptance of the discharge concept stemmed, in part, from the legislative process itself. Except in New England, the state legislatures simply lacked the independent energy to forge a coherent bankruptcy policy "which would benefit lenders, borrowers, and society, and which at the same time would be simple to administer, free from fraud, and relatively stable" (p. 277). Thus drift, default, and fleeting experiments—familiar themes to nineteenth-century legal historians—ensued until Congress finally enacted a national measure in 1898.

Coleman's concluding "perspectives" are important but out of place—they are not linked to data derived from the state-by-state narratives; nor do they account for state and regional variations in the chronological evolution of debtor-creditor law. In short, they are hypotheses which ought to have been tested in the body of the text. Nevertheless, Coleman's keen appreciation for detail and his penetrating observations make this a valuable book. A more complete "preliminary exploration" is difficult to imagine.

CHARLES W. MCCURDY
University of Virginia

CARL BRIDENBAUGH. *Fat Mutton and Liberty of Conscience: Society in Rhode Island, 1636-1690*. Providence: Brown University Press. 1974. Pp. xxiv, 157. \$8.00.

It is a pleasure to watch Carl Bridenbaugh at work as he coaxes a fresh and revealing history of early Rhode Island out of a wide range of manuscript and published sources. He reminds us that the Narragansett Bay country developed as a region of commercial farming—often on a scale that was large by the standards of the day—and that there was much more than religion to the colony's story.

Bridenbaugh's larger purpose is to show from the Rhode Island example that the "central theme of early American history is to be found in the nature and achievements (or failures) of an agricultural-commercial society" (p. xxii). In urging the guild back to the farm, he recognizes the paradox. How can a historian who has spent a lifetime studying various aspects of colonial urban life now repudiate or at least minimize the importance of his own career?

The answer, of course, is that he only appears to be discounting his own work. Bridenbaugh, however, misses another paradox; in a more junior historian, his strident call for a return to the true essentials and his indictment of what has been fashionable among colonial historians in recent years would be dismissed as youthful carping. "American historians are all city slickers," he proclaims; the "neophytes of the historical profession . . . have not the slightest insight into . . . how much different the seventeenth century was from the twentieth" (p. xix). Even were these allegations true, which they are not, the tone is unfortunate, since it diverts attention from the substantial contribution made in this little book.

With infinite patience and care, Bridenbaugh reconstructs the process of farm making; produces fascinating evidence on the deliberate, systematic, and large-scale investment in commercial farms; properly assesses the importance of climate, topography, and the many islands to the development of both pastoral and cropping activities; and links commercial agriculture to the colony's maritime trade. It is certainly to be hoped that other historians will pay him the supreme compliment of studying in similar detail the agriculture of the rest of New England, though one would also hope to see a higher level of synthesis and greater skepticism about lambing rates.

PETER J. COLEMAN
*University of Illinois,
Chicago Circle*

ARTHUR J. RAY. *Indians in the Fur Trade: Their Role as Trappers, Hunters, and Middlemen in the Lands Southwest of Hudson Bay, 1660-1870*. Buffalo, N.Y.: University of Toronto Press. 1974. Pp. xii, 249. \$4.50.

Although limited to the Crees and Assiniboines of present Manitoba and Saskatchewan, this is a model study of the fur trade in all its economic aspects. It consists of a series of topical essays, each thoroughly done. Arranged chronologically and buttressed by graphic figures and statistical tables, they form a very satisfactory history. Illuminating inferences from scattered statements in primary sources demonstrate the author's mastery of his subject.

Some of his interesting observations are that brandy gave the French traders an advantage that their English rivals were only partially able to offset with Brazilian tobacco; that droughts, fires, and epidemics were nearly as important as fierce competition and over-trapping in causing the decline of the fur-bearing animals; that sundries were prized by the Indians as much for their trade value to other Indians as for their intrinsic utility. Demographic changes are well delineated, but social changes are less often discussed. The viewpoint of the author is always that of the economist, never that of the moralist or the dramatist.

The overall conclusion is that the fur trade enabled the Indians to maintain a partnership with the whites for over two centuries. Never an equal partnership, it enabled them to survive and to retain a measure of independence. The Indians adjusted to changing conditions over which they had no control, much as this reviewer does in accepting his first paperback in a lifetime of reviewing.

HARVEY L. CARTER
Colorado College

ERNEST BENSON LOWRIE. *The Shape of the Puritan Mind: The Thought of Samuel Willard*. New Haven: Yale University Press. 1974. Pp. xi, 253. \$12.50.

Samuel Willard's seventeenth-century treatise on Puritan theology, *A Compleat Body of Divinity*, which has long been considered lengthy, difficult, dry, and tedious, is admirably explicated by Ernest Benson Lowrie. He arranges, clarifies, and analyzes the major doctrines in this classic compendium in a thorough manner. Unfortunately, he professes to do no more than give a "careful reading" of his text, and he specifically disclaims any effort "to trace the various ideas to their origins." This is regrettable, since the author affirms in the preface that Willard's "thought exhibits the depth and breadth of the cultural heritage New England received from England and the medieval universities." In his text, moreover, he draws a parallel with the "medieval mind," and he devotes a large part of his bibliography to Willard's European heritage. In a short epilogue, Lowrie suggests that the religious ideas of both Benjamin Franklin and Jonathan Edwards grew out of Willard's theological system. He offers no specific parallels, signs of continuity, or formal relations, however, except the biographical coincidence that Willard officiated at Franklin's baptism. Neither the presumed medieval antecedents nor the suggested eighteenth-century connections are developed in the main body of the book. It cannot, therefore, be considered in any sense a study in the history of ideas, for there is no history presented. Willard's

notions are treated in an absolute vacuum. George Keith, for example, is introduced as one of Willard's intellectual opponents, but nothing is said to identify him, not even that he was a Quaker. A list of Willard's publications reveals that all were printed in Boston, with the exception of some observations on witchcraft which were brought out by Bradford in Philadelphia. An intellectual historian might wonder why.

One major hiatus exists in Lowrie's exposition, even granting the legitimacy of studying a theological system abstracted from its predecessors, contemporaries, and followers. This hiatus is the debate over freedom and necessity. Both Calvin and Edwards denied the freedom of the will, as did many of Willard's contemporary Puritans. Lowrie does not raise the issue at all in the sense of a controverted point, but treats Willard throughout as taking the doctrine of freedom for granted. The latter's ingenious distinction between absolute and natural necessity would have offered an opportunity to determine whether, in Willard's view, pure contingency is ever possible. Unfortunately, Lowrie does not pursue inquiries of this nature.

A. OWEN ALDRIDGE
University of Illinois,
Urbana

ROBERT E. LEE. *Blackbeard the Pirate: A Reappraisal of His Life and Times*. Winston-Salem, N.C.: John F. Blair. 1974. Pp. viii, 264. \$8.95.

Though Blackbeard is almost legendary, surprisingly little is known about him except during 1717-18 when he attracted notoriety as a pirate. Such basic facts as his birthplace and his real name are obscure at best. After the peace in 1713 he apparently refused to remain an inconsequential legitimate mariner, continued sea-roving, and evolved from a privateer into a pirate.

Lee has mined Virginia and North Carolina sources, perhaps too much so, because sometimes in the narrative Blackbeard gets lost in the minutiae. A more fundamental weakness is that Lee does not fully comprehend the nature of English piracy in the Western Hemisphere. Piracy flourished in the 1660s and 1670s when Henry Morgan and his fellow buccaneers brought their booty from the sack of Panama to Jamaica. But during the 1670s European mother countries began to curb their American freebooters. Morgan became Jamaica's lieutenant governor, and before 1700 much of Port Royal had sunk beneath the waves. Pirates of a later generation, such as Blackbeard, were driven to the Bahamas, the Carolinas, and other peripheral areas. Lee's attempts to make Blackbeard another Morgan, or even a Sir Francis Drake, do not succeed. Moving beyond the Outer

Banks the author sometimes founders, such as in placing the Bahamas in the Caribbean, asserting that the Navigation Acts were a prime cause of piracy, or even putting tri-colors on French vessels of the period.

Lee portrays Blackbeard's North Carolina far more favorably than did Virginia's William Byrd. According to Lee, Governor Spotswood, who illegally captured Blackbeard, was almost the villain, while Governor Eden of North Carolina was a paragon of virtue. This argument has at least some merit, as do various aspects of local history. Historians are not likely to agree, however, with many of Lee's dicta. Americans today undoubtedly would still speak English rather than Spanish even if Blackbeard and his associates had never existed.

J. LEITCH WRIGHT, JR.
Florida State University

FRANCIS F. GUEST, O.F.M. *Fermin Francisco de Lasuén (1736-1803): A Biography*. (Publications of the Academy of American Franciscan History. Monograph Series, volume 9.) Washington: the Academy. 1973. Pp. xx, 374. \$17.50.

George Vancouver in 1793 attempted to immortalize the second great head of the Franciscan missions in California, Fermín Francisco de Lasuén, by naming the points of San Pedro Bay after him. Point Fermín to the west stands out boldly. That name is currently in use, but almost no one asks who Fermín was. Point Lasuén to the south, though a source of contention during the tidelands litigation, has an uncertain location and is almost never mentioned. Lasuén's contemporaries praised him highly, notably Governor Borica, Lapérouse, Malaspina, and Vancouver, and historians, from H. H. Bancroft on, acclaim him even in comparison to his famous predecessor, Junípero Serra. Lasuén administered Serra's nine missions plus nine more and over a longer period. In his time the missions were rebuilt more sturdily and with tile roofs. They grew prosperous, and the number of neophytes increased. But the word count on Lasuén will never catch up with that on Serra. A biography of the first father-president appeared in 1787, and several others followed; the present work 186 years later is the first of Lasuén.

Francis F. Guest, one of a cadre of modern Franciscans concentrating on this historical microcosm, benefits from earlier scholarship such as the documentary studies by Herbert Bolton, Maynard J. Geiger, Antonine Tibesar, and Theodore E. Treutlein on topics prior to Lasuén's presidency. Guest also makes good use of Finbar Kenneally's two volumes of Lasuén's writings. The author's research extends further and involves Lasuén's youth in the Basque provinces and his work at San

Borja in Baja California, at San Diego, San Juan Capistrano, and elsewhere during the seventies and early eighties. There are chapters on his relations with military governors Rivera, Fages, and Neve, with whom Serra clashed. Guest deals with the issues of how to avoid single missionary assignments, what to do about runaway Indians, and the duty as presidial chaplains. He is frank about missionaries unfit for assignment to the frontier and is modest in his estimate of achievements in arts and crafts. His stout volume, rich in insights on California in its middle years as a Spanish outpost, is the biography that Fermín Francisco de Lasuén deserves.

JOHN CAUGHEY
University of California,
Los Angeles

BERNHARD KNOLLENBERG. *Growth of the American Revolution, 1766-1775*. New York: The Free Press. 1975. Pp. xxii, 551. \$15.00.

ALAN ROGERS. *Empire and Liberty: American Resistance to British Authority, 1755-1763*. Berkeley and Los Angeles: University of California Press. 1974. Pp. xi, 205. \$8.95.

Published fifteen years ago, the late Bernhard Knollenberg's *Origin of the American Revolution: 1759-1766* is certainly one of the half-dozen most important books to appear on that subject. Based on prodigious research, it established that the shift toward a more restrictive colonial policy by Britain occurred not in 1763 but at least as early as 1759 and demonstrated in detail how a variety of British measures seriously antagonized many colonists between 1759 and 1764 and thereby constituted an important precondition for the violent outburst against the Stamp Act in 1765-66.

These two volumes extend Knollenberg's earlier discussion. Examining the experiences of the northern colonies—primarily Pennsylvania, New York, and Massachusetts—during the Seven Years' War, Alan Rogers argues that the "child, America, was already rebellious and plagued by doubts about the power held by her parent, Great Britain," by 1759. "While the struggle to drive France from the North American continent was being waged," he contends, "Americans from every social class experienced firsthand, or had some cause to fear the use of arbitrary [imperial] power." Some American discontent derived from the royal attempts to centralize Indian administration and still more from the overt condescension of British regulars toward American provincials and the "discriminatory military structure" that assigned American officers and soldiers to subordinate roles. Most, however, arose from the insistence by British military commanders that military

necessity overrode all other considerations. "Granted sweeping powers by the Crown," they "imposed embargoes on shipping, ordered press gangs into the streets and countryside to seize men and property, forced citizens to quarter soldiers in their homes, and insisted that the authority of colonial political agencies was subordinate to their own military power." Whereas in 1754 Americans had thought that their "liberty could be maintained while imperial authority was increased," their wartime experiences "stimulated the development of attitudes that led them to take an anti-imperialist stance and gave rise to the question of whether empire and liberty were [any longer] compatible."

Largely because of the narrowness of the research base and a limited focus, this reading of the causal importance of the Seven Years' War seems to require several important modifications. First, the author's failure to relate wartime military policy to larger currents of change in imperial behavior toward the colonies prevents him from seeing that the "assertive imperialism" began not in 1755 but in the late 1740s and was in many areas actually relaxed during the early war years when colonial assistance against the French seemed necessary. Second, the colonial reception of the Albany Plan of Union indicates that even before the war American leaders had strong feelings "about the dangers of arbitrary [British] power." Third, given the extraordinary outpouring of British patriotism by many American leaders during the war, it is by no means clear that Americans were so alienated by their wartime experiences as the author suggests. Finally, it is probable that the importance of the Seven Years' War for the origins of the Revolution lies less in the extent to which it may have contributed to American unhappiness with British rule than in still other psychological consequences and structural changes it produced: Americans had expectations for better treatment and a larger role within the Empire at the same time that there was less need for Britain to be conciliatory toward the colonies. Given these qualifications, all that can be said with confidence is that the particular wartime experiences Rogers describes confirmed the suspicions of many Americans about Britain's arbitrary exercise of power. Such confirmation probably intensified residual anxieties over the constitutional safeguards of colonial liberty and property, but was probably insufficient to weaken significantly the strong ties of affection to the British empire.

The Knollenberg volume does not suffer from limitations of evidence or perspective. Like the *Origin*, to which this book is a sequel, it is built upon extensive research. Indeed, the scholarly apparatus is actually intrusive. The text accounts for

only about thirty-five percent of the total number of pages, while the notes and bibliography take up almost fifty percent and twenty-seven appendices most of the remaining fifteen percent. Focusing primarily on the main developments in the dispute between Britain and the colonies from the repeal of the Stamp Act of 1766 to the outbreak of war at Lexington and Concord in 1775, it is useful for the authoritative and concise summaries it provides of the central issues and developments in the dispute and for reminding us of the significance of the substantive political issues which activated those determinants.

Yet it does not match its predecessor in scholarly importance. As stated in the front matter, Knollenberg's thesis is that "the Provocative Conduct of the British Parliament and Government" between 1759 and 1775 wrought such a profound "Change in the Minds and Hearts" of the colonists that neither "appeals to self-interest" nor "ties to a common heritage" could "prevail against the force of passion aroused by the shedding of American blood" at Lexington and Concord. Unfortunately, this thesis rarely informs the author's subsequent handling of his material, so that the volume lacks that clarity of argument and marshalling of supporting evidence that have been such a refreshing feature of Knollenberg's earlier works. At the same time, although the volume does provide the fullest discussion yet available of some previously little-noted irritants in imperial-colonial relations, it does not often leave the central areas of dispute to explore those on the periphery. Thus we are offered no discussion of the idiosyncratic, but important, ingredients that contributed to the make-up of a revolutionary mentality in colonies other than Massachusetts and New York, with the consequence that the richness and breadth that characterized the earlier volume is missing.

JACK P. GREENE
Johns Hopkins University.

JAMES E. CRONIN, editor. *The Diary of Elihu Hubbard Smith (1771-1798)*. (Memoirs of the American Philosophical Society, volume 95.) Philadelphia: the Society. 1973. Pp. xiii, 481. \$15.00.

Elihu Hubbard Smith was a bright young man of amazing energy and multifarious interests. His thoughts about activities in Connecticut and New York and involvement in medical and literary circles at the end of the eighteenth century merit scrutiny. Although he died shortly after his twenty-seventh birthday, Smith accurately, if a bit immodestly, describes himself as an individual whose "attainments certainly surpass those of most persons of my age . . . It is probable that the

same is true of my talents." In addition to obligations as a physician, he wrote an opera, published the first anthology of American poetry, joined the Connecticut Wits in a publishing venture, and served as friend and critic to dramatist William Dunlap and novelist Charles Brockden Brown. As a physician he tended patients and corresponded with figures such as Benjamin Rush and Benjamin Smith Barton. He also helped found the Hartford County Medical Society, and with Edward Miller and Samuel Latham Mitchell founded the *Medical Repository*, the first American medical journal. He was also a voracious reader, inveterate theatergoer, and compulsive writer. Though barely begun, Smith's life was already complex; he simultaneously involved his mind and efforts in numerous activities.

The diary includes not only entries spanning the period 1795 to 1798, but also weekly and monthly summaries, nearly two hundred letters, and several essays. Smith is adept with words, and his self-conscious literary effort is eminently quotable when characterizing people, probing philosophical or medical problems, evaluating dramatic and literary performances, or describing various social conditions. Smith's descriptions of living conditions and social habits lucidly recreate life. For example, his interest in fevers—a problem of intense public concern—and diseases frequently caused him to portray conditions of filth, dampness, and overcrowding which clearly destroyed health. When he casually records his own ill-health as he sets off to deliver babies or visit the sick, he unconsciously reveals a good deal as well. He provides useful descriptions of medical and scientific education and practices. Although Smith admitted that "we must look to Europe, for a long time, for a large portion of our scientific improvement," he also actively sought observations from qualified Americans. Knowledge about medicine, agriculture, indeed any useful knowledge, was to be shared, for the editors of *The Medical Repository* hoped to serve as "conduit-pipes, in conveying this information to the public." This young bachelor's opinions concerning family, child-rearing, sex roles, and the like are certainly fascinating. His family and kin remained important to him, and his correspondence and entries in the diary reveal significant details about family relationships. Particularly valuable in this regard are his lengthy and vivid recollections of his childhood. His fascination with sex roles and interest in Mary Wollstonecraft's writings crop up periodically.

We are indebted to James E. Cronin, not only for making this detailed diary available, but for sensible editing, which enhanced the work. A brief biographical sketch of Smith, a glossary of about fifty individuals, and terse yet illuminating foot-

notes are valuable additions. Cronin's rearrangement of certain sections to create a more coherent whole and recapture chronology strengthened the volume; omission of certain extraneous matter and material previously printed and available tightened it.

The diary certainly figured as a major and significant project to Smith, for during three years he devoted considerable time and effort to produce nearly half a million words in it. Smith thus expected to provide a valuable contribution to subsequent generations, and especially to historians. We are indebted to Cronin for capably preparing this legacy, which should enrich those interested in American studies, social and intellectual history, and medical history.

RANDOLPH S. KLEIN
Connecticut College

JAMES THOMAS FLEXNER. *Washington: The Indispensable Man*. Boston: Little, Brown and Company. 1974. Pp. xvii, 423. \$12.50.

Scholars of the American Revolution era will quickly recognize this abridgment of Flexner's four-volume biography of Washington. Apparently designed as a bicentennial contribution for the general reader, the book attempts to capture the essence of the larger work without burdening the casual reader with unnecessary impedimenta such as footnotes and a comprehensive bibliography. Consequently, anyone seeking a full treatment of any aspect of Washington's life or documentation of information must still consult the earlier volumes.

Flexner attempts to accomplish three main objectives. First, he discards all previous accounts of Washington and returns to eighteenth-century sources for his information, certainly a laudable decision in view of the slanted biographies written by glorifiers and debunkers alike. Then, as the title indicates, he contends that Washington was indispensable to the success of the American Revolution, the Constitution, and the establishment of a viable republican government from 1789 to 1797. And finally, he attempts to put the real flesh and blood on the marble bust and the posed portraits that most of us associate with the first president.

The author succeeds in depicting the successes and failures of a man destined for immortality. Starting from a modest background of a grade-school education and a small planter family, Washington was among the most precocious of all noted Americans, was always a leader, and showed early signs of greatness by being Virginia's most celebrated hero at age twenty-two. Yet not all was success, for he fell in love with Sally Fairfax, the wife of his friend George William Fairfax,

married the widow Martha Custis, and then wrote at the end of his life that his happiest moments were those that he spent in Sally's presence. Although he was a hero to Virginians for his early military exploits, he did not impress the British and thus did not achieve the military fame in the British army that he desired. As a general in the Revolution, he failed when he copied British military tactics and succeeded only when he used men and terrain intelligently in a hit-and-run type of warfare. As president, Washington also experienced success and failure—success in getting the country started but failure in preserving the unity among leaders that he considered so essential. And finally, a man who was loved and respected by his own soldiers, who impressed Abigail Adams with his social graces and other women with his impudence (the kind that women liked), and whose wife called him "her old man" had to possess human characteristics in a large way.

Not all readers will be happy with this biography. Partisans of Jefferson and John Adams may feel that their men have not received just treatment; and, if biographers come either to love or to hate their subjects, no one will be in doubt about Flexner's position. Yet with all this, the book can be highly recommended for the general reader.

ROBERT E. BROWN
Michigan State University

DUNCAN J. MACLEOD. *Slavery, Race and the American Revolution*. New York: Cambridge University Press. 1974. Pp. vi, 249. \$15.95.

"The American experience knows no greater tragedy than the old South's twistings and turnings on the rack of slavery." Charles Grier Sellers wrote those words about the antebellum South. Duncan MacLeod finds the crucial point in the development of that tragedy in the American Revolution. By expanding and intensifying debate over an institution already a century old, the Revolution's libertarian philosophy "laid bare" the anomaly of slavery in a society which championed freedom.

The fledgling antislavery movement embraced the Revolutionary philosophy, imbuing it with a religious character. To Quakers and Puritans, the sin of slavery embarrassed the nation abroad and corrupted it at home. But slavery, the author emphasizes, was not crucial to Northern economic or cultural life, as it was in the South. Equally attached to Republican principles and slavery, Southerners ultimately found it necessary to adapt those principles to the realities of human bondage.

Much of this is familiar ground; MacLeod's contribution is his careful examination of the process by which Southerners accommodated Revolutionary ideology to a slave society. It was a process

that led the South away from a "pure republicanism" into a legal and constitutional conservatism. It affected institutions such as the courts and political leaders who discovered that their economic and political interests demanded a personal accommodation to slavery. Tragically, the process reinforced racial prejudice because Southerners sought "in the character of the Negro the explanation for slavery which the Revolutionary debate demanded." Thus one legacy of the Revolution was a positive racism which explicitly denied the rights the Revolution proclaimed.

MacLeod's evidence linking the process of accommodation to Revolutionary ideology is not entirely persuasive. Nor can one easily accept his view—crucial to his argument that the Revolution was a watershed—that slavery became an avowedly racial institution only after the Revolution.

PETER M. MITCHELL
Seton Hall University

contributes to a better understanding of the background of Anglo-American colonization in Texas, giving an alternative to the viewpoint on colonization offered by the Austins. The papers presented here deal primarily with the background to the empresario contract. Some material in the introduction does not bear directly upon the subject and may be considered antiquarian in nature, although it contains interesting and little-known morsels about the history of the American Southwest and Texas.

The publication is fully documented, contains several interesting maps, and has a complete and useful index. The index does not have adequate cross-references, however, and is cluttered with the names of places where printed items cited were published and with the names of depositories housing documents mentioned.

JOSEPH MILTON NANCE
Texas A&M University

MALCOLM D. MCLEAN, compiled and edited by. *Papers Concerning Robertson's Colony in Texas*. Volume 1, 1788-1822. Fort Worth, Tex.: Texas Christian University Press. 1974. Pp. lxxi, 566. \$20.00.

This volume of the *Papers Concerning Robertson's Colony in Texas* is the first of one of the most significant and extensive collections (ultimately eight or nine volumes) pertaining to the early Anglo-American colonization of Mexican-Texas by the Texas Association. This small group of Tennessee and Kentucky promoters and land speculators were bent on taking advantage of the liberal Spanish-Mexican land policy in Texas. The group included Nelson Patteson, Sam Houston, John W. Overton, Andrew Erwin, Ira Ingram, John Harding, Amos Edwards, Robert Leftwich, and four sons, a grandson, and a nephew of North Carolina-Tennessee frontiersman and colonizer General James Robertson.

Much of the material in this volume appears in print for the first time and includes personal correspondence and official documents of various kinds that have been translated and edited by Malcolm D. McLean. Serious students of Texan history and of the westward movement in the United States will be forever grateful to the author and to the Kathryn O'Connor Foundation for making available important source materials so meticulously and accurately edited.

Volume I contains a lengthy, somewhat folksy, rambling introduction with a few unnecessary presumptions. It traces briefly the early Spanish and French expeditions and activities touching upon central Texas, supplies valuable biographical data on many individuals, contains an account of the preservation of the Robertson Colony Papers, and

DAVID P. MCKAY and RICHARD CRAWFORD. *William Billings of Boston: Eighteenth-Century Composer*. Princeton: Princeton University Press. 1975. Pp. xii, 303. \$16.00.

William Billings of Boston, based mainly on exhaustive research in primary sources, is unquestionably the long-awaited, definitive statement on "the foremost American musician of the eighteenth century" (p. ix). Billings may have lacked proper recognition in the past. But now McKay and Crawford place the Boston composer in the pantheon of America's culture heroes. No general study can hereafter ignore Billings, whose significance is so convincingly revealed.

Billings' unprepossessing appearance—deformed in person, blind in one eye, one leg shorter than the other, one arm somewhat withered—masked an "artist of the people," whose career paralleled his country's changing fortunes. His *Singing Master's Assistant* (1778) is "the most impressive published work by any American musician of the eighteenth century" (p. 77). Billings' rollicking prefaces—a prose of "comic ingenuity and verbal virtuosity" (p. 83)—mirror the swaggering self-confidence of revolutionary America. "I am not tied down by any rules laid down by others," he stated. He had strayed far from the Calvinist tradition of enforced musical simplicity. "O enchanting! O ecstatic!" wrote Billings in defense of the fuguing tune. "Push on, push on, ye sons of harmony . . ." (p. 177).

The authors place Billings' comic prose in a tradition of oratorical-literary resonance with François Rabelais, W. C. Fields, and Father Divine. If Henry Fielding were a possible prose model for Billings, the music itself has a more

archaic quality reflecting the isolation of Americans from the mainstream of European developments. American composers paralleled the medieval practice of the "successive" composition of the voices of a work rather than their "simultaneous" composition as in Palestrina and later composers. Billings' response, however, to the declamatory potential of his texts, according to the authors, recalls such masters as Monteverdi and Schütz.

"Father, compiler and publisher, composer, singing-teacher, and . . . self-declared *litterateur*" (p. 103), Billings was also a patriot. *Chester and the Lamentation Over Boston* "helped to focus the militant emotions the American Revolution aroused." "It is a truly valuable addition to America's symbols of democratic culture," remark the authors, "that a Boston tanner was our first native musical artist" (p. 189).

Introducing six chapters that combine narrative and analysis is a superb prologue, "Eighteenth-Century Sacred Music in New England," from which students of American culture will particularly profit. An epilogue interestingly surveys "The Reputation of Billings and his Music, 1800-1970." Numerous musical examples explicate the text. Appendices include important accounts of the inhibiting effect of copyright law, performance of Billings' music, and instructive illustrations.

THOMAS WENDEL
San Jose State University

GEORGE DARGO. *Jefferson's Louisiana: Politics and the Clash of Legal Traditions*. (Studies in Legal History. Published in association with the American Society for Legal History.) Cambridge, Mass.: Harvard University Press. 1975. Pp. x, 260. \$15.00.

In 1803 Jefferson's Virginia had experienced 184 years of local self-government; Jefferson's Louisiana could remember only a century of colonial rule, barren of even the slightest acquaintance with the democratic process. The task of bringing this relict of European imperialism into the union of "sovereign American states" was inevitably laden with monumental difficulty.

This compact study is a long-needed and excellently crafted examination of the variety of levels on which the Latin culture and society of Louisiana attempted to preserve its identity and maintain a continuity with its past even as it assumed a new role in the dominantly Anglo-American republic. Because of his particular attachment to legal history, George Dargo is intrigued by the fierce battle waged in the territory of Orleans between protagonists of the civil- and common-law traditions, which in many ways symbolized the clash of cultures. He contends that the

adoption by the territorial legislature of the Civil Digest of 1808 and its acceptance by Jefferson and the American authorities represented a significant resolution of the struggle and one which helped guide national policy thereafter.

The great value of Dargo's presentation is in his realization that the legal battle encapsulated an infinitely complex confrontation and in the skill with which he demonstrates that the 1808 accommodation was more than simply a legal decision. He explores and connects to his theme such diverse developments as the Burr Conspiracy and Edward Livingston's clash with Jefferson over principles of law in the famous *Batture* controversy.

Dargo allows a few careless blunders concerning legal aspects of the Burr trial to slip into his pages, but this is a minor flaw in a first-class study.

JOSEPH G. TREGLE, JR.
University of New Orleans

RENNARD STRICKLAND. *Fire and the Spirits: Cherokee Law from Clan to Court*. Foreword by NEILL H. ALFORD, JR. (The Civilization of the American Indian Series.) Norman: University of Oklahoma Press. 1975. Pp. xx, 260. \$9.95.

Rennard Strickland, a legal scholar and long-time student of the Cherokees, focuses on law as practiced by a people who apparently accepted the American system. He persuasively argues, however, that Cherokee law was not created from a dramatic cleavage with past "savagery." In fact, Strickland discovered that Anglo models were adapted slowly to existing tribal traditions—traditions that have remained among primitive Cherokee elements.

Viewing law as dynamic and organic, Strickland traces the development of the Cherokee legal system, its nurture in an Indian and mixed environment, and its adaptation to encroaching white civilization. Although the first written law was promulgated in 1808, the Cherokees already had a reverence for legality which persisted beyond the end of their judicial system in 1898. The idea behind the institution received attention in terms of the Cherokees' traditional law ways, their goals and values, and the development and adaptability of the codes. Strickland pushes Indian history beyond the simple narrative chronicle of events into the realms of institutional and theoretical studies.

Despite the merits of thorough research and promising insights in this work, some weaknesses stand out. Most obvious is the omission of probing examination of the crucial events in Cherokee legal history. Strickland gives only a cursory glance at the critical issue of Cherokee reunification in Indian Territory in 1839. He fails to explain how the

Cherokees managed to construct a new government in the face of bitter and bloody factionalism. Space given to copying Cherokee laws in their entirety might better have been allotted to their analysis.

Strickland's rigorous standards of scholarship will enhance this addition to the already widely acclaimed *Civilization of the American Indian Series*, and he is sure to find an appreciative audience among students of Indian and legal history.

GARY E. MOULTON

Southwestern Oklahoma State University

ROBERT L. ALEXANDER. *The Architecture of Maximilian Godefroy*. (The Johns Hopkins Studies in Nineteenth-Century Architecture.) Baltimore: Johns Hopkins University Press. 1974. Pp. 246. \$16.00.

In 1805, at the age of forty, Maximilian Godefroy, a civil engineer, came to the United States as a refugee from France and settled in Baltimore, where for twelve years he taught engineering and drawing at St. Mary's College. Self-educated in architecture, Godefroy was soon engaged in wide-ranging architectural activities, designing churches, banks, an exchange, a courthouse, monuments, burial vaults, and fortifications, though only one private house. A man of great personal charm, Godefroy was also erratic and temperamental, sometimes quarreling bitterly with those about him. When he fell on hard times in the panic of 1819, he suddenly left the United States for England. He remained in London for seven years and then returned to France to practice architecture in the provinces. He died about 1840.

Godefroy introduced into the United States the neoclassical forms and decorative details that he gleaned from French architectural books. Probably his most important American work was the Unitarian Church in Baltimore (1818), designed in an austere, boldly geometrical, classical style, which was popular in France a generation earlier. St. Mary's Chapel (1808) mixed Gothic elements with classical details. The Battle Monument (1822), perhaps the earliest civic monument in the United States, added Egyptian motifs to classical forms. His engineering work at Fort McHenry (1814) embodied ideas that he had previously set forth in a small book on military defenses appropriate to the United States. Godefroy's buildings, freely inventive and fusing a variety of stylistic elements, stood far outside the American mainstream of late federal and early Greek revival forms of the time.

Robert Alexander has labored mightily to bring together information on this little-known architect and his work. The footnote documentation is ex-

haustive. The extended discussion of conjectured prototypes of Godefroy's architectural ideas may seem excessive, however, to the historian with no specific interest in architecture. An inelegant prose style and the omission of a bibliography weaken the monograph.

PAUL R. BAKER

New York University

MICHAEL PAUL ROGIN. *Fathers and Children: Andrew Jackson and the Subjugation of the American Indian*. New York: Alfred A. Knopf. 1975. Pp. 373, xii. \$13.95.

Michael Rogin has written a thought-provoking study of Andrew Jackson's role in American history as viewed through the perspectives of psychohistory, marketplace analysis, and Indian-white relations. He argues that historians have failed to recognize that Indians were at the very center of Jackson's life and the age named after him. Indeed, America was "built on Indian graves." The replacement of Indians by whites on the expanding frontier symbolized America's maturation from childhood, its regeneration through violence, and its transfer "from nature to capitalist civilization."

In his encounter with the Indians, Jackson confronted not only another culture but also his own fantasies, longings, and fears. The subjugation of Indians helped him to "destroy lingering fears of feminine domination and grow securely to manhood." The familial language he employed in speaking to and about Indians, Rogin claims, had more than symbolic meaning. Jackson's entire life was allegedly a continual battle to establish "paternal authority." His generation contrasted its own materialism unfavorably with the heroism of the revolutionary fathers. "Too young" to fight in the Revolution, arriving "too late" in Tennessee to share the glory of James Robertson and John Sevier, suffering from "anxieties over bodily control," and uncertain whether he had actually "imbibed courage with his mother's milk," Jackson sought paternal authority through "just war" against the Creeks and later the Seminoles. His defeat of these Indians "emancipated him from parental domination" and established him in the ranks of the fathers. The major formulas of Jacksonian Democracy also developed in Jackson's dealings with the Indians. His new paternalism supposedly explains the war on "the Mother Bank" and other "Jacksonian reforms."

Although this is a fascinating book, it suffers from several serious defects. Rogin qualifies many of his statements with a "perhaps" or a "may have." Yet he ignores these qualifiers when he draws his conclusions and, thereby, forecloses al-

ternative hypotheses. There are serious problems involved in examining the human psyche to achieve a comprehensive analysis of motivation when the subject is dead and only traces of his personality can be analyzed. Not only is there no one accepted theory of human motivation today, but even the demonstrated existence of a recognizable pathology may not necessarily explain all of an individual's actions. Such problems relating to the writing of psycho-biography aside, the book also suffers from some factual errors and problems of interpretation, including sweeping generalizations that lack scholarly documentation. For example, Rogin's statements about Jackson's hatred of the Indians, and his defiance of the Supreme Court's decision in *Worcester v. Georgia*, among others, are refuted in my *American Indian Policy in the Jacksonian Era*. Psychohistorians will undoubtedly applaud the appearance of this book, but historians of Indian-white relations will find the author's tendency to overlook the frontier antecedents of many of Jackson's policies a major weakness. In spite of such problems, however, Rogin's attempt to penetrate into "the dark interior of Jackson's life and career" is interesting reading.

RONALD N. SATZ
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Martin

JOHN MICHAEL CUDD. *The Chicopee Manufacturing Company, 1823-1915*. Foreword by SIDNEY RATNER. Wilmington, Del.: Scholarly Resources Inc. 1975. Pp. xiv, 325. \$8.95.

This company history suffers from the limited nature of its primary sources. These consist chiefly of directors' and stockholders' minutes spanning the years 1823-1837 and a complete run of semiannual, annual, and special reports issued by Chicopee's chief executive, the treasurer, for the latter half of its history (1861-1915). On the basis of these, Cudd analyzes early corporate formation and structure and quantifies costs, output, sales, and profits from the Civil War onwards. Chicopee, a vertically integrated, cotton-manufacturing company, emerges in its antebellum years as one of the less-successful promotions of the Boston Associates. It was most profitable in the decade after the Civil War when three-quarters of production consisted of flannels. Then its dividends were consistently higher than the average found in a sample of New England cotton companies. In 1871 it declared its highest dividend, seventy percent. The three decades after 1885 saw the company's relative position decline, until in 1915 it was purchased by Johnson & Johnson to secure their wartime supply of medical gauze.

Because of the nature of the sources, we are

denied precise insights into the changing problems of production and marketing. To bridge the hiatuses, Cudd conjectures from the economic and technological performances of comparable textile companies studied by other business historians. This leads him into suppositions which at times are dubious. For example, estimates of cloth production from spindleage assume the constancy of at least nine technological and three economic variables. Additionally, Cudd could have explored the backgrounds of the first stockholders, directors, and mill managers with greater tenacity.

In short, this volume offers an externalized, and in parts protracted, view of the Chicopee Manufacturing Company's development. It is a useful, but hardly pathbreaking, study.

DAVID J. JEREMY
Westcliff-on-Sea, England

LAWRENCE J. FRIEDMAN. *Inventors of the Promised Land*. New York: Alfred A. Knopf. 1975. Pp. xviii, 344, xiii. \$15.00.

According to Lawrence J. Friedman, the "spread-eagle" patriotism of the early national and Jacksonian periods was permeated by sexism in the form of the ideal of "True American Womanhood" and racism as reflected in a widespread commitment to the removal of blacks from the United States. The vision of "the rising glory of America" was thus a constricting, diversity-denying image of white male supremacy. Friedman sees this narrow and bigoted patriotism as the outgrowth of an effort to combine "rootedness" or stability with a continual striving to live up to the ideal of individual and national perfection established by the revolutionary generation. The tensions created by this contradictory quest impelled patriots to reject equality for women and blacks and to place a wall of rhetoric between themselves and the complex realities of human existence.

There is clearly a measure of truth in Friedman's thesis. What we today call racism and sexism were obviously prevalent American attitudes during this period and presumably influenced the quest for an American nationality. But Friedman weakens his own case by trying to force too many individuals and movements into the same bag. As he sees it, every patriot was a narrow chauvinist, and even those white male reformers who sought enlarged opportunities for blacks and women fully shared the general commitment to white male supremacy. To sustain this image of a malevolent consensus, he engages in a number of questionable practices. His selection of individuals, incidents, and quotations to be discussed is even more arbitrary than is normally the case with intellectual historians, and his attempts at psychological pene-

tration seem loose or strained even by the flexible standards of psychohistory.

Friedman loads the dice by limiting his discussion of the early prophets of American nationality almost entirely to the Federalist or conservative school of thought. His pretext for ignoring the Jeffersonians is that the sage of Monticello was an "intellectual," who recognized that the patriotic crusade "was antithetical to the life of the mind." Yet Jefferson certainly considered himself a patriot and had strong views about American mission. Furthermore, in his role as a major political leader he presumably spoke for the hopes and aspirations of a large number of his countrymen. Friedman ignores the possibility that the Jeffersonian view of America as a testing ground for Enlightenment ideas of liberty and equality might have competed with the particularistic nationalism of Federalists. The real issue is why the Enlightenment promise of liberty and equality was not extended to blacks and women. But Friedman's commitment to demonstrating that American patriotic values were rotten to the core will not allow him to see the question in this light.

Even more lamentable is Friedman's misguided effort to show that Garrison and other radical abolitionists remained covert colonizationists and proponents of an exclusive white nationality even after they had rejected the American Colonization Society. His method is to seize upon a few isolated and tentative statements of sympathy for limited black-emigration projects. It seems incredible that anyone could actually read through Garrison's writings with their continual and eloquent insistence on interracial fraternity and conclude that he was a white nationalist at heart. When Friedman attempts to include in his colonizationist consensus the slavocrats of the deep South—men whose very identity depended on the presence rather than the absence of blacks—Friedman's bag simply bursts. This book began with a modicum of truth, but then obscured it by exaggeration and excessive speculation.

GEORGE M. FREDRICKSON
Northwestern University

STEPHEN B. OATES. *The Fires of Jubilee: Nat Turner's Fierce Rebellion*. New York: Harper and Row. 1975. Pp. xiii, 187. \$7.95.

Stephen B. Oates, of the University of Massachusetts, is perhaps best known for his good biography of John Brown, which appeared in 1970. This account of the Turner uprising in Virginia in 1831 is generally accurate and is filled out with imaginative suggestions which seem sensible. More might well have been done on the situation in the South during the decade prior to the outbreak and on the

machinery of control which confronted would-be insurrectionists. The impact of the rebellion is well done, and here some new material—especially from the unpublished diary of Governor Floyd of Virginia—is brought forward.

Oates' account differs markedly from that in William Styron's best-selling novel of 1967 on the same subject; it confirms the findings in Eric Foner's reader of 1971, the splendid documentary edited by Henry I. Tragle, also appearing in 1971, and the work on the subject by this reviewer, written some forty years ago.

HERBERT APTHEKER
Hostos Community College
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RAYMOND A. SCHROTH, S. J. *The Eagle and Brooklyn: A Community Newspaper, 1841-1955*. (Contributions in American Studies, number 13.) Westport, Conn.: Greenwood Press. 1974. Pp. xv, 304. \$12.95.

As its title suggests, this is not just the history of a newspaper, but a study of the relationships and interactions between that newspaper and the community in which it was published for more than a century. The *Eagle* and Brooklyn provide a particularly apt combination for such a study.

Despite being overshadowed by the newspaper giants in nearby Manhattan, the *Eagle* was for many years one of the more highly regarded American journals. Under the editorship of men like Walt Whitman and St. Clair McKelway, it earned an international reputation. Yet it was essentially a provincial newspaper, its fortunes closely intertwined with those of Brooklyn. The *Eagle* declined with the city and died in 1955, the immediate casualty of a long and bitter fight between its owners and the American Newspaper Guild, which culminated in a strike in January of that year.

This is greatly to oversimplify the many-faceted story of the life and death of a newspaper, which, in attempting to cling to the patterns of operation that had made it a success, could not adjust to the startling changes that were taking place around it. Its editors and publishers remained committed to a mystical concept of "the Brooklyn spirit" long after economic and demographic forces had tied it more closely to Manhattan. These same forces opened up a new potential for the *Eagle* on Long Island which was largely ignored.

The author—a nephew of the *Eagle*'s last publisher, Frank D. Schroth—provides a lively, if necessarily foreshortened, account of the growth and decline of a newspaper, while appearing to avoid any bias on the side of his uncle and other members of the Schroth family. His analysis of the relationships between the *Eagle* and Brooklyn are always perceptive, sometimes striking.

More studies of this sort would greatly enhance the history of journalism in the United States, which has been written almost exclusively in terms of its "giants," and with little regard for the kinds of interactions with their communities that give an added dimension to *The Eagle and Brooklyn*.

JOHN M. HARRISON
Pennsylvania State University

BRUCE SINCLAIR. *Philadelphia's Philosopher Mechanics: A History of the Franklin Institute 1824-1865*. (History of Technology.) Baltimore: Johns Hopkins University Press. 1975. Pp. xi, 353. \$15.00.

ELTING E. MORISON. *From Know-How to Nowhere: The Development of American Technology*. New York: Basic Books. 1975. Pp. xiii, 199. \$10.00.

Bruce Sinclair traces the history of the Franklin Institute of Philadelphia from its founding in 1824 as a mechanics' institute to its emergence as a very different kind of institution in 1865. Two themes run through his account. The first concerns the relation between science and technology. The founders of the Institute were convinced that the two should be brought together. They discovered slowly and painfully, however, that the process did not involve using scientific theories to enhance technology. Instead it meant applying the experimental methods and the intellectual rigor of science to technology. The Institute provided a forum in which Philadelphia's scientific community and its emerging industrialists could work out this new and useful relationship. The theme pervades almost all aspects of the Institute's activities—its important publication (the *Journal of the Franklin Institute*), its varied educational endeavors, and its scientific and technical research.

Sinclair's second theme concerns the problems of a scientific and technical institution in the rapidly changing decades of the pre-Civil War years. The Institute showed a considerable ability to adapt itself to changing circumstances and clients. Originally a mechanics' institute, it was later transformed by Alexander Dallas Bache and others into a national center for technical research and technical adviser to both state and national governments. By the time of the Civil War, however, the Institute moved in yet another direction as it sought to meet the needs of Philadelphia's engineering industries.

This is a first-rate study that demonstrates how the analysis of a local scientific institution can illuminate larger national issues. The research is detailed and thorough; the writing is clear and consistently holds the reader's interest. If the price seems high, the reader can be assured that he receives not only a thorough bibliography and a good index but also such unusual amenities as

twenty-six pages of illustrations and elaborate footnotes at the bottom of each page.

If Sinclair illuminates larger issues by analyzing a local scientific institution, Elting E. Morison attacks the "big questions" directly, for in his book the history of American technology becomes part of a prophetic vision. Morison agrees with Alfred North Whitehead that the business of "philosophers, students, and practical men" is "to recreate and reenact a vision of the world." Either "we must succeed in providing a rational coordination of impulses and thoughts, or for centuries civilization will sink into a mere welter of minor excitements" (pp. 14-15). It is to this task that Morison puts his very considerable analytical powers.

His argument is straightforward: man has developed a scientific and technological method which enables him to create technical systems that "in their operation profoundly modify and sometimes take the place of our natural environment" (p. 3). Technology has developed an internal logic of its own which has generated change that has destroyed the idea of progress, the governing vision of the industrializing world, and has brought into question the ends that technology presumably serves.

Morison suggests a new vision for a technological society. That vision is thoroughly humanistic, for the central assumption underlying it is that the "technological universe should be designed to fit and serve the human dimensions" (p. 169). His method similarly owes more to the humanities than to science. He writes of "visions" and titles his penultimate chapter "The Parable of the Ships at Sea." Although he reports that a scientist friend "finds my idea that the narrative can be a form of thought hilarious" (p. ix), Morison depends on that form of historical discourse in the bulk of his volume.

The heart of the work is a series of related essays on facets of the history of American technology, ranging from the builders of the Middlesex Canal to the growing importance of scientific knowledge, particularly as displayed in the development of the incandescent electric lamp at the General Electric Research Laboratory. A person reasonably well read in the history of American technology will find little new information in these chapters. But the essays are filled with lucid exposition of the changing modes of thought that characterized engineering in America and offer an impressive discussion of the growing interaction between science and technology in the twentieth century. Furthermore, the argument is presented in urbane prose that makes the book a delight to read.

The two books share at least one common concern: the relationship between science and tech-

nology. Both would probably agree that, as Morison put it, in the pre-Civil War period science offered few "general ideas or theoretical considerations that would help in building and making new things" (p. 90). While Morison is concerned with the provision of scientific theoretical underpinnings for technology in subsequent decades, Sinclair emphasizes the ways in which science's experimental method, intellectual rigor, and emphasis on original research had begun to influence Philadelphia industrialists before 1860. This is a topic that deserves much more research, but both books represent a significant contribution to the topic and to the general history of American technology.

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MERTON L. DILLON. *The Abolitionists: The Growth of a Dissenting Minority*. (Minorities of American History.) DeKalb: Northern Illinois University Press. 1974. Pp. xiii, 298.

Like oil wells, historical topics sometimes run dry. In this judicious study Merton Dillon, author of two fine antislavery biographies and several classic articles in the field, surveys the abolitionist experience from the American Revolution through the Civil War. Nevertheless, the procedure is a bit arid. Gilbert Barnes, Dwight Dumond, Louis Filler, Gerald Sorin, and others have traversed the same terrain with varying degrees of success. Dillon's contribution is no less worthy than theirs, and his mixture of sympathy and critical judgment provides a better balance than most other examples of the genre. Yet the book comes late in the day.

Dillon graciously acknowledges the work of recent scholars—Kraditor, Stewart, Thomas, Perry, the Peases, which he skillfully weaves into his organization; unfortunately, though, he does not challenge their sometimes contradictory views or move the topic into new directions. By and large, this work is a conventional narrative about the rise and impact of antislavery, chiefly in its political rather than intellectual phases. Given the author's undoubted knowledge of the complexities involved in abolitionist reform, the result is bland and homogenized. Not until the final pages does he raise a suggestive question: just how important were the abolitionists in the overthrow of slavery? Less than we might think, he replies, because, as he remarks earlier, "something beyond moral commitment and skill was needed to assure abolitionist success" (p. 83). Southern foolhardiness and volatility did more to stimulate anti-Southern reactions than

the actions of reformers alone. It may be an old perception, but he could have pursued it with greater vigor and precision than his predecessors did. Otherwise, Dillon hugs pretty close to familiar pathways. One would have liked to see less about Garrison and Tappan, more about antislavery lawyers who ingeniously forced the legal system to confront constitutional questions in the fugitive and personal liberty cases; less about the 1840 division and more about the sectional church schisms—in the mode of Donald Mathews; less about reform leaders in general, more about followers, about whom we still know little. As a straightforward, narrative survey, the work is certainly satisfactory, but as a provocative interpretation of antislavery significance, it is a mild disappointment.

BERTRAM WYATT-BROWN
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DAVID C. KLINGAMAN and RICHARD K. VEDDER, editors. *Essays in Nineteenth-Century Economic History: The Old Northwest*. Athens, Ohio: Ohio University Press. 1975. Pp. xiv, 356. \$12.00.

Viewed from a charitable perspective, this collection of eleven essays combines the talents of five seasoned scholars in economic history with the work of some newer contributors. This is a book of highly uneven quality, but there are some gems to be found in it. And it is produced handsomely. But viewed from another angle, it is astonishing that its publication can be justified at a time when university presses are constantly turning down excellent manuscripts of young scholars because of fiscal constraints. The longest of the contributions—an eighty-seven page essay on railroads and economic development from 1870 to 1890 by Jeffrey G. Williamson—consists of verbatim sections of Williamson's book, *Late Nineteenth-Century American Development* (1974). Another essay is merely a recapitulation of Roger Ransom's well-known published articles on social returns from canal investment. And I think two of the remaining chapters could never have passed muster with referees of even a middling-quality historical or economics journal.

What, then, of the other half of the book? The most valuable contributions here, apart from Williamson's, are by Robert E. Gallman and Richard A. Easterlin. It speaks poorly for the precision of the book's title that neither of these two essays treats principally the Old Northwest; both deal with agricultural output, income, labor force, and productivity for the nation as a whole in the nineteenth century. As one would expect of his work, Gallman's study reveals ingenuity and remarkable

industry in the gathering and analysis of data, together with rigorous presentation of quantitative measures from the prestatistical era. Gallman reconsiders earlier estimates (by Paul David and others), and he argues persuasively that agricultural productivity accelerated greatly after 1850. He offers a startling partial explanation: there was under-utilization of farm labor in the early nineteenth century. The hours of farm labor, he maintains, were possibly one-third less than the hours of contemporary manufacturing workers; there were many slack periods and "substantial leisure."

Easterlin's study provides invaluable estimates of output, labor force, and income of farms by state for 1840 and by region for 1840-1860. He addresses the intriguing problem of "start-up" low-income periods in the early development of what later became rich farming regions. Also considered is the question of productivity on Southern farms and plantations; Easterlin argues that the South did not compare unfavorably with other regions. Finally, he contends that productivity gains attributable to shifts to more fertile western lands were probably less important, at ca. 5.6 percent from 1800 to 1840, than other sources, which totaled about 30 percent.

David Klingaman deals with changes in wealth in three northeastern Ohio counties, in an essay that, despite its severely localized data base, links nicely with one by Lee Soltow on the growth of wealth in Ohio. The latter study leans heavily on the weak reed of tax valuation data from the early nineteenth century. An interesting essay by Don R. Leet argues that the decline in birth rates preceded industrialization and urbanization in early Ohio, a pattern largely explained by rising population-to-land ratios and consequent declining opportunity. But lacking in Leet's analysis is any consideration of alternative opportunities to life and work on local farms, or their effects on fertility rates.

Richard Vedder and Lowell Gallaway contribute a workmanlike statistical study of migration and the Old Northwest, long on interesting data but short on analysis. The other essays—on Old Northwest regionalism, on banking, and on land speculation—offer little that is novel to students of these subjects. Presumably Williamson's study will be evaluated, as it should be, in reviews of his own book—undoubtedly one of the most challenging works in the literature of American economic history, but hardly one that required partial reprinting in this format.

HARRY N. SCHEIBER
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WARD E. Y. ELLIOTT. *The Rise of Guardian Democracy: The Supreme Court's Role in Voting Rights Disputes, 1845-1969*. (Harvard Political Studies. Published under the direction of the Department of Government in Harvard University.) Cambridge, Mass.: Harvard University Press. 1974. Pp. xiv, 391. \$15.00.

ROBERT M. COVER. *Justice Accused: Antislavery and the Judicial Process*. New Haven: Yale University Press. 1975. Pp. xii, 322. \$15.00.

Whether inspiring or exacerbating, high-court decisions between 1954 and 1974 helped to shape the period. In turn, campus disagreements about public policies shape scholars' views of the more distant past. Most historians try to minimize this presentism. The authors of both books under review approach their subjects in candidly present-minded ways.

Ward E. Y. Elliott, a political scientist, has grappled with aspects of the history of Supreme Court decisions involving voting rights. It is unfortunate that he expended his large talents to prepare a book-length missile against many of his co-disciplinarians.

His argument appears to be that academic theorists, including some in judicial robes, were—and are—ill-equipped to solve democracy's problems. Elliott deplores the Court judgments favoring swift social changes rather than acceptable progress. He charges that the Court assumed a guardian responsibility which it could not sustain and which proved to be seriously out of phase with public opinion and with minorities' true needs. Elliott wants judges to be restrained priests, not crusading prophets.

Whatever the merits of Elliott's dour view of the Kennedy-Johnson-Nixon years, the *Rise* portions of his *Guardian Democracy* lessen the book's impact. Elliott's historical data fail to support his judgment that alternative policy institutions were ready, willing, and able to act if courts and judges did not, or that inaction was preferable to judicial intervention. As examples, Conron's and Gittleman's work on the Dorr Rebellion and *Luther v. Borden* raise questions concerning legislative sensitivity to which Elliott does not address himself. His Civil War and Reconstruction analysis depends on publications no more recent than the very early 1960s. A fair share of the rich newer work directly disagrees with Elliott.

History offers sharp alternatives to the author's convictions that only the representative branches of government should initiate or shape social changes. The Court's history suggests that the high bench should and can balance as well as check. And better balances were what the voting rights cases were about.

Robert M. Cover is a lawyer. He wrote *Justice Accused* out of Vietnam-inspired concerns about whether judges should express distaste for allegedly undesirable public policies or simply render judgments based on existing laws. Cover's compelling illustration of the judge's conflict between duty and conscience is Captain Vere in Melville's *Billy Budd*.

Cover's is a better and more useful book than Elliott's; its tone is calm and its logic and documentation are impressive. This reviewer is less sure than Cover that "Judges . . . are conscious of the baggage of the past" (p. 7). With notable exceptions, I find today's legal professionals dismayingly ahistorical. But scholars should be grateful to Cover for his often brilliant illumination of tensions created in judges by changing eighteenth- and nineteenth-century jurisprudential attitudes and legal standards.

Cover suggests that the narrowly positivistic Captain Vere-role that even antislavery judges imposed on themselves, helped produce the 1860 deadlock in democracy's representative institutions, and that this attitude carried over into Reconstruction with, as this reviewer would add, equally corrosive effects. Therefore, the advantage in litigations lay with slaveowners and amorlists before 1865 and with opponents to race equality after Appomattox. With respect to race most nineteenth-century jurists remained priests not prophets.

Cover rarely descends to professional jargon, even when dealing with complex, choice-of-law conflicts, comity situations, and legal codification. I remain unsure, however, why Cover believes that Warren (not William) Howard's *American Slavers* is "confused" (p. 286), why he asserts (p. 166 n.) that *Ableman v. Booth* rather than *Tarble's Case* should remain the "leading" one on its central question (the whole Civil War, including the 1863 Habeas Corpus and the 1866 Civil Rights Acts, separated the two cases), and why, considering the author's presentism, the ten Broek-Graham view of the Fourteenth Amendment is less rigorous, because presentist (p. 155).

Cover should have caught the variants on the Ohio town (p. 185) and on Bertram Wyatt-Brown's name (p. 287), as well as several distracting typos (pp. 98, 171, and 285). Dallin Oaks' *University of Chicago Law Review* article is not the "only discussion in the legal literature of the writ *de homine replegiando*" (p. 298); Catherine Tarrant deserves space on that uncrowded stage.

Cover's merits overshadow these shortcomings. He began this exciting adventure in interdisciplinary history with an apt reference to *Antigone*. This estimate of his and Elliott's book can end with another quotation from that useful model

of civil disobedience, where Creon says: "There is no art that teaches us to know/The temper, mind, or spirit of any man/Until he has been proved by government/And law giving."

HAROLD M. HYMAN
Rice University

THOMAS BENDER. *Toward an Urban Vision: Ideas and Institutions in Nineteenth-Century America*. (Published for the Organization of American Historians.) Lexington: University Press of Kentucky. 1975. Pp. xv, 277. \$14.50.

This stimulating book deals with one small part of the historical development of the theory of urban society. During the early nineteenth century the new textile machinery led to the building of manufacturing towns whose growth from scratch provides the resourceful historian with excellent opportunities to study the reactions to urbanism of participants and observers under relatively simplified conditions. The intellectual environment in which this urban industrial development occurred was highly influenced by romanticism with its preoccupation with the esthetic and moral values of nature. The city represented a challenge to these values and required those Americans who were unwilling to repudiate urbanism to attempt to reconcile city and country. The most obvious solution was to bring the country into town in the form of parks, the inspiration for which came from the earlier rural cemetery movement. Lowell, Massachusetts, a result of the narrowly conceived manufacturing plans of the Boston capitalists, provides the illustrative material for the processes of urban growth.

By mid-century, Lowell had ceased to be a seven-days' wonder and had come to exhibit the perennial urban problems of poverty, crime, juvenile delinquency, and class conflict; and neither its spokesmen nor contemporary observers seem to have been able to articulate its problems in a useful manner. The author now shifts his attention rather abruptly to New York City, where Charles Loring Brace and Frederick Law Olmsted continued to explore the possibilities of the romantic reconciliation of nature and culture. Brace struggled vainly to preserve the values of community by strengthening the primary-group relationship, even to the extent of rustivating homeless and delinquent youths in rural midwestern homes. Olmsted seems to have persisted to the end of his life in the quaint conviction that city parks would elevate and humanize their users while reconciling the divergent interests of social classes.

Thomas Bender considers this movement a lost historical tradition alternative to the dominant

pattern of urban thought, whatever that may be. But the environmentalism of this lost tradition is certainly compatible with the ecological approach to urban study, which must be one of the major modes of dealing with urban development.

STOW PERSONS
University of Iowa

IRA BERLIN. *Slaves without Masters: The Free Negro in the Antebellum South*. New York: Pantheon Books. 1974. Pp. xxi, 423. \$15.00.

Slaves without Masters is the first comprehensive study of Southern free Negroes before the Civil War. Ira Berlin's volume is a synthesis, drawing upon secondary materials as well as the author's own considerable research. This book is to some extent, however, a premature one. The field needs at present a large number of microanalytic local and county studies of free Negroes, utilizing such sources as census and tax records, local newspapers, and municipal archives. In the absence of these studies, Berlin necessarily must make frequent generalizations based on fragmentary evidence.

A good deal of the content of *Slaves without Masters* is to be found in the monographs of John Hope Franklin on North Carolina, Luther Porter Jackson on Virginia, and Donald Everett on Louisiana, among others. Furthermore, in Berlin's effort to flesh out his materials, he devotes an inordinate amount of space to his secondary theme—the nature of Southern race relations. For example, there is a great deal of unnecessary material on colonization.

Yet the book will attract a wide readership, both because of its overall picture of the status of free blacks through time, and because of the arresting contrast it draws between the free people of color of the upper South and those of the lower South. Berlin very soundly points out that the free Negro population of the deep South was much smaller and lighter skinned than the free blacks of the upper South. Only in the latter area had there been a substantial number of slaves manumitted during the era of the Revolution when a minority of slaveholders, motivated by the egalitarian ideology, freed a number of slaves who were not related to them by kinship ties. Yet paradoxically, although the deep South was more extreme in its proslavery ideology, the tiny group of free people of color living there was better educated, wealthier, and more skilled and enjoyed closer relations with aristocratic whites than their counterparts in the upper South. Berlin may overdraw the con-

trast, but he does put his finger on a very important matter.

Berlin's volume should stimulate other scholars to further in-depth research—particularly local studies of the antebellum free Negroes.

ELLIOTT RUDWICK
Kent State University

JOHN F. COLEMAN. *The Disruption of the Pennsylvania Democracy, 1848-1860*. Harrisburg: Pennsylvania Historical and Museum Commission. 1975. Pp. v, 184. \$5.50.

Pennsylvania was a complex state with wide economic, religious, and ethnic diversities, but since 1800 it had evinced an astounding fidelity to the parties of Jefferson and Jackson in its politics. Overwhelmingly agricultural, Pennsylvania was a banner industrial state, and its economic interests were seemingly at variance with those of the party to which it clung so faithfully. John F. Coleman notes that "adroit party leadership, the adhesive of the patronage, the disarray of the opposition, and, above all, the weight of tradition kept Pennsylvania in the Democratic ranks" (p. 14).

From 1848 to 1860 the state experienced a political revolution which overthrew the Democrats and ensconced the new Republican party firmly in power. Political success shifted rapidly under the impact of such issues as the tariff, nativism, temperance, the ten-hour day, and, most of all, Free-Soilism. The Whig party enjoyed a brief ascendancy in 1848 but quickly disappeared through its own disunity. The Know-Nothings surged forward in 1854, but then declined, although they remained a potent political factor throughout the period. Though the Democrats experienced mixed success on the issues, they suffered constantly and severely from the factional disputes of the Dallasites, Buchananites, and Cameronians. The Republicans achieved success by uniting the opposition and emphasizing "economic self-interest, nativist principles, and hostility to the further spread of slavery" (p. 148).

This is a welcome addition to the Pennsylvania Historical and Museum Commission's series on Pennsylvania politics. After two introductory chapters Coleman proceeds through seven narrative chapters to his conclusion and provides twelve appendices, mainly devoted to election statistics. He writes well and evinces a thorough command of his complicated subject. Unfortunately, a short review cannot present more of his conclusions, and regrettably the volume is so brief that these conclusions often lack desirable supporting detail.

SANFORD W. HIGGINBOTHAM
Rice University

PAUL H. MATTINGLY. *The Classless Profession: American Schoolmen in the Nineteenth Century*. (New York University Series in Education and Socialization in American History.) New York: New York University Press. 1975. Pp. xxiii, 235. \$15.00.

In this essay on nineteenth-century school leaders, Paul Mattingly reveals one of the ways that scholars have been writing history during the last decade. Compare this book, centered on the activities of the American Institute of Instruction from 1830 to 1918, to Richard Sennett's essays that took off from an analysis of family patterns in Chicago. The writer in each case began by selecting a group of people whose origins, career lines, and accomplishments could be laid out in analytical, quantitative form. The formal data produced mildly interesting information, but seem to have disappointed the writer himself, who then took off to exploit the subject as a way to exercise sensibility upon the minds of the people involved. With an organization like the AI, this intellectual tactic has considerable potential. The Institute began as a forum, which met in Boston, for men of professional, academic, or mildly patrician background, interested in promoting school reform. Over the years it included more working principals and administrators, and by the end of the century it turned into a slightly frivolous excuse for resort excursions.

Mattingly has concentrated on the ideas of the earlier leaders, drawing in material about teachers' institutes and other school endeavors. He shows these people talking about the need for teacher preparation, yet settling for a sense of that preparation as inspiration, revivalistic rousing, or the production of teachers with "character." In studying this nebulous rhetoric, he reveals their failure to perceive themselves in relation to either practical techniques or class identity. Referring to them as "schoolmen," he lets that term suggest that they were somehow representative of teachers in general. This still attributes too much importance to the rhetoric and too little to any function that the rhetoric played for people within a changing social structure. *The Classless Profession* pictures an elaborately textured mental surface. Did that surface, and its exclusion of technique, serve the same functions as does the culture of sensibility in the late twentieth century?

DANIEL CALHOUN
University of California,
Davis

FREDERICK M. BINDER. *The Age of the Common School, 1830-1865*. (Studies in the History of American Education Series.) New York: John Wiley and Sons. 1974. Pp. ix, 191. \$8.95.

PATRICIA ALBJERG GRAHAM. *Community and Class in American Education, 1865-1918*. (Studies in the History of American Education Series.) New York: John Wiley and Sons. 1974. Pp. xi, 256. \$8.95.

DAVID MADSEN. *Early National Education: 1776-1830*. (Studies in the History of American Education Series.) New York: John Wiley and Sons. 1974. Pp. vii, 162. \$8.95.

The history of American education has unfortunately been dropped as a requirement in many teacher-training programs at the time when the subject enjoys an emancipation from the narrow institutional and didactic constraints placed upon it by some of its founders. For more than a decade—largely through the work of Bernard Bailyn, Lawrence Cremin, Merle Borrowman, and their students—this field of study has outgrown its previous mission to document and celebrate the rise and triumph of both the American public school and our democratic form of government. Recently a group of younger revisionist historians set out to construct a radically critical interpretation, arguing that our schools have been the institutional means for inculcating middle-class values, which kept the burgeoning urban and immigrant proletariat law-abiding citizens and dependable laborers.

Taking advantage of the renewed interest in this subject, the editors of the series in which these three titles appear had hoped to provide "new interpretations of American educational history based on the best recent scholarship." Judging from these three books they have been only partially successful. The works range from an uninteresting restatement of old material to a fresh attempt at understanding the complexity of the relationship between schooling and the culture in which it is imbedded.

Frederick Binder's work on the common schools contains little that is new. Some attempt is made to counteract the excessive attention given to the New England reformers at the expense of leaders in other parts of the nation. He has also included an informative section on the elitist image that the first public high schools had in the minds of the wage earners who opposed them. This aside, his general thesis assumes that the public schools have been the most appropriate educational institutions for our democracy. Following this line of reasoning, he produces the thumb-worm album of heroes such as Horace Mann, Frederick Barnard, and leaders in the workingmen's societies along with the unflattering portraits of the unenlightened and perverse who differed with common-school reformers in approaches—such as the Boston Schoolmasters—or simply preferred private, religious, or familial alternatives—such as the officers of the American Sunday School Union.

Patricia Graham's work is in an entirely different mode. In place of a more general survey, covering a half-century of developments following the Civil War, she has studied four communities and two universities in depth and provides the reader with local case studies on the establishment and extension of comprehensive systems of schooling. Although such research is needed, the sum of the parts fails to make a whole. The fault here lies in her failure to state at the outset a clear rationale for her research. Not until the end are we informed why counties in Indiana, Michigan, and Alabama and the city of New York were singled out for study. In addition, the comparison between the changes at Princeton and those at the University of Wisconsin appears as an addendum on higher education rather than an integral part of the previous studies. This lack of a lucid basis is symptomatic of a larger problem, which is not satisfactorily solved by the author. Although her facts and statistics are impressive, her work lacks a clear conceptual framework that would order the data and make them relevant. What unifying theme there is again must be found at the end of the book. There she informs us in almost truistic terms that "where one lived was a fundamental determinant of one's educational opportunities."

Of the three authors, David Madsen has been the most successful in setting up a framework that makes the educational developments between 1776 and 1831 more understandable. Attempting to escape the narrow institutionalism, which has in the past choked off a more comprehensive educational history, he first turns to the political and cultural life of the new nation. This enables him to describe how much of a child's learning was familial and communal and, therefore, relatively unstructured and informal. This framework also reveals a basic social fabric upon which a number of educational planners attempted to superimpose their various systems of formal schooling. Once recognizing how intimately the mores of the postrevolutionary period were interwoven in the agrarian society of small landowners, one can see why Jefferson's "enlightened farmers" supported voluntary local arrangements—from district schools to academies—in preference to the national school system advocated by some of the republican intelligentsia. Because Madsen understands the life of the people, he makes a convincing case for why they educated their children as they did.

According to the scholarship of Ellwood P. Cubberley and Paul Monroe, the public schools have liberated our people from ignorance and enabled them to enjoy the greatest degree of equality of opportunity a free nation has ever achieved. A more recent and radical interpretation asserts that our schools have been custodial institutions, which

regiment children through a bureaucratic control completely unresponsive to the will of the public. The problem with the books presented here is not that they support one position or the other, but that they avoid coming to grips with this question. Schools may liberate and they may coerce; they may promote either cultural pluralism or homogeneity. It is unfortunate that these works carry us but a little way toward a better understanding of the manner in which the schools have made a difference in the development of our culture and the degree to which they shaped the personalities and enlarged the opportunities of the children attending them.

JONATHAN MESSERLI
Fordham University

G. N. SEVOST'YANOV, editor, *Osnovnye problemy istorii SShA v amerikanskoi istoriografii: Ot kolonial'nogo perioda do grazhdanskoi voyny 1861–1865 gg.* [Basic Problems in the History of the U. S. A. in American Historiography: From the Colonial Period to the Civil War, 1861–1865.] (Akademiia Nauk SSSR, Institut Vseobshchei Istorii.) Moscow: Izdatel'stvo "Nauka." 1971. Pp. 373.

This volume includes essays on American historiography of twelve major themes in United States history during the period before the Civil War. They range from the treatment of the Indians to the writings of Franklin and Paine, the influence of the frontier, and the abolitionist movement, and all essays show their compilers to have engaged in a surprisingly broad and diligent study of the relevant writings. One finds citations of material ranging from the earliest days to, approximately, the mid-1960s, and including not only such prominent historians as Bancroft, Turner, Beard, or Nevins, but also the more obscure chroniclers of the events of their own time, such as the New York clergyman A. McLeod, whose book of 1815 on the War of 1812 is cited to show something of then current opinion. It would be difficult for one reviewer to state with certainty that the expositions which these Soviet scholars make of the themes and interpretations of all the American historians referred to are always succinct and just, but there is evidence of a scholarly approach and of an appreciation of the complexities of American events and of their analysis. The influence of the Soviet world view is, of course, always evident, and this reviewer finds an implicit gradation of American historians against the scale of the degree to which they agree with Marxist and "progressive" points of view. Yet those who have some familiarity with other Soviet writings on American history can see that the analyses offered here are based on a wider knowledge of the literature and a greater feeling

for the nuances than has, at times, been the case previously. For example, M. V. Demikhovskii's study of the frontier and federal land policies manages, without citing references that Marx and Lenin made to the topic, to indicate that the Homestead Act may not have had so marked an effect as other Soviet historians, notably A. V. Efimov, have suggested.

There are, unfortunately, few American scholars whose knowledge of their own history is combined with a knowledge of the Russian language, so that this volume will undoubtedly find few readers in this country. This is disappointing, not so much because this volume provides any major new interpretation of American historiography, but rather because Soviet "Amerikanistika" deserves to be taken into account as an area of serious scholarship. Beyond the flow of ephemeral Soviet works dealing with the United States, there is a group of books and articles, particularly in history and literary studies, that is worthy of attention. Except for some studies in diplomatic history or comparative literature, these publications provide little in the way of new sources, but they often contain evidences of great diligence and of an effort to extend and refine Marxist interpretations of America. The present volume falls within this framework and, therefore, should be of interest to those who wish to see a very different point of view of American historiography.

ROBERT V. ALLEN
Washington, D.C.

BENJAMIN FRANKLIN COOLING. *Symbol, Sword, and Shield: Defending Washington during the Civil War*. Hamden, Conn.: Archon Books. 1975. Pp. 300. \$12.50.

WILLIAM C. DAVIS. *The Battle of New Market*. Garden City: Doubleday and Company. 1975. Pp. xii, 249. \$8.95.

These two modest military histories, one dealing with a neglected and one a saturated subject, are well-composed, solid works which should become useful additions to the library of Civil War literature. Benjamin Franklin Cooling, Assistant Director for Historic Services at Carlisle Barracks, was fascinated by the wartime defense works around Washington as far back as childhood. Failing to find an adequate book on the subject, he has now written one himself. William C. Davis, editor of *Civil War Times Illustrated*, tackles a topic—the Battle of New Market—rich in lore and legend, but lacking a recent synthesis and evaluation.

Washington, "symbol" of the Union, was protected by the Army of the Potomac, the "sword," and a \$1,400,000 set of defense fortifications, the

"shield." At the time of First Bull Run the capital was virtually defenseless. The Confederate threat taught an important lesson. Forts were hastily thrown up around the perimeter of the District and across the Potomac in Virginia. In addition, the responsibility of safeguarding Washington would govern every future move of the Army of the Potomac. The cost was great, and although no serious challenge occurred, save for Early's raid, the author concludes that the investment to protect the capital was worth it. While a well-mounted Confederate force could possibly have broken into the city, the deterrent effect of the forts discouraged such a move.

Cooling has researched, organized, and written the story well, but the book suffers from the absence of readable maps. The innumerable diagrams of the various forts are of interest, but would be more useful if their positions were known. Readers must make do with one microscopic map of Greater Washington, which defies even a four-inch magnifying glass, and a sketchy layout of the Northern defenses at the time of Early's raid.

Maps are even more important in Davis' blow-by-blow account of the New Market affray of May 15, 1864, and he supplies them—good ones, too—in abundance. Moreover, he has read everything ever written about the battle and has squeezed the essence into this compact account. At times the compactness almost overpowers a reader not careful to keep up with all the regimental positions as they shifted about on that hectic day. But if you like your battles in minute-by-minute portions, you could ask for no more here.

Although Davis has provided us with a good up-to-date narrative of New Market, it is unlikely the student will discover a great deal he did not know before reading the book. Davis lays bare the shortcomings of the incompetent Franz Sigel, commanding general of the Federal forces. The fine generalship of John C. Breckinridge, the Confederate commander, emerges clearly, although one might wonder if Breckinridge was really "another Stonewall Jackson," if such a judgment is to be based on this one battle in which he faced an inferior foe. Davis notes, too, the heroism of the Virginia Military Institute cadets, but he is quick to point out how the legend of their noble effort has far surpassed the reality. There were, after all, 5000 or so other Confederates at New Market in addition to the 260 cadets. The Confederate victory at New Market certainly eased temporarily Lee's problems in Eastern Virginia. Still, the possibility remains that the struggle was not quite as significant as the author suggests.

E. C. MURDOCK
Marietta College

HOWARD N. MEYER. *The Amendment That Refused to Die*. Radnor, Pa.: Chilton Book Company. 1973. Pp. xii, 250. \$7.95.

Despite its melodramatic title and the lack of notes or citations, this book by Howard Meyer is well worth reading and recommending for constitutional history classes. Meyer's book is more of a tale, filled with anecdotes and vignettes, than a work of legal or historical analysis. It is the story of "Big Fourteen," as the author calls the Fourteenth Amendment, from its inception in Madison's proposal in 1789 to extend key Bill of Rights provisions to the states, through its ratification in the late 1860s, on to its distortions in the late nineteenth and early twentieth centuries, and finally to its resurrection in the 1920s.

Often simplistic and seemingly oblivious to legal and historical controversy (progressive historiography is still alive and well with non-professionals), Meyer's book is nevertheless solidly based on the legislative and judicial history of the Fourteenth Amendment. This reviewer is convinced that Meyer is right in the long, bitter debate over whether the Fourteenth Amendment was intended to include the Bill of Rights. The Fourteenth Amendment did indeed contemplate a national and unitary citizenship for all Americans; and the rights of that citizenship surely included the principles of the Bill of Rights.

Meyer's review of the twentieth-century history of the Fourteenth Amendment is more spotty but includes short chapters on a wide range of topics—the rise of the NAACP, the women's suffrage movement, the dramatic civil liberties cases of the 1920s, and the judicial breakthroughs of the 1940s and 1950s. The emphasis is biographical and episodic, touching on dozens of distinguished personalities who contributed to regeneration of the Fourteenth Amendment.

Doubtless contemporary psychohistorians and behaviorist quantifiers will smile at much of Meyer's presentation. But it is salutary that there continues to be some old-fashioned, "good-guy" history, especially in so forbidding a field as Fourteenth Amendment historiography. Meyer will hardly replace Howard J. Graham or Edward S. Corwin on the Fourteenth, but the epic chronicle still has its role in the history of a people and their institutions.

ARNOLD MILTON PAUL
*University of California,
Santa Barbara*

JOHN Y. SIMON, edited, with notes and foreword by. *The Personal Memoirs of Julia Dent Grant [Mrs. Ulysses S. Grant]*. With introduction by BRUCE CAT-

TON. With a foreword by RALPH G. NEWMAN. New York: G. P. Putnam's Sons. 1975. Pp. 346. \$12.50.

PAUL E. FULLER. *Laura Clay and the Woman's Rights Movement*. Lexington: University Press of Kentucky. 1975. Pp. ix, 216. \$12.50.

The two women who furnish the subject matter for these two books played two entirely different roles. Julia Dent Grant was the wife of Ulysses S. Grant, and very conscious of this fact. Throughout the book she refers to herself by her husband's titles, and she refers to other women in the same way. In fact, sometimes only her use of a new title indicates that her husband has been promoted or changed jobs. Laura Clay was a feminist, and after 1888 she became a power in the suffrage movement, first in her home state of Kentucky, then later on the national scene. Both books probably owe their publication to the current concern about women.

Mrs. General Grant, as she often called herself, wrote her memoirs after the death of her husband in 1885, partly to supplement his own memoirs, partly to defend her husband against any imagined slight, and partly, if not mainly, for money. One of the reasons the memoirs did not appear in her own lifetime was that no publisher would meet her price; ultimately reconciled to this she tried to sell the manuscript to Andrew Carnegie for \$125,000. She rarely discusses issues in the world at large, perhaps because she regarded her work as supplementing that of her husband. There is little evidence in the memoirs that she understood most of the issues. She is certain that her Ulysses was a good man; she goes to great effort to clear up any alleged drinking episodes and deny that her husband had any part in the scandals which afflicted the White House. Much of her concern is to tell us what she wore, where she sat, what she ate, and whom she met. Occasionally she offers some interesting sidelights, but even with the Lincoln assassination she concentrates on her husband. A large part of the book is devoted to the trip around the world that she and the general undertook after they left the White House. Julia Dent Grant was a devoted and proud wife, as well as a concerned mother. The book is ably edited by John Y. Simon, who identifies most of the individuals and incidents mentioned. Its value is for the insights into Julia's personality; and inasmuch as Julia reflects other women of the time, the book offers a glimpse into the limited lives of the upper-middle-class women married to the powerful and famous.

Laura Clay is a different kind of person, although her father, somewhat unsuccessful as a businessman or farmer, had served as Lincoln's

minister to Russia. The Clay family stayed only a few months in Russia, however, before her mother, Mary Jane Clay, left her husband and returned to Kentucky with her family. Here Mary Jane took over a nearly bankrupt family farm and made it extremely successful. After the return of her husband there was an attempt to reunite the family, but this failed because of the philandering of Clay. Mary Jane sued successfully for divorce, but in the process lost the benefits of most of her life's work. This inequity imposed upon their mother apparently impressed the Clay women, for all entered into the suffrage movement, although only Laura, the spinster, devoted her life to it. She was able to do so because of an inheritance from her father, part of the family farm that her mother had built into a thriving enterprise. One of the main themes in Paul Fuller's able account of the suffrage movement is the importance that money played in its leadership. Laura Clay, in part because she controlled some funds, loomed large on the national scene, and she was supplanted only when others who had more money or access to more money emerged.

Clay's career with the women's movement was not without controversy, and Fuller devotes considerable space to recounting Clay's break with the movement over the decision of its leadership to go full speed ahead for the Nineteenth Amendment. Throughout her career Clay had adhered to a states' rights position, and she did not change easily. Fuller attempts to explain Clay's backing of a literacy test combined with women's suffrage in the Southern states as a realistic approach instead of playing to the inherent racism of the region. His argument fails to convince, although he makes it clear that Clay was no bigot; this was evidenced by her campaign for the election of Al Smith in 1928, despite her long-time support for the WCTU. Her support of the WCTU, and perhaps her support of literacy tests in the South, can be explained in part by a willingness to face political reality. It is obvious that she was very much a woman of her time and region. She was also a politician who believed that women's rights were the most important issue facing the country. Although she had strong principles, the goal that counted most was that of women's rights, and she fought for this in the Church, in the Democratic party, and on the national scene.

The author has used the Laura Clay papers at the University of Kentucky, a particularly valuable collection since most of the other leaders destroyed their own papers. Laura Clay's career presents the other side of woman's life and a clear contrast to the experience of Mrs. General Grant. For her role Laura Clay had to sacrifice marriage and family,

the very things that Julia Dent Grant held most dear.

VERN L. BULLOUGH
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KRISTIAN HVIDT. *Flight to America: The Social Background of 300,000 Danish Emigrants*. (Studies in Social Discontinuity.) New York: Academic Press. 1975. Pp. viii, 214. \$13.50.

H. ARNOLD BARTON. *Letters from the Promised Land: Swedes in America, 1840-1914*. Minneapolis: University of Minnesota Press, for the Swedish Pioneer Historical Society. 1975. Pp. vi, 344. \$16.50.

DOROTHY BURTON SKÅRDAL. *The Divided Heart: Scandinavian Immigrant Experience through Literary Sources*. With a preface by OSCAR HANDLIN. Lincoln: University of Nebraska Press. 1974. Pp. 393. \$20.00.

EMORY LINDQUIST. *An Immigrant's American Odyssey: A Biography of Ernst Skarstedt*. (Augustana Historical Society, number 24.) Rock Island, Ill.: the Society. 1974. Pp. xi, 240. \$5.95.

Each of the books under consideration contributes to a broader and clearer view of Scandinavian emigration and of Scandinavian-American life.

Kristian Hvidt's *Flight to America* is a welcome response to a long-felt need for a scholarly interpretation of Danish migration to the United States in the nineteenth and twentieth centuries. With a remarkable economy of words, he views this migration from 1868 to 1914 against a background of European and Atlantic forces and compares it with that of other countries, especially Norway and Sweden. His major source is a collection of emigration records kept by police officials, mostly in Copenhagen, and he deals statistically with persons who left Denmark before 1900, after which time the records become somewhat unreliable. After analyzing his data with the aid of a computer, Hvidt proceeds with a search for the "why." Although his answers are similar to those of scholars who have studied the migrations from Norway and Sweden, he makes clear that 44 percent of Danish emigrants before 1900 were from towns or cities and that few land owners joined them. His explanation is found in the relatively early industrialization of Denmark and in geographical conditions that had made internal migration easier than in Sweden and Norway. This industrialization was not sufficient, however, to absorb easily the increase in population; as a consequence, many people left either for Copenhagen or America, or both. The Danes who emigrated were those who had the greatest difficulty in adjusting to a chang-

ing society—craftsmen who were the immediate victims of industrialization, wage earners who heard of better opportunities in the New World, and rural workers unable to acquire land holdings before 1899.

Hvidt recognizes the importance of popular education in promoting emigration and emphasizes the role played by personal relationships across the Atlantic. His treatment of settlement in the United States and of Danish-American life, however, is sketchy in the extreme. Unfortunately, this English translation of his original *Flugten til Amerika* does not contain his brilliant analysis of the activity of steamship lines, railroads, industry, and government agencies—the “intermediaries and the connecting links between the push and pull factors” in migration.

Although some “America letters” written by Swedish immigrants have been translated, edited, and published in periodicals, H. Arnold Barton is the first scholar to bring a substantial number of these together in a single volume. His *Letters from the Promised Land* thus provides students of immigration with a collection of valuable sources. He has drawn some materials from accounts of Swedish travelers, reminiscences, and diaries, but in the main the documents he uses are letters written by persons, often of slight education, actually experiencing the hardships, frustrations, successes, and excitement of immigrant life. Barton has not hesitated to edit the letters liberally, but he has nevertheless succeeded in retaining their original flavor. In addition, he offers a short introduction, an excellent background essay for each of the three parts into which the documents are divided, a helpful epilogue, and a select bibliography. His footnoting is both skillful and useful to the person unfamiliar with Swedish terminology and institutions.

As Barton’s subtitle, *Swedes in America, 1840-1914*, suggests, he has attempted to cover, in time, the whole of Swedish immigration in the nineteenth and twentieth centuries; he has also applied the same broad measurements to space, even reaching out to Hawaii. In part one he deals with the pioneers in the Middle West, South, Far West, and East to 1864, then turns to what he calls the “Great Farmer-Land in the West,” a story he carries to 1889 in part two, and covers in part three “Farm, Forest, and Factory” from 1890 to 1914. Barton has attempted to produce a general review of Swedish migration with sources and short interpretive sketches, and has been influenced as much by reader interest as by a need to document major episodes in the Swedish story. He has succeeded remarkably well, and the Swedish Pioneer Historical Society deserves praise for publishing his book.

In *The Divided Heart*, Dorothy Burton Skårdal attempts to interpret the painful assimilation of Scandinavian immigrants into American life through the imaginative writings of Danish, Swedish, and Norwegian immigrant authors. Although the almost countless writers she includes tend to become lost in notes at the rear of the book, their comments on a variety of themes—the European background, the Atlantic crossing, cultural adjustments and conservation, and American economic, social, educational, religious, political, and other practices and institutions—are quoted at length in the text.

Skårdal is particularly interested in the changes wrought by American materialism on old-country values and in the changes that took place in the New World in immigrant morals and mores, and she gives considerable attention to the problem of success in the New World and the pressures it brought to bear against the European heritage. In the end she describes the “pain of a divided heart” and draws up a balance sheet of loss and gain for the immigrant. She forcefully urges the importance of creative literature as a source for the study of Scandinavian-American life; few historians will now dare to ignore it wholly in favor of the more matter-of-fact remarks found in “American letters” and similar records. It would be folly, however, to assume that migration can be understood solely from the imaginative works of sensitive poets and novelists, even when they are in agreement. One may ask, too, if a volume focusing on the personal sufferings, frustrations, and creative works of a gifted group of transplanted writers would not have contributed even more to an understanding of the complex problem of assimilation and the subject of surviving ethnic feelings among the “old immigrants.”

Emory Lindquist presents a careful study of Ernst Skarstedt, one of the authors discussed briefly by Skårdal. Lindquist titles his biography *An Immigrant’s American Odyssey*, an apt term for the American career of the Swedish vagabond-poet-editor-farmer-champion of freedom. Skarstedt may not have been a top-flight poet, but his observations of the Swedish immigrant, especially in *Svensk-amerikanska folket i helg och söcken*, are both detailed and penetrating. Son of a well-known professor of theology in the University of Lund, he took to the sea early in life, scorned his European class advantages, and came to the New World in 1878. Here he lived much of his life as something of a hermit in the Puget Sound region, writing constantly and leaving his farm from time to time to work on Swedish-language newspapers in Chicago, New York, and San Francisco. Fortunately, Skarstedt kept detailed diaries from early years

and Lindquist makes good use of them as well as the published writings of the poet. What emerges from the biography is something much more than an eccentric rebel and cultural conservationist—a gifted individualist who decries the superficial trappings of American life, but champions the basic ideals of his adopted country and, surprisingly, defends the performance of the captains of industry in an age of exploitation.

All four books are well edited, contain adequate bibliographies and indexes, and—with the exception of *The Divided Heart*—are illustrated. More significantly, their authors also accept the presupposition of the progressive school of historians: complete assimilation of the immigrant into an improving American society. The common theme therefore is transition. Together, they are a valuable addition to scholarship in a long-neglected but vital phase of American history.

KENNETH O. BJORK
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RICHARD E. LINGENFELTER. *The Hardrock Miners: A History of the Mining Labor Movement in the American West, 1863-1893*. Berkeley and Los Angeles: University of California Press. 1974. Pp. viii, 278. \$12.50.

The subtitle of Richard Lingenfelter's monograph accurately defines the scope of this chronological and institutional history of the hardrock miners' unions. The introductory chapter characterizes the miners and their environment; the remaining chapters trace the course of these industrial unions from their inception in Virginia City to the formation of the Western Federation of Miners in 1893. The book complements the existing literature by describing the fitful extension of unionism from the 1860s to the early 1890s. Though miners lacked a regional organization before the WFM, individuals transmitted the union idea, as evidenced by the widespread adoption of local constitutions modeled upon those drafted on the Comstock and in Butte.

Using newspapers, periodicals, and existing union records, Lingenfelter examines stands on the controversial issues of wages, hours, technological innovations (for example, dynamite), and Chinese laborers. He argues that miners' organizations were a direct response to impersonalization stemming from "growing industrialization, capitalization, and absentee ownership of the western mines" (p. 30). He views the organizations as but another aspect of the national reaction to the extension of industrial capitalism.

The study does not define precisely how the term "industrialization" is applied to hardrock mining. Focusing upon the unions and the capital-

ists, Lingenfelter gives scant attention to the sentiments and reactions of resident owners of small properties or of unaffiliated mine laborers. He shows clearly, however, the link between declining mineral prices and management pressure for wage reductions. In the 1890s this situation became the fateful catalyst for the Coeur d'Alene labor war.

Though strikes and crises predominate, this work does cast additional light upon routine union activities and thereby enriches our understanding of miners and their concerns. Carefully researched and well written, it makes an important contribution to the history of both the American labor movement and the American West.

RONALD C. BROWN
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D. C. BLOOMER. *Life and Writings of Amelia Bloomer*. With a new introduction by SUSAN J. KLEINBERG. (Studies in the Life of Women.) Reprinted from the Arena Press edition of 1895. New York: Schocken Books. 1975. Pp. xv, 387. Cloth \$7.50, paper \$3.95.

ELIZABETH EVANS. *Weathering the Storm: Women of the American Revolution*. New York: Charles Scribner's Sons. 1975. Pp. 372. \$12.50.

MARY P. RYAN. *Womanhood in America: From Colonial Times to the Present*. New York: New Viewpoints. 1975. Pp. 496. \$15.00.

While historians of the United States have become increasingly conscious of the need to integrate the history of women into their teaching curriculum, the lack of publications appropriate for classroom use has hampered such integration. These three books offer the history teacher a means for transcending this pedagogic barrier. Ryan's text covers the entire sweep of the history of American women, Bloomer's writings focus on the pivotal shift to overt feminism in the mid-nineteenth century, and Evans' anthology of diaries and journals from the Revolution makes heretofore obscure primary sources easily accessible.

Mary Ryan's *Womanhood in America* is a concise and scholarly text that should remain authoritative for many years to come. Its merits are those characteristic of all good history and few textbooks—patience with the complexities of continuity and change, effective presentation of primary evidence, and a persuasive interpretation of secular trends. Necessarily highly selective, the book emphasizes three central themes of female experience: family, work, and feminism. The first four of its seven lengthy chapters consider pre-twentieth-century developments: "Women in Agrarian Society," primarily before 1750; "Women in Commercial America," primarily from 1750 to 1830; "The Common Woman, 1830-1860"; and "Work-

ers, Immigrants, Social Housekeepers: Women and the Industrial Machine, 1860-1920." The last three chapters focus on more recent history: "The Sexy Saleslady: Psychology and Consumption in the Twentieth Century"; "A Kaleidoscope of Roles: Twentieth-Century Women at Work and in the Home"; and "Current Snares of Sexism: Inequality in Any Class."

Ryan's footnotes reflect recent social history as well as rich primary material, and her index is a guide to the main issues and contours of the history of American women. Aware of the importance of class, ethnicity, race, and region, she is careful to draw distinctions among different groups of women, but also clarifies the historical circumstances women have shared because of their gender. The demographic, social, and intellectual conditions of motherhood among various groups of women and the economic circumstances shaping their patterns of work receive special attention as do the changes of consciousness that promote feminism. Ryan's discussions of changes in sexual behavior, women's legal status, and female stereotyping make her book as comprehensive as it is insightful.

In a more specialized vein, but equally valuable for classroom use, is D. C. Bloomer's *Life and Writings of Amelia Bloomer*, with an interpretive and scholarly introduction by Susan J. Kleinberg. Bloomer (1818-1894) both reflected and symbolized the generation of mid-nineteenth-century, middle-class women whose participation in social reform drew them into the assertion of autonomous female politics and activism. Marrying an anti-slavery reformer and Whig editor of the *Seneca County Courier* in 1840 (the word "obey" being omitted from the bride's vows), Amelia Jenks Bloomer settled in Seneca Falls, where in 1848 she attended Elizabeth Cady Stanton's famous convention on women's rights and in 1849 founded, with other Seneca Falls women, a "little temperance paper," the *Lily*. Gradually assuming editorial leadership, Bloomer advocated women's rights as well as temperance. In her writings, students will discover the ferment of the early women's movement as well as its connections with other reform movements.

Dress reform and the *Lily's* advocacy of Turkish pantaloons was but one aspect of the early feminist effort to free women from the rigid conventions surrounding their behavior. Eventually dropped because Bloomer, Stanton, and others concluded that "dress was drawing attention from what we thought of far greater importance," dress reform gave way to questions of "woman's right to better education, a wider field of employment, to better remuneration for her labor, and to the ballot for the protection of her rights." The *Lily* attained a

national circulation of over 6,000 by 1853, but it ceased publication in 1856 after the Bloomers moved west to Council Bluffs, Iowa. In 1869 Amelia Bloomer represented Iowa at the meeting of the American Equal Rights Association in New York City, and in 1871 became president of the Iowa Woman Suffrage Society. An effective public speaker, writer, and editor of one of the first women's rights newspapers, Mrs. Bloomer epitomizes the westward movement of women's rights as well as its Seneca Falls origins.

While clearly conceived to celebrate the Bicentennial and collected by a journalist rather than a historian, the eleven diaries and journals contained in *Weathering the Storm* constitute a valuable classroom tool. Elizabeth Evans has provided a descriptive introduction to each journal that places it in its immediate historical context. They range from the flirtatious Sarah Wister, who describes her youthful attraction to soldiers garrisoned near her Pennsylvania home, to the avid patriotism of Deborah Sampson Gannett, a former indentured servant who dressed as a man and fought for three years in the Fourth Massachusetts Regiment; from Grace Growdon Galloway, who records her determined but unsuccessful fight to retain her property when that of her loyalist husband was confiscated, to Margaret Hill Morris, a young Philadelphia widow, Quaker, and neutralist, who reports on the battle of Trenton and the difficulties of raising four small children in wartime. The accounts of these and other women in the Revolutionary era make their war experiences vivid and meaningful for today's students.

KATHRYN KISH SKLAR
Fellow, NHI, Yale

GEORGE M. WOYTANOWITZ. *University Extension: The Early Years in the United States, 1885-1915*. (Series on Continuing Education, number 2.) Iowa City: National University Extension Association and the American College Testing Program. 1974. Pp. xi, 171. \$3.00.

AMERICO D. LAPATI. *Education and the Federal Government: A Historical Record*. New York: Mason/Charter. 1975. Pp. v, 388. \$15.00.

"Soap and education are not as sudden as a massacre," wrote Mark Twain at the turn of the century, "but they are more deadly in the long run." The quests for health and learning are inherent elements in the American story, the latter of which is reflected in the material contained in these two books. Both are commendable additions to the bibliography of the history of American education.

In a well-written, well-documented monograph George M. Woytanowitz traces the first thirty years of university extension programs in the

United States. Beginning with the background of English university extension, he then proceeds to discuss, in turn, the background of American university extension (the lyceum, Chautauqua, and Johns Hopkins University); extension in the 1880s (noting that "Herbert Baxter Adams, professor of history at Hopkins, was the first to push the idea of university extension in America," [p. 24]); the founding and activity of the American Society for the Extension of University Teaching and the first national conference on university extension; extension in the West through the efforts of the Universities of Chicago, Kansas, and Wisconsin; the "seven fat years: 1892-1899"; the impact of university extension on American education; the relationship of university extension to the urban crisis; and the decline and reorientation of extension between 1900 and 1915.

The author provides a careful assessment of reasons why in the late nineteenth century "of all the educational innovations, university extension was the least successful in winning acceptance from either educational leaders or the interested public" (p. x). He shows how after 1900 university extension underwent a transition, becoming "more entertaining than educational."

Although *University Extension* includes an extensive, though selected bibliography, it unfortunately contains no index, a "must" for a study wherein many names of individuals, institutions, and organizations are mentioned.

In *Education and the Federal Government* Americo D. Lapati provides a valuable reference work. Divided into two parts, "Congress and Education" and "The Supreme Court and Education," this volume begins with a brief history of the Office of Education. In chapters two through five the author discusses seriatim the many acts and programs related to education enacted by Congress, beginning with the Ordinances of 1785 and 1787. For example, in the chapter on higher education, Lapati takes up plans for a national university, the service academies, Howard University, the Morrill Acts, the National Youth Administration, the G.I. Bill of Rights, the Higher Education Facilities Act of 1963, and prohibition of sex discrimination (Title IX). Other chapters in Part I relate to federal legislation affecting elementary and secondary education, vocational education, rehabilitation, and the handicapped, as well as international education.

Part II begins with a chapter devoted to court decisions concerning the financing and control of public education. The remaining chapters deal with decisions of the Supreme Court regarding private and religious schools; religious teaching and activities in the public schools; teachers (for example, contracts of employment and tenure, loy-

alty oaths, and freedom of expression and association); students (for example, secret societies in schools, conscience—military training and compulsory flag saluting—and corporal punishment); and integration.

The author provides ample documentation and a selected bibliography for this important facet of American history, and he presents the material in clear, concise prose that deserves approbation.

WILLIAM LLOYD FOX
Montgomery College

CLAY MCSHANE. *Technology and Reform: Street Railways and the Growth of Milwaukee, 1887-1900*. Madison: the Society Press of the State Historical Society of Wisconsin, for the Department of History, University of Wisconsin. 1975. Pp. ix, 187. \$5.95.

During the last decade of the nineteenth century electric street railways were replacing horse-car routes in hundreds of American cities. Larger cars, faster service, and extended routes to suburban areas all resulted from the change in urban transportation first introduced by Charles Van Depoele and Frank Sprague. Clay McShane's study carefully reviews the experience of Milwaukee as that city adopted and sought to regulate this important new technology.

The economy of Milwaukee late in the nineteenth century was shifting from a major dependency upon flour milling toward an industrial mix of breweries, meatpacking plants, and factories. The new plants, several of which were located outside the city limits, employed much of the large German- and Polish-born population of the city. Growing from a population of 115,000 in 1880 to 285,000 in 1900, Milwaukee was ready for innovation in urban transportation. Henry Clay Payne, Milwaukee businessman and a prominent Wisconsin Republican politician, played a vital role in merging several horse-car lines into a consolidated street railway system. Possessing both a favorable municipal franchise and financial backing from Henry Villard of the Northern Pacific, Payne was able by 1893 to fully electrify his new Milwaukee Street Railway Company. The depression of 1893 ended the real estate boom in Milwaukee, and a decline in trolley traffic forced the traction company into bankruptcy. Payne managed successfully to reorganize his company and was also victorious over civic reformers in disputes concerning taxes, lower fares, and route extensions. His triumph over the reformers was complete with the enactment of a new franchise approved in 1900.

The author reviews early electric traction in Milwaukee with clarity and a full appreciation for the

varied economic and political forces involved. Perhaps his introduction to the subject is too extensive, since four of the eight chapters cover mainly background material. *Technology and Reform* is a valuable addition, however, to the growing literature on electric traction in urban America.

JOHN F. STOVER
Purdue University

JOSEF J. BARTON. *Peasants and Strangers: Italians, Rumanians, and Slovaks in an American City, 1890-1950*. (Harvard Studies in Urban History.) Cambridge, Mass.: Harvard University Press. 1975. Pp. xii, 217. \$12.00.

A regrettable cleavage has developed for about a decade now between two groups of American social historians who ought to be more compatible, those of the "new urban" history and immigration specialists. The division has persisted not only because of topic but also because of their differing emphases on methodology. Urbanists have heralded quantification, while most "ethnicists" have stressed the more traditional but heretofore neglected group materials. Owing to the difficulty in mastering both techniques, few substantial studies, with the possible exception of demographic or political histories such as Frederick Luebke's work on Nebraska Germans, have satisfactorily integrated both the newer and older methods. Josef Barton, more advantageously than in any previous work, employed both and thereby has contributed to the restoration of a necessary dialogue. This lucid account of the social determinants of Italian, Slovak, and Rumanian ethnic adjustment in an American city has established a new standard of scholarship for historians of urban and ethnic life. In its methodological synthesis, the work inaugurates a new generation of younger ethnic historians.

Barton demonstrates an additional talent in employing still another new approach, the comparative, cross-cultural technique. Immigration and ethnic historians are well aware of the research difficulties in studying individual groups other than their own, language being only one of several handicaps. While Barton had the help of the rich immigrant collection at the University of Minnesota, he shows a remarkable individual sophistication in comparing three dissimilar ethnic groups, especially at their place of origin.

His discussion of emigration conditions in Eastern Southern Europe offers no surprises to the knowledgeable immigration specialist. The motivations, social structure, and traveling patterns of the departing waves, for economic and status reasons via pioneer, chain, and family migration, are

familiar to readers of Balch, MacDonald, Banfield, Vecoli, and others. Nowhere to this reviewer's knowledge have these insights been comparatively synthesized and, more important, linked to the groups' social mobility in America. One revealing and provocative conclusion, for example, is the close correlation between the immigrant father's mobility and that of his child; variables of education or family size are less significant.

It is in relating his place and subjects to a larger ethnic and urban framework that Barton is weakest. While effectively employing the several methodologies, his work also suffers from their defects. He focuses excessively, for example, on one city—Cleveland—with the implied assumption that it is a typical laboratory for late nineteenth-century ethnic adjustment. The reader moreover gains an insufficient sense of the relationship of Cleveland's Slovaks, Rumanians, and Italians to their American communities in general. Ethnic historians would seriously question Barton's suggestion that his three groups shed much light on other "similar" peoples—Poles, Hungarians, and Greeks. The latter are simply too (though not entirely) dissimilar in composition.

These limitations, in dealing inadequately with broader segments of America's past, are as much a weakness of the urban and ethnic fields as of Barton's work per se. We will have to await later studies by him and his colleagues. He has produced a work of distinguished scholarship, and this reviewer hopes he eschews his intended next study and remains essentially an Americanist.

VICTOR GREENE
National Humanities Institute

STUART GALISHOFF. *Safeguarding the Public Health: Newark, 1895-1918*. Westport, Conn.: Greenwood Press. 1975. Pp. xv, 191. \$13.50.

This narrative account of the development of public health in Newark is derived from a variety of sources whose unearthing and mastery were no mean feats. In addition to the few pertinent manuscript sources, the author has based his work primarily on newspapers and ephemeral reports and notices of relatively obscure public, quasi-public, and private agencies.

Beginning with an introduction, "The Nation's Unhealthiest City," as the census dubbed Newark in 1890, the author traces the development of the Newark Board of Health and the bacteriology laboratory and the problems and control of the water supply, of sewage, and of mosquitoes. He then describes the movement for "Milk Reform," in which Henry L. Coit and his introduction of certified milk gave Newark a prominence not matched

by the city's actual accomplishment. Galishoff also covers the campaigns against infant mortality, tuberculosis, and industrial disease, and the problem of tenement housing. Pertinent morbidity and mortality data and other statistics are included.

Although Newark's accomplishments, especially with regard to infant mortality and tuberculosis, were considerable, the work claims no extraordinary feats for the city. The author rightly warns against generalizing on the basis of Newark's experience, but he does offer a not atypical case history of the political, economic, social, and personal forces that sometimes fostered, sometimes hindered, progress in public health.

The time limit set by the author is artificial, but it does reflect an attempt to associate the public health improvements with the Progressive era. Only in the conclusion of the book is there an attempt to make this connection. Although the author may be correct in his contention that in seizing upon the "revolutionary discoveries made in sanitary engineering and medical science, the Progressives effected a profound reduction in morbidity and mortality," his study does not effectively prove it. It is difficult to think of engineers and physicians—medical societies opposed the distribution of diphtheria antitoxin by public dispensaries—as Progressive reformers. The humanitarian instincts that motivated physicians like Henry L. Coit and Julius Levy (who was largely responsible for a successful attack on the high infant-mortality rate) may not reflect reformist social or political attitudes. In any event, the author gives no indication that either was in any way aware of or involved in Progressivism. There are similar difficulties with the many voluntary organizations that forced the problems of public health out of the private and into the public sector: nothing in the book demonstrates a relationship between their social consciousness and humanitarianism on the one hand and progressive social and political philosophy on the other. Only the newspapers, especially in their campaigns for pure milk, clearly fell into the muckraking spirit of the Progressive era. The public health movement may have reflected or benefited from the Progressive movement, but the author has not come to grips with the question.

An occasional error in footnoting is discernible—a wrong date, a wrong page, a misplaced attribution, an entirely wrong attribution—but the documentation is thorough. At only one point was significant documentation lacking: the author refers to the patronizing of immigrants by physicians and observes that "Levy was proposing to teach 'dumb' immigrants how to guard their infants against serious disease" (p. 112). It would be sig-

nificant to know whether Levy, a dedicated humanitarian, actually used the epithet.

DAVID L. COWEN
Rutgers University

WILLARD B. GATEWOOD, JR. *Black Americans and the White Man's Burden, 1898-1903*. (Blacks in the New World.) Urbana: University of Illinois Press. 1975. Pp. xi, 352. \$12.95.

Several studies of various aspects of American imperialism at the turn of the twentieth century have appeared recently, but this admirable volume covers an aspect of the issue insufficiently developed in other works. The book's considerable strength lies in its analysis of articles in black newspapers, letters of black soldiers, and other materials of the period to show how blacks dealt with the issue of imperialist expansion. The author probes the basic dilemma facing blacks as they confronted this issue and argues convincingly that they viewed it differently from white citizens. As members of a racial minority all too often subjected to discrimination and violence in their own country, Negro Americans quickly identified with the struggle for national independence by the oppressed populations of Hawaii, Cuba, and the Philippines. Yet they had to take care lest their support be used to advance the purposes of those who wished to annex these territories as part of the new American empire. To be sure, there were black Americans who favored such acquisition, notably Edward E. Cooper, editor of the *Colored American*, a weekly published in Washington, D.C. But as Gatewood demonstrates, blacks favorable to acquisition were a distinct minority, and, moreover, were linked to Republican policies through political appointments.

Black participation in the war against Spain was favored by a majority of the black press on the ground that it would improve the status of the Negro people in the United States. But as Gatewood makes clear, it had the opposite effect and was followed by an intensification of racist violence. It is perhaps unfortunate that this discussion of the relationship between imperialism and racism does not include recognition of the belated understanding on the part of important white anti-imperialists that imperialism had to be combatted precisely because it was intensifying racism in this country.

Those already familiar with Gatewood's collection of papers on black American soldiers in the war against Spain and his volume of letters by Negro soldiers covering the years 1898-1902 will not be surprised by his perceptive use of sources and felicitous writing. In the present work, he has

fulfilled the expectations aroused by his earlier writings. He has produced a scholarly and readable book for which all who work in the era of the "nadir" of black American history will be grateful.

PHILIP S. FONER
Lincoln University

JULES A. KARLIN. *Joseph M. Dixon of Montana. Part 1, Senator and Bull Moose Manager, 1867-1917; Part 2, Governor Versus the Anaconda, 1917-1934.* Missoula: University of Montana. 1974. Pp. xiii, 257; xvii, 269.

For a sparsely populated state Montana has produced a surprising number of nationally prominent political figures: Jeannette Rankin, Thomas J. Walsh, Burton K. Wheeler, James E. Murray, and Mike Mansfield. Jules A. Karlin, professor of history at the University of Montana, believes Joseph M. Dixon belongs on the list. Born in North Carolina and reared by Quaker parents, Dixon was twenty-four years old when he arrived in Missoula in 1891. After admission to the bar, Dixon embarked upon a long political career, successively as Missoula County Attorney, state legislator, congressman, senator, governor, and Assistant Secretary of the Interior in Herbert Hoover's administration. During his first years in Washington (1903-1913) he became a friend of Theodore Roosevelt and espoused some politically reformist ideas.

These volumes, the product of more than twenty years of effort, are based on Dixon's papers which the author supports with an impressive list of other manuscript sources, interviews, and about 150 newspapers. To conserve space, he used few footnotes, so anyone wishing to use these voluminous sources will have to turn to the original manuscripts in the University of Montana archives.

Although he managed Theodore Roosevelt's 1912 preconvention campaign for the presidential nomination and was later chairman of the Progressive party's national committee, Dixon was a minor figure in American political history. He was a more important force in the history of Montana. These volumes will be of interest to local and regional historians and to students of state and local politics. Their chief value lies in the massive amount of details Karlin has assembled and the leads they will provide to future local historians. A microscopic study of a career such as Dixon's might have shed light on our understanding of progressivism by testing some of the ideas historians have propounded in recent decades. Unfortunately, Karlin's books are so badly lacking in conceptualization they confuse rather than clarify. The reader is not even clear what kind of progressive Joseph M. Dixon was. The only clear point is that in Karlin's mind Dixon was just about the

only public figure in Montana who supported progressive reforms, at least honestly so, and all who were not pro-Dixon were controlled by Anaconda, Montana Power, and their other corporate allies. The closest the reader gets to any sort of analysis is to be told over and over again that Dixon (whom the author compares to David) was pitted in what was ultimately a losing struggle against the Goliath of Anaconda's corporate power.

Karlin's prose style is annoying. The volumes abound with archaisms—"erstwhile" is worked to death. A confusing array of minor characters are identified by meaningless parenthetical descriptions which do not establish their importance to the story. But worst of all is the author's pomposity: "Dixon's inhalations of the election-scented air of 1914 did not affect him to the same degree as previous exposures to similar aromas" (Part I, p. 204).

Withal, these paperbacks are physically ugly. The first volume in particular is marred by many typographical errors. It also broke apart on its first use. The second is held together by huge staples, which means that it cannot be laid open, and holding it is like clutching a coiled spring. The uneven right-hand margins help create the impression of a badly typed thesis.

Finally, the books will fail in their main purpose. Karlin's uncritical hero worship will not rescue Dixon from obscurity.

RICHARD B. ROEDER
Montana State University

JAMES B. LANE. *Jacob A. Riis and the American City.* (National University Publications, Interdisciplinary Urban Studies.) Port Washington, N.Y.: Kennikat Press. 1974. Pp. xi, 267. \$12.50.

ALLEN F. DAVIS and MARK H. HALLER, editors. *The Peoples of Philadelphia: A History of Ethnic Groups and Lower-Class Life, 1790-1940.* Philadelphia: Temple University Press. 1973. Pp. ix, 301. Cloth \$9.95, paper \$3.25.

FRANCIS RUSSELL. *A City in Terror: 1919—The Boston Police Strike.* New York: Viking Press. 1975. Pp. v, 256. \$10.00.

James B. Lane's *Jacob A. Riis and the American City* is a scholarly biography of Riis and his role as a reformer. The book is an outgrowth of a doctoral thesis, and unfortunately it reads too much like one. Yet Lane has carefully researched his subject and has written a thorough study.

Lane concentrates on Riis' attitudes toward the city, but he also devotes a considerable amount of time to the relationship between Riis and Theodore Roosevelt. The Riis that emerges in Lane's account is a middle-class Progressive. Although

Riis had an optimistic view of the world and was concerned about poverty, immigrants, and cities, he shared many of the Progressive attitudes about poverty and reform. He also worshipped Roosevelt and was uncritical about American foreign policy; the immigrant Riis became an American patriot. Lane is aware of the limitations of Progressivism, but nonetheless believes that Riis played an important part in alerting the American public to the problems of the city and the poor. There is no doubt that he did; Riis was one of the first to catch the national eye about these problems and he was an important minor figure of Progressivism. In spite of his relationship to Roosevelt, Riis appears to have been less influential after 1900. The student of reform will not learn much new about Progressivism, but rather will gain an understanding of one reformer.

Most of the essays in *The Peoples of Philadelphia: A History of Ethnic Groups and Lower-Class Life, 1790-1940*, are original. They are uneven in quality, but on the whole make rewarding reading for the student of urban and ethnic history.

Bruce Laurie's excellent article on fire companies and gangs demonstrates the use of material on fire companies in studying ethnic and working-class life. Hershberg also has some interesting things to say about the impact of urbanization on black life. The articles by Dennis Clark on the Irish, Caroline Golab on the Poles, Italians, and Jews, and Richard Varbero on the Italians are informative and offer the possibility of comparisons between Philadelphia's ethnic minorities and those of other cities.

Mark Haller suggests in the conclusion that the two overriding themes of these essays are the relationship of social change to crime and violence in the city and the interaction between the values of newcomers and the institutions of the city. Actually the essays do not dwell much on crime and violence, except for the mid-nineteenth century. This reviewer did not always find the issue of adjustment of values to the city pronounced in the essays. One should not be disturbed by a lack of unity in the articles; for, as the editors note, we know so little about lower-class and ethnic life in Philadelphia that the volume is useful as a beginning. The reviewer hopes that these scholars and others will continue their probing of Philadelphia.

Unlike the authors cited above, Francis Russell has written a popular history. *A City in Terror* is a compelling and lively book about the Boston Police Strike of 1919. He omits footnotes and has only a limited bibliography. For example, under Public Sources and Official Reports he ends his sketchy list by noting the "annual reports of the police commissioner, Harvard class reports, and so forth."

A City in Terror has plenty of drama and heroes and villains. Russell makes clear that he detests Calvin Coolidge, a judgment hard to quarrel with, and he especially dislikes Andrew Peters, the Boston Police Commissioner during the strike. He blames Peters for the strike, because he had limited abilities and was rigid and unsympathetic in his dealings with the police. There are violent mobs and also a multitude of students and blundering citizens who wanted to play policeman in the strike. Russell is sympathetic to the plight of the police, although one gets the impression that because of the violence he does not condone their actions. Russell is also aware of the general forces contributing to the strike: the rising cost of living, labor unrest, and the grievances of the police. Because he concentrates on drama and personalities, one does not find a penetrating analysis of why the violence erupted in Boston.

Russell is at home in the history of the era and in Massachusetts, and he tells his story well; *A City in Terror* makes stimulating reading. One problem, however, is the light touch which begins the book, a personal, warm discussion of his father during the strike. This light touch did not seem consistent with the title of the book and much of the discussion of the violence accompanying the strike. One of Russell's conclusions may be dated. He says one of the legacies of the strike was that no American city for 50 years was threatened by a walkout of its police. With the increasing unionization of municipal employees that legacy may be ending. We now have the possibility of more police strikes, and indeed, have had some recently.

DAVID M. REIMERS
New York University

ROBERT W. LARSON. *New Mexico Populism: A Study of Radical Protest in a Western Territory*. With a foreword by HOWARD R. LAMAR. Boulder: Colorado Associated University Press. 1974. Pp. xiv, 240. \$10.00.

Long a favorite subject of historians, Populism has recently been the focus of a number of important studies. Historians like Nugent, Parsons, Brodhead, Clanton, Hackney, and Kouser have sharpened our understanding of the Populists, adding new dimensions and broadening the movement well beyond its original Plains-States boundaries. Wright, Bicha, and others have looked productively at Rocky Mountain Populism, especially the Colorado variety, and now Robert W. Larson has given us a richly detailed account of Populism in New Mexico.

Drawing on careful research in local newspapers and other neglected sources, Larson traces the development of the Populist movement through

the decade of the 1890s. As his analysis makes plain, the political phenomenon of Populism accomplished little in New Mexico. Populists won few elections, attracted relatively few voters, and passed virtually no legislation. As elsewhere, in its later years New Mexico Populism succumbed increasingly to the convention and electoral maneuvering. But it did embrace a wide variety of New Mexicans, far wider than previously suspected, and it had some influence on territorial development.

New Mexico Populism was a thing of extraordinary complexity; it varied from county to county, in some cases from town to town. In Lincoln and Colfax counties it reflected local protest against encroaching land and cattle monopolies. In Albuquerque merchants found it a useful means to seek better railroad rates and party reforms, while in San Miguel County antimonopoly sentiment took a special and intriguing twist, with the formation of *Las Gorras Blancas*, the White Caps, a group of Hispanic settlers who rode at night to defend their communal land holdings. Variety and localism somehow dwindled in mid-decade, to be replaced by depression, silver, and more "national" concerns. By 1898, less than ten years after its birth, Populism in New Mexico was dead.

Larson's study, with its wealth of local detail, is a welcome addition to Populist historiography. The attempts of the 1950s to provide a broad analysis of Populism left a good deal to be desired. Thanks to Larson and others, it may be time for a historian of vision to try again.

R. HAL WILLIAMS
Southern Methodist University

ROBERT MILTON BURTS. *Richard Irvine Manning and the Progressive Movement in South Carolina*. Columbia: University of South Carolina Press. 1974. Pp. viii, 259. \$9.95.

Scion of landed gentry, Richard Irvine Manning counted among his ancestors at least four governors of South Carolina as well as prominent military figures from the Revolution through the Civil War. A successful lawyer and businessman, he entered politics, at first supporting the Bourbons against Benjamin R. Tillman and later opposing the forces of Coleman L. Blease. In 1914, after service both in the state legislature and on various Democratic party committees, Manning, with the tacit approval of Tillman, was elected governor over the Blease candidate.

The major emphasis of this volume is on the reforms initiated either by Manning as governor or by the legislature, usually with his approval. His four-year record, 1915-19, included increased appropriations for the public schools and a new com-

pulsory attendance law; a revision of the tax structure, which Manning felt was his most important accomplishment; the development of agricultural markets and an extension of agricultural education; labor legislation, which included a child labor law and a board of conciliation created to solve labor disputes; political reforms, which tightened election procedures and introduced the secret ballot in Democratic primaries for all but the smallest rural areas. Manning, the aristocrat, was able to establish a progressive record in line with other contemporary administrations in the South. He saw to it that South Carolina participated fully in World War I. Besides urging patriotism and sacrifice and fighting the antiwar activities of the Bleasites, he was able to secure army camps at Columbia (now Fort Jackson), Greenville, and Spartanburg. The naval and marine installations around Charleston were enlarged. Although these military establishments may have created problems of vice, lawlessness, and race relations in the camp areas, they "were a great boon" to the state's economy (p. 159), which perhaps was why South Carolinians eagerly sought them.

Other than noting an occasional editorial lapse, the only criticism of this volume might be that it needs a bit less of Manning and a much more extensive account of what constituted progressivism in South Carolina and of how that state's reform movement was identical to, or different from, that of her sister states. Nevertheless, the work adds a needed and interesting chapter to the historiography of the Palmetto State.

THOMAS H. COODE
California State College,
Pennsylvania

JAMES WHORTON. *Before Silent Spring: Pesticides and Public Health in Pre-DDT America*. Princeton: Princeton University Press. 1975. Pp. xv, 288. \$12.50.

Insecticidal contamination of the environment is not a new problem. Whorton's volume chronicles that issue from the development of chemical pesticides in the 1860s to the DDT era. Insecticides such as Paris green were introduced early in this period to assist farmers with a recently intensified insect threat. These chemicals were first used at a point of great need and long before any awareness of the environmental dangers posed by them.

The first significant federal legislation applicable to the health problem raised by insecticide use was the 1906 Pure Food and Drug Act. This legislation defined illegal adulteration as the presence of poison or deleterious ingredients which rendered an article injurious to health. The federal regulatory problem, henceforth, was to detect such

substances, define danger levels, and make these decisions stick legally. New food and drug legislation was enacted in 1938 after five years of legislative efforts, a struggle which both opened and closed with the issue of pesticide residue on fruits. This act strengthened federal controls but in no sense solved the pesticide problem. Indeed new insecticides, principally DDT in the post-World War II years, would prove more dangerous than previously popular lead arsenate, because they were more destructive to the environment in which they were used.

While actions by states have been slighted, this volume is an excellent one. It offers a balanced, sophisticated treatment of a problem which has often been subjected to oversimplification and unproductive, passionate indictments. Moreover, if most consumers are aware of charges that their foods are tainted with pesticide residue, they may be surprised to read that their grandparents faced the same hazards. The historical perspective should provide a valuable corrective for a popular inclination to see this issue solely in contemporary terms.

CHARLES O. JACKSON
University of Tennessee

MARK H. ELOVITZ. *A Century of Jewish Life in Dixie: The Birmingham Experience*. University: University of Alabama Press. 1974. Pp. viii, 258. \$10.00.

It is true, as Jacob R. Marcus suggests in the foreword, that American Jewish historiography is still embryonic and Southern Jewish history even more so. Marcus is undoubtedly justified in claiming that Mark H. Elovitz's volume on Birmingham, Alabama, ranks as "the first substantial history of the Jews in any inland town or city of the industrial South." We have histories, all either fragmentary or otherwise inadequate, of Southern Jewish communities like Charleston, South Carolina, Nashville, Tennessee, and New Orleans, but Elovitz is the first to attempt an account of Jewish communal development in a new, that is, post-bellum, city like Birmingham.

In many respects, his work is admirable. He brings the history of Birmingham Jewry up to the present, offers his readers a description of the changing socioeconomic and religio-cultural scene, does not avoid divisive factors or controversial issues such as Jewish attitudes toward the desegregation conflict of the 1960s, documents his study well, and supplies some useful appendices. He is aware, too, that Birmingham Jews have not lived in a vacuum and is at some pains to connect the city's Jewish experience with the experience of the larger non-Jewish community. He also documents the ethnic bifurcation of Birmingham

Jewry: from the earlier Central European settlers came the important merchants and investment bankers of the Phoenix Club and Highland Avenue; the East Europeans who arrived a few decades later created on Birmingham's north-side an area "so Jewish that 'you could tell and smell Friday night because everyone was dressed up [for the Sabbath] and what was cooking pervaded that several block area.'" A half-century passed before the two groups fused. Birmingham lags behind in this respect, but the Jews do not lag behind where mercantile activity is concerned: as recently as the mid-1960s six of Birmingham's top seven general merchandising firms were Jewish-owned. Still, despite their prominence in commercial and civic life, the Jews have not been at one with their non-Jewish neighbors. According to one of Elovitz's confidential sources, the Jewish community leadership views the desegregation fight as a "Christian problem" between whites and Negroes and "not simply a racial problem."

Elovitz describes, or perhaps one ought to say chronicles, the Birmingham Jewish experience. He appears, however, to work without any conceptual framework. The reader knows something of what has happened in Birmingham; he is told virtually nothing as to why it happened. His book is heavy with data (for which future historians will surely be grateful). But what these data mean, what they tell us historiographically, is apparently no concern of the author's. That diminishes the value of his work.

STANLEY F. CHYET
Hebrew Union College

RALPH ELDIN MINGER. *William Howard Taft and the United States Foreign Policy: The Apprenticeship Years, 1900-1908*. Urbana: University of Illinois Press. 1975. Pp. xii, 241. \$9.50.

William Howard Taft had a significant role in United States foreign policy even prior to his election to the presidency. He headed the Taft Commission to the Philippines in 1900 and then served as governor there. After becoming Secretary of War in the cabinet of Theodore Roosevelt, he had Panamanian affairs under his jurisdiction. Roosevelt also dispatched him to Cuba to deal with political unrest there in 1906. Meanwhile, he had undertaken the first of two trips to the Far East. In 1905 he visited Japan, the Philippines, and China. Two years later he visited the same countries and proceeded around the world via Russia.

In recounting these events Minger draws primarily upon the papers of Taft, McKinley, and Roosevelt. The story from this limited range of sources is already known to many readers. Most of the chapters of this book were published as

articles in the early 1960s. They are now republished with little revision, and therefore do not come to grips with interpretations of scholars who have published in the last decade and a half. There are no references to the studies made by Richard D. Challener, Michael H. Hunt, Akira Iriye, Allan R. Millett, Charles E. Neu, and Peter W. Stanley. The book also has serious editorial deficiencies, such as the material on pages 7-9 being repeated verbatim on pages 183-185. Minger's study leaves much to be done on Taft's role in foreign policy in the period 1900-1908.

RAYMOND A. ESTHUS
Tulane University

JOHN DEWITT MCKEE. *William Allen White: Maverick on Main Street*. (Contributions in American Studies, number 17.) Westport, Conn.: Greenwood Press. 1975. Pp. x, 264. \$12.95.

John McKee has written a brief, engaging biography of William Allen White (1868-1944), the small-town Kansas editor and progressive reformer who became one of the esteemed figures in American journalism during the early twentieth century. The book has several distinct virtues. It is readable; it reflects careful research in White's published and unpublished materials; and it seeks to interpret as well as to describe White's fascinating career.

A recurring theme is that White sought continually (and with considerable success) to make his "private sentiment public opinion."¹ McKee stresses that White's maverick qualities always rested on a solid core of beliefs, including the assumption that conduct was proper insofar as it reaffirmed the middle-class respectability of village America. Such a view allowed him to rebuke Populist malcontents who seemingly challenged that world and, subsequently, to cheer for Main Street progressivism that sprang from the right kind of people. Predictably, his great hero was Theodore Roosevelt, who exuded respectability while reaffirming the righteous individual's impact on society. To the end, White kept his faith in a moralistic universe, full of promise, imbued with Christian ethics, and responsive to the demands of justice and responsibility. As McKee argues, White was "one of the best lay preachers ever to use the editorial page as a pulpit" (p. 27).

Although McKee's narrative is interesting, it should not surprise historians long familiar with White's masterful account of his own life. *The Autobiography of William Allen White*—which contributed substantially to White's historical significance—confronts his biographers with the unenviable task of supplying more insights into the man than he himself so gracefully provided. There

are ways around the problem, however, as Robert Bannister demonstrated in his excellent study of Ray Stannard Baker, another progressive journalist who, like White, wrote a major autobiography. McKee could have drawn considerably more from secondary materials and related manuscript collections; he could also have been more attentive to historiographical issues (for example concerning the nature of progressivism and its relationship to the twenties and thirties). By overlooking many relevant secondary sources, and by generally skirting historiographical matters, McKee missed a chance to write a more substantial book.

LEROY ASHBY
Washington State University

FLORETTE HENRI. *Black Migration: Movement North, 1900-1920*. Garden City, N. Y.: Anchor Press/Doubleday. 1975. Pp. xi, 419. \$9.95.

THOMAS SOWELL. *Race and Economics*. New York: David McKay. 1975. Pp. ix, 276. Cloth \$9.95, paper \$4.95.

Black Migration: Movement North, 1900-1920 delivers both more and less than its title suggests. It is not the careful, detailed examination of the migration process and its consequences—especially of the Great Migration—that scholars so badly need. It is a survey of race relations and conditions of black life, urban and rural, North and South, in the first two decades of this century. "In 1900," Florette Henri observes, "black people were practically invisible on the American scene; by 1920 they were visible, audible presences everywhere in the national life. How and why did it happen?" (p. vii). These questions are answered competently and intelligently on the basis of a fairly thorough survey of existing scholarship and published sources, although there are curious omissions of important, recent monographs. The only original research in unpublished sources relates to the situation of blacks in the military, a subject on which Henri has previously published. The book will be useful to the student seeking a general overview, but it will offer no special surprises or new insights to the scholar.

Thomas Sowell takes one of the themes in Henri's narrative—economic opportunity and achievement—and examines how it has been shaped historically by race and ethnicity. Studying the economic development not only of blacks but also of Jews, Irish, Italians, Japanese-Americans, West Indians, Puerto Ricans, and Mexican-Americans, Sowell investigates the impact on the socioeconomic position of the various groups of such factors as "color, language, nationality" (p. 60), religion, "time of arrival in America," and "the

time of movement . . . into a modern, industrial, and commercial economy . . ." (pp. 34-35). He focuses especially on the relationship between discrimination and economic progress, and he argues provocatively, for example, that economic advancement has had little to do with tolerance; that "the lessening of group animosity has not been a precondition for economic advancement, and rapid economic advancement has often been accompanied by increased group animosities" (p. 162); that "the current disabilities of black Americans are due not only to current discrimination but also to past deprivation and disorganization that continue to take their toll" (p. 102); that "low-income origins, overcrowded and substandard housing, prejudice and discrimination, inadequate educational opportunities, and a general failure of public services . . . to do their jobs properly in minority neighborhoods" all "impeded the progress of all American minorities; but it is by no means clear that the more successful minorities had any less of such handicaps than the less successful minorities" (p. 143). Sowell concludes that "how long a group's culture has been an urban, industrial, and commercial one is a key factor in its success in urban, industrial, and commercial America" (p. 149); that "current income differences between ethnic groups" can be explained in part by the difference in "the average age of the respective groups" (p. 150); that "a general standardization of jobs, wages, and promotions patterns has also made it harder for contemporary disadvantaged minorities to advance than for their nineteenth-century counterparts" (p. 154). Sowell holds, therefore, that those who castigate blacks and Puerto Ricans for failing to pull themselves up by their own bootstraps in the manner of Irish or Jewish immigrants of the last century are ignoring a whole complex of cultural and economic factors that need to be understood in historical perspective. Florette Henri offers such a perspective.

NANCY J. WEISS
Princeton University

BURTON I. KAUFMAN. *Efficiency and Expansion: Foreign Trade Organization in the Wilson Administration, 1913-1921*. (Contributions in American History, number 34.) Westport, Conn.: Greenwood Press. 1974. Pp. xviii, 300. \$12.50.

ARTHUR S. LINK *et al.* *Wilson's Diplomacy: An International Symposium*. (The American Forum Series.) Cambridge, Mass.: Schenkman Publishing Company; distrib. by General Learning Press, Morristown, N.J. 1973. Pp. xiv, 120. Cloth \$7.65, paper \$4.50.

These two volumes, one a research monograph and the other a collection of essays by inter-

nationally known historians, add significantly to the knowledge about Woodrow Wilson's presidency and point to areas in need of additional research. Kaufman argues convincingly that "the Wilson administration fundamentally altered American foreign commercial policy by coordinating and integrating foreign trade activity and, in the process, by defining new business-government relationships that continued to exist even after Wilson left office in 1921" (p. xiii). Such a policy, Kaufman notes, points out some of the contradictions in Wilsonian thought, for while Wilson advocated the principles of laissez faire and free-marketplace competition, his administration assumed a much greater role in "directing the nation's foreign trade forces, even going so far as to exempt business combinations engaged in foreign trade from the provisions of the antitrust laws" (p. xiii).

Kaufman's study builds on the works of Gabriel Kolko, Robert Wiebe, Paul A. Koistinen, James Weinstein, Robert Cuff, Carl Parrini, and Joan Hoff Wilson. All these authors have examined the changing nature of the American political economy in the Wilson years and noted the cooperation between government and business, especially because of World War I. But some of them, according to Kaufman, place too much stress on the differences of strategy for economic development between the government and certain sectors of private business, and as a result they seriously underestimate "the coherence of purpose, the breadth of accomplishment, and, indeed, the historical uniqueness that characterized business-government efforts at foreign trade organization during the Wilson years" (p. xvi).

Kaufman concentrates on the organization of foreign trade, pointing to the frequent cooperation between government and business. Of special interest are the discussions of Wilson's personal views on foreign trade; of the influence that the dramatic growth of German trade, aided by close cooperation between the German government and merchants, had in America; and of the intensive rivalry between the United States and Great Britain for trade supremacy in Latin America before, during, and after the war.

Kaufman notes that there were problems in the relationship between business and government that had not been resolved by 1921. The Edge Act proved inadequate to meet the medium- and long-term investment requirements of American overseas commerce. The depression of 1920-1921 brought a contraction of American banking operations overseas. And neither the attempt to develop an inland-waterway network in the United States nor the effort to construct an effective American merchant marine proved successful.

Nonetheless, business and government leaders found vast areas of agreement by the end of the Wilson presidency. "Never before," Kaufman writes, "had business and government worked so closely in developing overseas markets; never before had an administration assumed such an active role in leading and directing the nation's foreign trade forces" (p. 262).

Wilson's Diplomacy: An International Symposium is a collection of essays and rejoinders by Arthur S. Link, Jean-Baptiste Duroselle, Ernst Fraenkel, and H. G. Nicholas. The editors hope that, by bringing scholars from the United States, France, Germany, and Great Britain together, a more complete picture of Wilson's contribution in international relations will emerge. The result is not altogether successful, as the four essays have no common theme. Yet, some of the essays are interesting.

Link's essay is a revision of one of the Albert Shaw Lectures he delivered at The Johns Hopkins University, published in *Wilson the Diplomatist, a Look at His Major Foreign Policies* in 1957. The most fascinating aspect of Link's essay is how his view of Wilson has changed in recent years, especially regarding Wilson's knowledge of the influence of economics upon American diplomacy. Clearly, recent historiography has helped Link reach the conclusion that Wilson was much more knowledgeable about economics than previously realized, and Kaufman's study will probably reinforce Link's view on this subject.

The Duroselle and Fraenkel essays discuss Wilson's reputation in France and Germany. The Duroselle essay measures Wilson's contemporary popularity in France, especially in newspapers. Unfortunately, Duroselle's essay has too little analysis. The Fraenkel essay is much better, showing Wilson's emergence in Weimar Germany as a scapegoat for Germany's troubles. While Wilson was soon forgotten in France, he remained a dominant figure in German history throughout the 1920s and 1930s, and the causes for this difference merit further research. The least successful essay is that by Nicholas, which stresses the weaknesses of Wilson's diplomacy. The book concludes with rejoinders from Link, Duroselle, and Nicholas.

Specialists on the Wilson presidency and on American foreign relations will find much of value in these two very different volumes.

EUGENE P. TRANI
Southern Illinois University,
Carbondale

YVES-HENRI NOUAILHAT. *Les Américains à Nantes et Saint-Nazaire, 1917-1919*. Preface by PAUL BOIS. (Annales littéraires de l'Université de Nantes, number 4.) Paris: Les Belles Lettres. 1972. Pp. 250.

Nantes and Saint-Nazaire were the main war matériel entry ports for the American Expeditionary Force. Yves-Henri Nouailhat has written a model history of the resulting flood of 3,300,000 tons of equipment and supplies and, not counting transients, some 60,000 men. Engineering problems were handled very well. But mutually reassuring official visits—carried to the point of obsequiousness by both sides—did little to curb French working-class irritation over prices, property damage, liquor, women, American trigger-happiness, and M. P. brutality, especially toward their own black stevedores. Thousands of American motor vehicles pot-holed the roads, caused monstrous traffic jams, killed four people and five horses, and hit thirty-four horsedrawn and forty-eight motor vehicles, thirty-four bicycles, nine wheelbarrows, and seventy-six streetcars in Nantes alone. But bad memories seem to have been as ephemeral as the great expansion of American trade through the ports. When the Germans destroyed Mme. Whitney's monument to American aid, the "American presence" in this part of Western France had become "a golden age."

THEODORE ROPP
Duke University

NORRIS HUNDLEY, JR. *Water and the West: The Colorado River Compact and the Politics of Water in the American West*. Berkeley and Los Angeles: University of California Press. 1975. Pp. xxi, 395. \$20.00.

In the history of the American West, no resource has been more precious than water. Because of its scarcity, Westerners have historically used it with greater care than their better endowed Eastern counterparts. Norris Hundley's impressive monograph treats the West's major effort to agree on equitable usage of this vital but limited commodity: the Colorado River Compact of 1922. The book posits that growing federalism finally overshadowed the attempt of seven Western states in the Colorado River Basin to solve their water problems. Using the 1922 compact as the focus of his study, the author devotes appropriate attention to antecedent problems, to negotiation of the compact under the leadership of Secretary of Commerce Herbert Hoover, and to its ratification. In his analysis, Hundley deals with the compact's implications for Mexico since the river reaches the sea through her territory. While American negotiators observed Mexico's legitimate interests, they strove not to augment any Mexican claims to waters of the Colorado. Although in 1922 they paid scant attention to the rights of reservation Indians to water, later adjudication established these rights. Because of decreased flow of the streams,

however, Indians have never irrigated as many acres as the Supreme Court said they could.

Prodigious research went into this monograph, as is reflected in the thorough, even heavy, documentation. While the author's admirable organization and clear prose provide ample thrust and movement, his extensive research has led him to include more information than necessary to establish his points, particularly in the chapter dealing with ratification of the compact in the seven involved states: Utah, New Mexico, Wyoming, Colorado, Nevada, California, and Arizona.

A major strength of the volume is the lucid analysis of complex legal and political aspects of water law and usage. Nowhere have I seen a clearer exposition of the conflicting water laws of the West—common law riparian rights and the doctrine of prior appropriation. Hundley has made a significant contribution to a difficult subject. In doing so, he demonstrates the kind of sophisticated analysis needed to provide understanding of the American West.

WALTER RUNDELL, JR.
University of Maryland

JOHN DOUGLAS FORBES. *Stettinius, Sr.: Portrait of a Morgan Business Partner*. (Publications of the Graduate School of Business Administration of the University of Virginia.) Charlottesville: University Press of Virginia. 1974. Pp. xii, 244. \$12.00.

This biography of Edward Stettinius, Sr., business executive and later partner in the J. P. Morgan investment banking firm during the First World War, is a disappointing book. The author may be only partially responsible for the outcome. One strongly suspects that John Douglas Forbes, a member of the Virginia faculty, was recruited to write a volume honoring the father of one of the university's most famous alumni, Edward Stettinius, Jr., Secretary of State under FDR, following the very recent donation of family papers to the university's manuscript collection. As Forbes candidly admits, there were grave misgivings from the outset whether sufficient material was extant on the senior Stettinius to prepare a genuinely scholarly book. There was not.

The available material has been stretched to the limit. There is much padding; we are frequently given lengthy samples of unedited correspondence, which often contains as much trivia as substance. Also included in the text are such items as news clippings from the society pages of the *New York Times* recounting the activities of the smart Long Island set to which the family belonged in the twenties. Much of this sketchy material is little more than memorabilia. Furthermore, the data are heavily biased. An indefatigable worker, Stet-

tinus was truly generous with his time and money, and friends and associates invariably depicted him in glowing terms. Undoubtedly much of the praise was merited, but the author seems largely unaware of the danger of constantly repeating his subject's virtues; many readers will probably receive the impression that this book was intended as another sympathetic rehabilitation of a prominent member of the Wall Street establishment. Stettinius had the misfortune to be posthumously linked to that imaginary group of businessmen the Nye committee maliciously labeled "merchants of death" in the 1930s, and while Forbes labors to absolve the Morgan partner from this cruel allegation, he overdoes it.

Despite the paucity and inherent bias of the primary sources, the author might have improved the book substantially if he had provided a better framework for discussing Stettinius' activities as the American purchasing agent for the British and French during the first two years of World War I and later as a member of the American mobilization (and demobilization) team. For example, there is no evidence that he consulted the important work of Robert Cuff or Paul Koistinen on the complexities of business-government relations during the war period.

EDWIN J. PERKINS
University of Southern California

AL-TONY GILMORE. *Bad Nigger! The National Impact of Jack Johnson*. (National University Publications.) Port Washington, N.Y.: Kennikat Press. 1975. Pp. 162. \$9.95.

RICHARD EDWARD LAPCHICK. *The Politics of Race and International Sport: The Case of South Africa*. (Center on International Race Relations, University of Denver. Studies in Human Rights, number 1.) Westport, Conn.: Greenwood Press. 1975. Pp. xxx, 268. \$13.95.

Historians have never made much use of sport as a vehicle for social analysis. Yet in countries like the United States and South Africa where sport is an important public activity intimately linked with matters of race and class, the potential for doing so is obvious. Traditionally, the ethos of sport has operated to obscure the political and sociological dimensions of athletics, but this is no longer the case. The use of recent Olympic games as a political forum by groups as diverse as black Americans, Palestinian terrorists, and cold warriors from both sides of the iron curtain is but one illustration of the close and growing union between sports and politics.

The union is hardly new, as the two books under review demonstrate. If neither book is felicitously written, both are informative and useful for histo-

rians and sociologists. Based primarily on newspaper sources, Al-Tony Gilmore's *Bad Nigger!* focuses on public reactions in white and black America to the activities of Jack Johnson, the first black heavyweight boxing champion. The years of Johnson's championship, 1908-15, were years of great difficulty for all blacks, including athletes. Jim Crow reigned in the boxing ring as elsewhere; white titleholders refused to fight black challengers. In 1908, however, heavyweight champion Tommy Burns accepted a challenge from Jack Johnson in hopes that an interracial fight would revive interest in boxing and incidentally earn him some money.

Johnson defeated Burns handily and proved at once to be a singular champion in and out of the ring. He was not what whites called "a credit to his race." He flouted the customs of Jim Crow, married three white women and had liaisons with many others, and defeated a succession of "white hopes." His greatest victory, over Jim Jeffries in 1910, occasioned racial clashes across the nation; his preference for white women provoked white officialdom to accuse and convict him of violating the Mann Act; and his very example caused Jess Willard, who defeated him in disputed circumstances in Havana in 1915, to restore Jim Crow to the ring. In describing public reaction to these events, Gilmore illustrates the political uses of sport, including the pride blacks took in Johnson's accomplishments and the concern Johnson aroused in conservatives like Booker T. Washington. *Bad Nigger!* is too short on analysis to constitute the final word on any of these subjects and too neglectful of Johnson's personal views on racial topics, but it does demonstrate the public impact of Johnson's championship.

The Politics of Race and International Sport, a more substantial work than *Bad Nigger!*, is a detailed, chronological account of the effort, between 1959 and 1970, to get South Africa expelled from the Olympic games and other international sports competition because of its practice of racial apartheid in selecting competitors. The story of this effort can only be described as incredible. The International Olympic Committee and other sports bodies were simply unwilling to enforce the Olympic rule that competitors must be selected solely on the basis of athletic merit. Lapchick shows convincingly that this was due to the dominance of international sports bodies by whites who were willing to tolerate discrimination against blacks in South Africa for racial and political reasons. He shows just as convincingly that expulsion succeeded for the same kinds of reasons. By the 1960s nonwhite athletes could no longer be ignored in international competition, nor could the non-white world be ignored politically. Perhaps the

most revealing aspect of this story is that white South Africans tried to avoid expulsion from international competition by making the first minor, cosmetic concessions they had ever made to critics of apartheid. Sport was that important to them.

I. A. NEWBY

University of Hawaii

JIM POTTER. *The American Economy between the World Wars*. A Halsted Press Book. New York: John Wiley and Sons. 1975. Pp. 183. \$10.50.

Jim Potter wrote this "introductory guide" primarily for British readers. In his words, "it does not set out to be comprehensive, but is intended to provide students with a fairly simple factual outline and with suggestions of differing interpretations." The heart of the book consists of a chapter on the common features of the interwar years, successive chapters on the subperiods 1922-29, 1929-33, 1933-39, and a final chapter assessing the accomplishments of the New Deal. A great deal of quantitative evidence appears in the twenty-eight text tables. Appendices include a chronology of events, a glossary of abbreviations, and some suggestions for further reading.

The writing is clear and concise, the major conclusions fairly compelling. Potter recognizes that the central issue is not the occurrence of a depression in the 1930s but rather its extraordinary severity and duration. He concludes that nothing in the 1920s made a Great Depression inevitable; that the Great Depression reflected a collapse of confidence in the future profitability of private investment; that the New Deal, an expedient and inconsistent set of policies for the attainment of a wide variety of objectives, failed in its recovery efforts precisely because it never succeeded in reviving business confidence; and finally, that the great significance of the New Deal arose from its enduring institutional reforms, not from its relief or recovery programs.

Potter's analysis takes an eclectic form, placing heavy emphasis on the social, political, and institutional context of the economic events. Such an approach has considerable virtues, yet this very eclecticism involves the author in some dubious economic theorizing (e.g., in his indictment of America's "anachronistic" institutions). In a section on "suggested explanations of the Great Depression" I was astounded to find only one sentence concerning the role of monetary policy in the early 1930s but three pages on the long-defunct stagnation hypothesis. Looking back over my copy, I see the margins littered with question marks, exclamation points, and other symbols of discontent, most of them summarized in my general feeling that the text could benefit from a more

careful attempt to integrate its analysis with modern macroeconomic theory.

Despite these limitations, the book generally succeeds well enough on its own terms, as "no more than a summary account, often of exceedingly complicated matters."

ROBERT HIGGS
University of Washington

BURL NOGGLE. *Into the Twenties: The United States from Armistice to Normalcy*. Urbana: University of Illinois Press. 1974. Pp. ix, 233. \$8.50.

Through a series of gracefully written essays on familiar topics Noggle attempts "to synthesize the numerous and discrete ideas and events" in post-World War I America "that spawned the phenomenon labeled 'the twenties'" (p. vii). His work deftly combines a wide range of secondary sources with selected manuscript research. Three brief chapters on reconstruction offer the most interesting and unfamiliar material, all suggesting that a disinterested president and a divided, apathetic Congress squandered the opportunity for postwar reform and revitalization. The chapter on the Red Scare attempts unsuccessfully to attribute more reasonableness to antiradicalism than has generally been conceded. The chapter on diplomacy departs from the narrative mode to present a useful historiographic essay. The penultimate chapter on American society is a polite bow to the existence of social history. The final chapter, "Remnants of Progressivism," does not explore the complex transformation of progressivism but surveys postwar politics from the perspective of several political figures.

The book, unfortunately, sidesteps the complex problems of historical synthesis and explanation. Lacking a sustained thesis, Noggle has no systematic basis for selecting and emphasizing different issues. Why, for example, devote thirty-eight pages to the Red Scare and thirty pages to foreign policy when issues related to urbanization, race, women, prohibition, leisure activities, religion, education, and scholarship are all crammed into twenty-seven pages? Except for the section on reconstruction, each chapter is a watertight compartment sealed off from the rest of the book. The work neither cumulates insights nor offers variations on an underlying theme. It even fails to include a summary or conclusion.

If Noggle had actually explained aspects of the twenties by reference to ideas and events in the postwar period, he could have established grounds for selection and emphasis and attained the unity and direction necessary for historical synthesis. Yet he short-circuits successful explanation by fail-

ing to specify those features of the 1920s that he wishes to understand. He mentions only such vague tendencies as "weariness," "apathy," "reaction," "chauvinism," and "disillusion," virtually caricaturing the decade. Noggle's argument about the failure of political leaders to exploit opportunities for reform could have evolved into a persuasive causal explanation, but it is neither thoroughly developed nor extended throughout the work. Noggle, moreover, never examines alternative hypotheses or critically assesses their logical and empirical validity.

The author appears to have scaled the historical mountain of post-World War I America simply because it had not been done before. The climb, however, has provided neither a coherent perspective on the period nor any new eye-catching insights or information. But he does present material that is not readily available elsewhere about a generally neglected period. I hope that his work will stimulate more systematic research.

ALLAN J. LIGHTMAN
American University

ARTHUR E. MORGAN. *The Making of the TVA*. Buffalo: Prometheus Books. 1974. Pp. xiv, 205. \$10.95.

Arthur Morgan was the first chairman of the Tennessee Valley Authority, and this volume, written in Morgan's ninety-sixth year, is an account of both his years on the Authority and his life as an engineer and social planner.

A generous portion of the book is devoted to the conflict between Morgan and his antagonists on the Authority, David E. Lilienthal and Harcourt Morgan. For Arthur Morgan, it was primarily a situation where his "ethical principles" and devotion to public service were arrayed against the "politics-as-usual" and patronage of his colleagues. Morgan, who wanted to use the TVA to enhance the overall quality of life for the people of the region, ultimately realized that his hopes were doomed when President Roosevelt succumbed to the more limited vision and public-power pre-occupations of Morgan's foes. In 1938, accused by his colleagues of being an impractical visionary whose dreams were hindering the development of public power, Morgan was dismissed by the president after failing to substantiate his own charges that his fellow directors were guilty of deceit and dishonesty.

Morgan also describes his long and distinguished life in engineering. Among other accomplishments, he includes his introduction of "dynamic design," which consisted of never finally completing a design "but of keeping it open to take full account of any special advantage or disadvantage that appeared in the course of construction"

(p. 95). Although criticized by others who misunderstood the approach, Morgan claims that it not only saved money but also put unemployed men to work sooner.

Despite his personal frustrations during those years, Morgan considers them among the most significant and rewarding of his long life. He insists that, at last, his broader concept of what the TVA ought to be has been realized through many of the Authority's projects in recent years.

THOMAS H. COODE
California State College,
Pennsylvania

R. J. C. BUTOW. *The John Doe Associates: Backdoor Diplomacy for Peace, 1941*. Stanford: Stanford University Press. 1974. Pp. x, 480. \$16.95.

For a full year preceding Pearl Harbor, Father John Drought attempted to influence the negotiating positions of the Japanese and American governments. Joining Drought as the "John Doe Associates" were his superior, Bishop Walsh, Postmaster General Frank Walker, a Japanese banker Tadao Wikawa, and a highly placed officer of the War Ministry, Colonel Hideo Iwakuro. On the periphery hovered Prince Konoye and Yosuke Matsuoka, Roosevelt and Hull. Under Drought's guidance the associates produced: 1) a "working analysis" to rally support among Japanese expansionists for a diplomatic overture to Washington; 2) a vaguely worded draft agreement; and 3) a proposal for a head of government summit (the Roosevelt-Konoye meeting which never took place).

Did these proposals or their authors have much backing from either government? Since the Tokyo War Crimes trials revealed the existence of the associates, this unsolvable mystery has bedeviled historians. The possible answers are many and complex. Some or all of the associates may have been direct agents, others only private parties; even if some were agents, the degree of government control may have been slight.

Butow provides a simple answer: The well-meaning associates were all amateurs, acting on their own, who succeeded in convincing both governments that they represented the "other side." Since the associates tended to minimize differences between Washington and Tokyo, the deception was most unfortunate. When the true position of each government was revealed, it seemed as if the gap had widened. Further negotiations seemed pointless. Both sides prepared for war.

Butow's theme, that amateur diplomats rarely advance peace, is sound, but his indictment is much too harsh and simple. Clearly, officials in both governments decided at some point to use

these private channels to seek a settlement precisely because they were disavowable. It is equally clear that the secrecy used to render them disavowable has hopelessly obscured the circumstances of governmental involvement. (If the evidence points in any direction, it is toward Iwakuro and Walker as the real agents, two figures who never clearly emerge in Butow's narrative.) But it is unlikely that either government much trusted the associates as a group or was much misled by their representations.

Butow's accusation that Drought spoiled any chance of a settlement (p. 320) seems particularly unfair coming from an author who does not really believe peace was possible and who applauds Hull's consistent rejection of any compromise peace as unacceptable appeasement (pp. 295, 301). But the United States, having failed to challenge one Japanese fait accompli after another, could only have avoided war in 1941 by some degree of appeasement, as Drought implied—and as the abortive American *modus vivendi* acknowledged. When Hull rejected appeasement, war was inevitable; this was probably the right decision but not better "diplomacy." Clinging desperately to peace, Drought was a minor figure in this tragedy.

While Butow's handling of Japanese sources is unmatched, and his insights into the intricacies of negotiations are often brilliant, he follows Hull too closely, even to the point of assuming Hull's prosecuting manner. Historians do well to avoid such a role.

BRIAN LORING VILLA
University of Ottawa

JOSEPH W. ESHERICK, editor. *Lost Chance in China: The World War II Despatches of John S. Service*. New York: Random House. 1974. Pp. xxviii, 409. \$12.95.

This book contains some seventy-one dispatches and memoranda written by John Service, a Foreign Service career officer stationed in China from the fall of 1935 to his recall during the spring of 1945 amidst governmental charges of pro-Communist sympathies, of which he was eventually cleared in 1957 by the Supreme Court.

The documents, twenty-six of which have never been published elsewhere, constitute a valuable addition to the voluminous literature on U.S.-Chinese relations during the last years (1941-1945) of World War II, a period that witnessed the increasing deterioration of the Kuomintang and the dynamic growth of the Chinese Communist movement. The chief virtue of these reports appears to lie in John Service's vivid and perceptive observations in his capacity as a member of the U. S. Army Observer Section to the Chinese Communist

headquarters at Yen-an in 1944-1945. Service's assignment was to report on the political and economic conditions prevailing in the Communist-controlled border region and on policy statements of the Chinese Communist leaders such as Mao Tse-tung and Chou En-lai. In view of the current diplomatic impasse between the United States and the People's Republic of China, it is of vital interest for those scholars and experts studying U. S.-China relations, as well as for policy-makers in Washington, to reexamine these incisive dispatches. Indeed the "Lost Chance in China" is still to be regained!

Unfortunately, the arrangement of the documents leaves something to be desired. Grouped under three broad topic headings, *Kuomintang China*, *The Communist Areas*, and *The Debate with Hurley*, they have no strict chronological order or centralizing theme to tie the various parts and headings together. There are perhaps too many repetitious passages and *non sequiturs*. But the editor deserves credit for his succinct notes which are interspersed throughout the book.

J. CHESTER CHENG
San Francisco State University

THOMAS C. KENNEDY. *Charles A. Beard and American Foreign Policy*. Gainesville: University Presses of Florida. 1975. Pp. xi, 199. \$8.50.

Thomas C. Kennedy has exhaustively and painstakingly examined all of Charles Beard's writings and all writings about him. The author has also made extensive use of manuscript collections, and he has garnered information from various private sources for fifteen years. He is particularly interested in appraising Beard's later views on American foreign policy, and his book is a tour of the historiography of the late forties and the fifties. While Kennedy concedes some of the details of Beard's indictment of the "interventionist" diplomacy of 1939-45, he endorses the consensus which "interventionist" scholars had established about Beard's work by the middle fifties: stemming from a doctrinaire, isolationist viewpoint, Beard's later scholarship is that of a publicist and not a detached scholar.

Although Kennedy's book is conscientious, it is not informed by interpretive penetration. The "detached scholars" Beard is implicitly contrasted to are such State Department partisans as Herbert Feis, William L. Langer, and Arthur Schlesinger, jr. If Kennedy must evaluate the truth of Beard's history, fairness to Beard demands that his work be measured against the available sources rather than the writings of historians of the fifties whose books were never out of step with their liberal internationalist politics. More importantly, Ken-

nedy fails to appreciate the central importance of Beard's belief that American involvement in war was destructive of reform at home. Certainly this belief led Beard to lose his critical sense in attacking Franklin D. Roosevelt's war policies: tormented by what he feared would be the domestic consequences of the war, Beard made FDR the chief villain of the 1939-41 period; for Beard the executive's failure to solve the problem of depression led to adventure abroad. Beard's views on New Deal diplomacy are a parody of the truth, but this is insufficient warrant for dismissing his understanding of the link between foreign and domestic policy. Kennedy treats this belief as an *idée fixe*, all but ignoring recent scholarship on the cold war. But surely time is past to examine whether causal connections exist between the diplomacy of 1939-49 and the events that terrified Beard—the loss of civil liberties, the regimentation of thought, the growth of a monstrous military establishment, and the large-scale and deliberate deception of the American people by a president of the United States.

Charles Austin Beard left all historians of the Republic in his debt. He still needs a study that is both diligent and critically intelligent.

BRUCE KUKLICK
University of Pennsylvania

DARWIN PAYNE. *The Man of Only Yesterday: Frederick Lewis Allen*. Foreword by RUSSELL LYNES. New York: Harper and Row. 1975. Pp. xi, 340. \$12.50.

This biography deals more with Frederick Lewis Allen's career as magazine editor than with his work as popular historian. Educated at Groton and Harvard, Allen served apprenticeships with the *Atlantic Monthly* and *Century* before joining the staff of *Harper's Magazine* in 1923. As editor from 1941 to 1953, he guided that periodical through some of its finest years. Payne praises Allen for not attempting to woo a mass circulation and building up instead a loyal readership among thoughtful people. Although Allen published distinguished stories and poems, he gave much more space to provocative articles. He had a flair for developing new writers. He defended intellectual freedom by resisting the pressure of Senator McCarthy and other pseudopatriots.

Busy though he was, Allen found time, mostly evenings and weekends, to write *Only Yesterday* and other best sellers. It is easy to criticize these. Allen relied mostly on secondary materials. He dealt too much with the fascinations of collective behavior—changing styles, popular hobbies, and the like—and not enough with major problems, such as the power structure, the frustrations of various groups, and the blight of poverty. Yet his work was never

cheaply popular. Written in a sparkling style, it challenged the reader to think about the significance of recent events.

Payne's biography is solid rather than brilliant. It provides useful information about the magazine world and the impact of two world wars. But it is not as penetrating as one might wish in probing the virtues and deficiencies of Allen's genteel liberalism.

NELSON M. BLAKE
Syracuse University

D. CLAYTON JAMES. *The Years of MacArthur*. Volume 2, 1941-1945. Boston: Houghton Mifflin Company. 1975. Pp. xix, 939. \$15.00.

With his second volume of the MacArthur biography, D. Clayton James continues to meet the high standards of careful research and scholarly writing set in his study of the Pacific commander's earlier career. The volume covers more familiar ground than the first book, but it is of great significance for the student of World War II.

The author has drawn heavily on official histories of the war in the Philippines and the South and Southwest Pacific. He has added to this material extensive study of the MacArthur papers and those of many of his contemporaries, has restudied official papers used by others, and has interviewed scores of the general's former subordinates and colleagues. He is restrained in his judgments and fair in his assessments of blame.

The picture of MacArthur is a more fully fleshed-out sketch of the character so admirably portrayed in the first volume—a highly controversial leader who was convinced of his special gifts for strategic thinking and high command and of his special destiny. Certain that proper strategy required greater attention to operations in the Pacific rather than in Europe, he was quick to assume that any failure to follow his lead was evidence of a plot against him in the White House and War Department. His frustrations resulted in attacks on the Navy, the Joint Chiefs of Staff, and the British.

On several occasions he reminded General Marshall that the people of the United States would not like to be told that the British were dominating American policy or that he was deprived of the opportunity to hasten the defeat of the Japanese. In such a state of mind he failed to discourage suggestions that he run for the presidency. Through the fall of 1943 and into the first few months of 1944, some newspapers spoke of him as Roosevelt's McClellan, and some members of Congress proposed that he be made super-commander of the armed forces.

James gives full credit to MacArthur for rallying the people of Australia, for backing General Ken-

ney in his air developments in the Southwest Pacific, for full cooperation with Admiral Halsey, for the use of air supply of troops and end-run amphibious operations to press his attacks. But James also raises questions about the failure to disperse planes at Clark Field long after MacArthur knew that Pearl Harbor had been attacked, about the handling of supplies on Bataan, about the retention of Richard Sutherland as Chief of Staff when MacArthur knew that Sutherland's abrasiveness made for difficulties with the Navy and the army commanders, about MacArthur's claiming credit for the island-hopping concept when others had demonstrated that strategy first, and about his dubious handling of public relations. James stresses that MacArthur's actions often had a touch of insubordination that he would not have tolerated in his own subordinates. James notes that we should not have been surprised by MacArthur's challenge to the President in Korea.

This is a perceptive account of a brilliant but erratic leader who failed to see the world conflict in a global setting and who judged strategy on the basis of the opportunities afforded him. The role of occupation chief in Japan would give him the chance he craved to be master in his own house.

FORREST C. POGUE
Eisenhower Institute for Historical Research

LENORE FINE and JESSE A. REMINGTON. *The Corps of Engineers: Construction in the United States*. (United States Army in World War II: The Technical Services.) Washington: Office of the Chief of Military History, United States Army. 1972. Pp. xviii, 747. \$8.00.

In any complex organization each part tends to insist that its contribution is central to the successful functioning of the whole. Fine and Remington make an effective case that in World War II domestic, military-centered construction was the *sine qua non* of successful mobilization, armament, air supremacy, and the development of the atomic bomb. Responsible for \$15.3 billion worth of construction, the Army supervised the largest building program in our history with boldness and imagination.

Moving with precision through some necessarily unexciting matters, the book studies adequately land-acquisition policies, contract forms, congressional relations, labor disputes, and dealings with contractors large and small. Although the book is included in the sub-series on the Corps of Engineers, half of it is concerned with the Quartermaster Corps, which, until December 1941, controlled the largest share of the program. There is extensive and evenhanded coverage of the bitter,

three-way struggle among the partisans of the Quartermaster Corps, the Corps of Engineers, and those who supported a completely separate construction division. The Engineers were victorious and, the authors contend, clearly demonstrated superior capabilities for planning and construction. Never completely explained is the lack of adequate preparation by the Quartermaster Corps for national mobilization.

The authors have mined the extensive military records and interviewed or corresponded with several hundred participants in the gigantic effort. The work is technically precise, and the footnotes are where they belong. The writing style, while clear and steady, is occasionally marred by clichés. It may seem a minor point, but it is unfortunate that the authors insist on referring to the relocation of "west-coast Japanese," without noting that a majority of them were, in fact, American citizens of Japanese ancestry.

The final chapter on the Manhattan Project is clearly the most dramatic. The two-billion dollar leap into the scientific darkness linked the Army with industry, labor, and the academic community and provided the capstone of the Engineer's efforts in World War II. It is sufficient praise to conclude that this lengthy volume hews to the high standards of scholarship set by the earlier works in the series.

CHARLES W. JOHNSON
University of Tennessee

JEAN EDWARD SMITH, editor. *The Papers of General Lucius D. Clay: Germany 1945-1949*. Volumes 1 and 2. Bloomington: Indiana University Press. 1975. Pp. xii, 524; ix, 527-1210. \$35.00 the set.

The publication of the recently declassified papers of General Lucius Clay should touch off debate within scholarly circles. On the basis of these volumes, the stereotyped view of General Clay as the originator of the Berlin airlift and dogged defender of American interests in Germany will give way to a more realistic appraisal of the man and his actions. The picture that emerges of Clay is that of a decisive, competent, and moral soldier increasingly disturbed by the breakdown of quadripartite government in occupied Germany. French intransigence, British suspicion, and growing hostility between the Americans and the Soviets brought about the demise of Allied cooperation and the division of Germany.

The editor of these volumes, Jean Edward Smith, judiciously selected 746 documents from Clay's term as military governor of Germany and presents them in chronological order with brief explanatory notes. The selection scans the wide variety of problems Clay dealt with, ranging from

the preservation of German art to the threat of war, but focuses on the key issues of occupational policies—recovery and the division of Germany. Complicated questions of currency reform, banking, decartelization, and dismantling reveal Clay's mastery of the art of governing. But even more impressive is the amount of influence he had in developing American policy in Germany. In contrast to the trend toward one-man, "star" diplomacy, Clay was more than just an executor of foreign policy—he helped shape it. His papers indicate he had as many disagreements with Washington as he did with Paris, London, and Moscow, but with unflinching determination he backed his arguments with facts, opinions, and, if need be, threat of resignation. More often than not, he won his case. For example, Secretary of State James Byrnes accepted virtually all of Clay's views on Germany in his famous Stuttgart Address of September 6, 1946. Clay was to have far less influence, however, on Byrnes' successors.

Clay's image as a cold warrior is not substantiated by these volumes. By late 1947, when Washington had all but given up trying to cooperate with the Soviets, Clay was still urging cautious but continual negotiations. Moreover, it was Clay who correctly estimated Soviet intent and advised standing firm on the airlift during the Berlin blockade.

Since the focus of the volumes is on the formulation and execution of policy, there is very little on the effects on Germany of U.S. occupation. But that is a minor flaw in what will be one of the major historical collections on the period. The editor and publisher deserve the appreciation of the academic world for their presentation of the Clay papers.

EDWARD L. HOMZE
Naval War College

Foreign Relations of the United States, 1948. Volume 1, part 1, *General; The United Nations*. (Department of State Publication 8805.) Washington: Government Printing Office. 1975. Pp. xvi, 505, xv. \$8.10.

Foreign Relations of the United States, 1949. Volume 2, *The United Nations; The Western Hemisphere*. (Department of State Publication 8789.) Washington: Government Printing Office. 1975. Pp. xii, 827. \$10.40

These documents reveal a dichotomy in American thinking about the UN's role. Major policy statements by Truman and other administration leaders of unequivocal support for the UN were not matched by actions at the international organization. America's reliance on the UN to resolve crucial issues in international relations in 1948 and 1949 was largely peripheral. Ambassador Warren

Austin and his colleagues devoted much energy to bringing the organization into the mainstream of handling major world issues.

Three areas illustrate the frustrations of their efforts. In 1948 there was concern over the Soviet Union's tactics which had blocked agreement in the United Nations Atomic Energy Commission (UNAEC). The U.S. had persuaded a majority to favor its Baruch Plan of 1946 and worried that dropping further efforts to reach agreement in UNAEC would enable Russia to charge the U.S. with halting negotiations as a rationale for an "atomic alliance." More important, most U.S. officials wished to keep the door open, hoping the Soviets would alter their policy. They also saw advantage in utilizing UNAEC for propaganda—to show the world the Russian refusal to accept effective controls of atomic materials. It is worth noting that throughout the extensive documentation on this subject, the Americans connected great power relationships to the atomic weapons topic and nowhere did they consider employing the U.S. atomic advantage to extract diplomatic concessions from the Soviets.

There was also frustration in the efforts to limit the veto power. The General Assembly's Interim Committee recommended a broad category of decisions where the veto would not apply, most notably in resolution of disputes by pacific means and on admitting nations to membership. Russia denied the legitimacy of the Interim Committee's work, and so all efforts in this field failed. This subject especially concerned State Department officials because public and congressional frustration was rising over apparent weaknesses of the UN. The U.S. delegation finally decided that the chief benefit in the issue was to bring public pressure to bear on Russia, showing the world how the Kremlin had barred progress toward reconciling disputes.

Russia's attack on NATO in 1949 as an aggressive instrument consumed much American attention. American attitudes had now hardened, and the documents indicate U.S. officials were pleased that the Soviets had moved openly toward "world domination." Otherwise, as Assistant Secretary for International Organization John D. Hickerson put it: "Had they lulled us into a sense of false security for a few years, they could easily have ruined us." U.S. spokesmen were eager to employ the UN as a vehicle for pinning the blame for the cold war on Russia. They devoted much of their time to marshalling support for "The Essentials for Peace" resolution to counter the Soviet denunciation of NATO, and the measure passed in November.

Other topics include relations between the U.S. government and UN headquarters. They range

from taxation of American employees, through delineating the area in which UN employees and foreign representatives would enjoy diplomatic immunity, to the problem of *laissez passer*. The latter question caused the greatest worry. State Department officials already felt the early waves of the anticommunist hysteria which were soon to sweep across America. Charges were made, some from within the department itself, that communist agents were gaining entry into the country via the UN for purposes of subversion. Material is disappointingly sparse on the important topic of the organizational relationship between the Department of State and the UN delegation. A few documents in the 1949 volume illustrate the lack of coordination on foreign-policy formulation, but historians will have to examine the files to pursue this subject.

Coverage of Western Hemisphere relations varies considerably in detail. Material relating to Canada is very scanty, confined to brief references on trade and mutual defense questions. Of the South American countries, strongest coverage is provided for affairs in Argentina, Bolivia, Brazil, and Chile. The volume indicates that neighbors of Argentina feared her intentions, especially during the revolutionary upheavals in Bolivia during 1949. U.S. representatives emerged in a mediator role. A significant amount of material on Latin America details economic foreign policy. The evidence suggests lack of a coordinated U.S. policy for the region, other than financial support through the Export-Import Bank or World Bank and modest military programs under the Mutual Defense Assistance Act. A joint Brazil-U.S. Technical Mission is covered, and the documents show American perception of the need to foster land reform to avert dangerous social revolution. The general tenor of U.S. officials reporting to Washington, however, was to stress immediate matters of political stability. With the exception of Colombia, Guatemala, and Panama, the theme of communist penetration is not pronounced in dispatches to Washington, although this problem did occupy Acheson's attention.

The editors of future volumes might consider providing fuller editorial summations of foreign-policy developments. Those that occur in these volumes are extremely helpful.

THOMAS M. CAMPBELL
Florida State University

Foreign Relations of the United States, 1949. Volume 3, Council of Foreign Ministers; Germany and Austria. (Department of State Publication 8752). Washington: Government Printing Office. 1974. Pp. xxv, 1324. \$14.55.

Like most of its forerunners, this volume is indispensable to the study of American diplomatic history. In both formal and informal relationships the machinery of diplomacy is laid bare. At times this makes the reading of documents and editorial notes somewhat tiresome, but it compensates with extremely engrossing material, and at other times the book is difficult to put down. The documents' original classifications—"secret," "top secret," "top-secret routine," "top-secret priority," "top secret and personal," "top secret, no distribution," "eyes only"—do help in setting a value on subject matter, as the statesmen and diplomats who worked with them thought they should.

That some of the documents in this volume are captivating goes without saying, especially when the reader reaches the Berlin blockade. The Joint Chiefs of Staff took the position that if the Soviet Union reimposed the blockade of Berlin the United States might want to consider it "dangerously close to an act of war" (p. 819). Secretary of State Acheson responded that in the event "we should immediately inform Soviet Government that we consider it hostile act," and "we would be squarely faced with the issue whether we would break the blockade even at the risk of war" (pp. 826-827).

Composed of nearly 1,300 documentary pages, this "fat" book (State Department publication 8752) covers the establishment of the German Federal Republic at Bonn and its economic, political, and international problems, the Soviet Union's steps toward the formation of the so-called German Democratic Republic, and the American attitude toward the two Germanies, the blockade of Berlin, and the establishment of an independent Austria.

Since relations with Austria never played the same role in the popular mind as German affairs, the Austrian documents are extremely interesting: what the Allies had in mind, in what ways they differed among themselves, and how the Soviet Union might, some thought, set up a separate government in eastern Austria in order to give the Russians an important central European strategic center, including the Vienna transportation network and the Austrian Danube. The American legation in Vienna held that the advantage in having American troops in that city, even as a token force, was not that they could successfully resist a Russian absorption of the capital, but that as long as such troops were present any Soviet action *would also be against the United States*, involving "a threat of war."

The more dangerous locale was Germany, however, not Austria, even though Austrian policy turned on the objectives of terminating the Allied military occupation and restoring Austria's sover-

eignty, which the Soviet Union would not accept in 1949. Instead the Soviet Union gave strong support to Yugoslav claims on Austria's frontiers, including an autonomous government for Slovene-speaking people, in furtherance of Soviet objectives in Yugoslavia itself. President Truman said in October 1949 that, regardless of the Soviet Union's position, "we should adhere firmly to our position that we would not agree to an Austrian Treaty which made it impossible for Austria to survive as an independent nation." Nor did he believe paying \$200,000,000 a year to remain in Austria was an excessive price for preventing the Russians "from extending the Iron Curtain to the western boundaries of Austria, outflanking Germany and Yugoslavia, and positioning themselves at the Brenner pass" (p. 1168).

Still, there was no grave development in Austria and no war danger, as there appeared to be on the German-Berlin question. And on that, the documents show how the Western nations' agreement to a North Atlantic defense treaty resulted in the Soviet Union's abandoning measures of duress against the Western powers in Berlin.

How the Soviet Union refused to agree to a common German currency as a means of stalling on German unification and creating her own dependent East Germany makes extremely interesting reading even for people accustomed to the techniques of diplomatic aims and purposes. The unfolding of West Germany's metamorphosis from a subjected and conquered country into a trizonal fusion, to be followed by the adoption of the "Basic Law" for a sovereign West Germany is still something that holds one's attention in both general outline and detail—especially the detail, so much of which was not known before this volume was made available.

ALBERT NORMAN
Norwich University

CHARLES C. ALEXANDER. *Holding the Line: The Eisenhower Era, 1952-1961*. (America since World War II.) Bloomington: Indiana University Press. 1975. Pp. xviii, 326. \$12.50.

JIM F. HEATH. *Decade of Disillusionment: The Kennedy-Johnson Years*. (America since World War II.) Bloomington: Indiana University Press. 1975. Pp. xvi, 332. \$12.50.

The appearance of volumes by Charles C. Alexander and Jim F. Heath constitute an impressive debut for the "America Since World War II" series under the general editorship of Warren Kimball. Together, they survey the cold-war years of Dwight D. Eisenhower through the demise of Lyndon B. Johnson's consensus. Developing the

themes of the most recent Eisenhower literature, Alexander weaves a deft analysis of the political, cultural, and economic conditions during the allegedly placid fifties, while Heath effectively reviews the turbulence that destroyed all hopes for a Democratic victory in 1968. Utilizing published sources almost exclusively and aiming mainly at the student, both are written with clarity, balance, and perception. The addition of comprehensive bibliographic essays further enhance works that ought to serve as models of their genre.

Neither Alexander nor Heath sees angels, devils, or dark conspiracies emanating from the White House. Both find individuals trapped by their own perceptions as well as the political and military realities of the United States in the cold-war. All three presidents reacted within the limitations of what they considered possible and necessary. Thus Eisenhower was not unlike most fellow Americans in seeing "the flow of world events through a prism of cold war conflict, of incessant struggle against a multi-faceted, nearly omnipresent Communist enemy" (p. 27). Heath's portrait of the Kennedy-Johnson period reveals two presidents who, representing a party that had been under persistent attack for being somewhat less than totally committed to "victory," were obsessed with "standing up" to communism. Caught between domestic forces that were perhaps beyond the scope of their ability to deal with, the ultimate result was failure. All that came despite the fact that, as Heath concludes, "Kennedy and Johnson were fundamentally decent men, whose ambitions both reflected and influenced the American people. The two Presidents believed that they could shape the destinies of the United States and the world by a combination of will and pragmatic knowledge" (p. 303).

Although judged as "confirmed a cold warrior as anyone," Eisenhower nevertheless emerges as the most effective of the three men. What William Shannon once termed the "great postponement" begins to "look more and more like a good job of holding the line—against the agonies and excesses of the post-Eisenhower years" (p. 292). Alexander believes that the president "revealed more understanding of the transformations the cold war had wrought in American government and economy than any other President. The commonly held belief that Ike knew more about matters involving the military than anybody else had much truth in it" (pp. 290-91). Undoubtedly, Eisenhower bequeathed neglect that inevitably plagued his successors, but Heath views Kennedy and Johnson as obsessed by the need to prove their strength. While much of what happened was beyond their control, the results were "often of their own making." By 1968, disarray, violence, and dissent characterized

America. "Instead of finding its Utopia, America became a country struggling desperately to escape its Armageddon" (p. 13).

Both historians offer some questionable judgments. Alexander contends that there *was* somewhat of a missile gap from 1959 until 1961. Heath ignores the surveys showing that Kennedy lost votes because of his religion and that the Catholic electorate also responded to economic conditions and concludes that a Protestant might not have done as well. Yet he offers no contrary evidence. Lamenting the plight of the civil-rights bill at Kennedy's death, Heath overlooks its extrication from the House Judiciary Committee in October and subsequent presentation before the full house on November 20, two days before the Dallas tragedy. These are minor quibbles, however, hardly enough to diminish enthusiasm for the forthcoming contributions to this series.

HERBERT S. PARMET
Queensborough Community College
City University of New York

ROGER MORGAN. *The United States and West Germany, 1945-1973: A Study in Alliance Politics*. London: Oxford University Press, for the Royal Institute of International Affairs and the Harvard Center for International Affairs. 1974. Pp. x, 282. \$22.50.

Roger Morgan has the credentials for producing an overview of American-West German relations for the past thirty years: he is Deputy Director of Studies for the Royal Institute of International Affairs; he has written on contemporary German politics; and he spent a year at the Harvard University Center for International Affairs. Surprisingly, the resulting book does not reach the depth it could have achieved.

The fault stems mainly from the author's decision to employ a methodological scheme which is imposed upon, rather than derived from, the historical data. The structure of each of the ten narrative chapters is made to conform to a set mold: internal politics of both countries, bilateral relations, alliance politics, and relations with the adversary. With this "rigorous framework of concepts" Morgan hopes to furnish a paradigmatic tool for use on other bilateral alliances (pp. 3-5).

The method's shortcomings, however, as revealed in this experiment, caution against repetition. Some important subjects are merely sketched, while others, like Eisenhower's "tripwire" proposals, are ignored in order to make room for relative trivia. The very term "alliance" becomes ambiguous, meaning sometimes a bilateral "special relationship" and sometimes the larger NATO context. Factual errors accumulate, such as a reversal in referring to currency revaluation and

devaluation (p. 250), thus materially affecting the quality of the volume. Finally, the major conclusion of the study provides little that is new—relations between the two partners generally remained harmonious, but were determined at any given moment by the degree of compatibility in their stances toward the common adversary (pp. 247–48).

Yet the assessment should not be entirely negative. Although restricted to the Dulles Collection at Princeton for archival sources, Morgan has culled a variety of materials in English and German; therefore, the bibliography is good. Future writers should consult it long before their visit to the National Archives and presidential libraries, which have begun, with agonizing slowness, to release the documentation from the American side needed to survey meaningfully postwar relations between the United States and its best continental ally.

JOSEPH MAY
Youngstown State University

LYNN ETHERIDGE DAVIS. *The Cold War Begins: Soviet-American Conflict over Eastern Europe*. (Written under the auspices of the Institute of War and Peace Studies, Columbia University.) Princeton: Princeton University Press. 1974. Pp. x, 427. \$15.00.

Analysts of the Cold War have generally found the origins of that conflict in three disputed areas of Soviet-American relations—America's use of the atomic bomb, U.S. economic policy toward Russia, and the confrontation over Soviet demands for influence in Eastern Europe. Lynn Davis focuses upon American policy as it related to the last issue, from the first stirrings of U.S. concern about applying the principles of the 1941 Atlantic Charter to the final reluctant, resentful acceptance of Soviet hegemony in this area. Davis' study was spurred, she notes, by "the simple question of who is right," the consensus or the revisionist historian, in the Cold War debate on Eastern Europe. That question remains, inevitably, unresolved by her study, which sides most often with the consensus viewpoint. But what disappoints most is how little the book adds to this debate. The first scholar to produce a book-length account of American policy toward Eastern Europe, using the extensive and recently declassified files of the State Department, Davis nonetheless offers little more than an elaboration of the familiar argument that America followed a naive "policy of principle" in attempting to get Russian acceptance of self-determination for Poland, Bulgaria, Romania, and Hungary. Not realizing that Soviet and American aims would conflict at war's end, she argues, American planners decided to defer talks with the Russians until victory over Germany had been won. What the

author sees as a U.S. "policy of postponement" concerning Eastern Europe is, ironically, in line with the revisionist critique—but with the vital difference that Davis ascribes this strategy of delay to a belief that "the Allies should fight now and negotiate later," and not to an attempt at stalling the confrontation with Russia while America's economic and atomic weapons were readied. Roosevelt's attitude at Yalta, she concludes, marked the abandonment of postponement—if not principle—in U.S. policy, with the result that Russia seemingly accepted U.S. ideas on self-determination at that conference. "Angry, shocked, and disillusioned" at later Soviet violations of the Yalta agreement, American planners (including Truman) continued Roosevelt's insistence upon the principles of the Atlantic Charter, even as it became clear that the U.S. had neither the vital interest nor the military means to prevent Soviet dominance in Eastern Europe.

Unfortunately, Davis' book passes by some of the most interesting and important questions of the Cold War, while dealing with major controversies in only a cursory fashion. This flaw reflects Davis' concern with policy but not the assumptions behind it. This also caused the author to ignore valuable evidence. The result is not only that Davis' book fails to break new ground in the interpretations of Cold War origins, but that many of her familiar conclusions remain suspect.

Chief among these is Davis' central contention that principle was the guiding motive behind American policy; here she fails to deal with the revisionist charge that—as with the policy of the Open Door—"principle" was but a disguise for self-interest without power. The author, curiously, even seems to suggest the same when she lists as last among five considerations directing State Department planners on Eastern Europe, that of a "strong personal commitment to implementation of the Atlantic Charter principles"—behind "ideas about how peace should be constructed after the war." Typical also of the limitations of her analysis is the assumption that atomic diplomacy meant only the explicit threat of the bomb's use—a narrow definition which caused her to miss the importance of the weapon in discussions of Eastern Europe at the post-war meetings of foreign ministers (and, apparently, to ignore in her research the papers of Secretary of State Byrnes on that subject).

Though Davis' study is definitive neither on the question of how the Cold War began nor on the place of U.S. policy toward Eastern Europe in its beginnings, her book is a start toward a scholarly answer to the subject. It has historiographical interest, as well, as a reaction not to

the Cold War issues directly but to the controversy that still surrounds them.

GREGG F. HERKEN
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ABRAM CHAYES. *The Cuban Missile Crisis: International Crises and the Role of Law*. Published under the auspices of the American Society of International Law. New York: Oxford University Press. 1974. Pp. viii, 157. Cloth \$5.95, paper \$1.95.

This is the first volume in a projected series, "International Crises and the Role of Law," to be published under the auspices of the American Society of International Law. Abram Chayes, now at Harvard Law School, was legal advisor to the State Department during the Cuban missile crisis, and he has been concerned with the subject ever since. The study, financed by the Carnegie Corporation and the Old Dominion Foundation (Melton), adds little new except a 1968 letter from N. A. Schlei on Justice Department activities in August 1962. For the facts, historians must still turn to Elie Abel (1966), Graham Allison's *Essence of Decision* (1971), and the memoirs of participants.

Chayes examines the role of law in three aspects of the crisis: the use of blockade rather than alternative actions; the appeal to the Organization of American States; and the approach to the United Nations. He decides that the role of law was "substantial." The process by which he reaches this conclusion is more revealing of the corruption of contemporary legal thinking than of the events of October 1962. All three aspects of the crisis were political, not legal, but Chayes smuggles in a legal element by blurring distinctions through verbal ambiguities. He rejects the sharp distinction made by the participants between policy questions and legal questions on the grounds that power is reflected in both. He rejects William P. Gerberding's suggestion that legal analysis is rationalization, "something cooked up after the event," and he rejects Robert F. Kennedy's statement that the OAS vote was political, not legal, by phrasing his discussion of this vote in terms of "authorization" and "justification." The use of "authorization," a legal term, and of "justification" rather than "rationalization" helps disguise the fact that the blockade was a political act in an area (Caribbean) and with a weapon (the Navy) where the Soviet Union could not resist except by using a different weapon (strategic) or area (Europe) which neither side wanted. Calling the blockade a "quarantine" was part of the deception since international law insists that a blockade is a political act, not a legal one. Chayes' conclusion that "'mere' justification carries greater practical im-

portance for the success or failure of great decisions than is commonly supposed by the analysts" (p. 48) neither refutes Gerberding nor supports Chayes' principal argument.

Like any honest brief for a poor case, this volume is full of inconsistencies. Chapter III ends with the statement that Article 2(4) of the United Nations Charter "was a significant factor in determining the decision against air strike or invasion and for . . . a quarantine." Chapter IV then turns to the "authorization" by the OAS, although if a "quarantine" was legal there was no need for any "authorization," and the appeal was political not legal.

Those who think this volume adds little to history will find it more valuable for evidence on the sociology of law at the highest levels of our society today.

CARROLL QUIGLEY
Georgetown University

EDWIN BICKFORD HOOPER. *Mobility, Support, Endurance: A Story of Naval Operational Logistics in the Vietnam War, 1965-1968*. Washington: Naval History Division, Department of the Navy. 1972. Pp. xviii, 278. \$4.25.

RICHARD G. HEWLETT and FRANCIS DUNCAN. *Nuclear Navy, 1946-1962*. Chicago: University of Chicago Press. 1974. Pp. xv, 477. \$12.50.

The end of World War II found the United States Navy, which had established control of the seas to an extent never before approximated, faced with the task of defining its role as an instrument of national policy. It was a crucial time when the nation was assuming broader world-wide responsibilities, and advances in technology promised to render obsolescent the weapons that had produced victory. These books reveal two ways the navy responded to the challenge and the demands made on the service. Both are written by members of the government who had access to materials that will long be denied other historians.

Admiral Hooper presents a first-person narrative derived from his experience as commander of the Service Force of the U.S. Pacific Fleet, which furnished the supplies that made it possible to sustain military operations in Vietnam. Providing logistical support for the waging of limited war in distant parts of the world was a role few in the navy had visualized, but for which many were prepared by having mastered the enormous supply problems in the Pacific during the Second World War and the Korean War. This account of a less-publicized but no less vital dimension of the Vietnam involvement possesses the advantages and shortcomings of an eyewitness account by one who

views the events with a detached perspective often lacking in autobiography.

The authors of *Nuclear Navy, 1946-1962*, both historians with the Atomic Energy Commission, have produced a detailed, heavily documented study of what amounted to one man's prevailing against the system. Although there had been an awareness at the top of the navy hierarchy of the implications of nuclear propulsion for warships, there was little awareness of how this should be accomplished. The selection of Hyman Rickover and the ways in which he overcame seemingly insuperable obstacles at virtually every stage constitute most of the story. Battling the navy, civilian firms, the Atomic Energy Commission, Congress, and almost everyone else involved, Rickover managed to supervise the development of practicable nuclear-propulsion systems, influence ship design, control the selection and training of crews, and intrude into command operations. Neither details nor personalities are lacking, with Rickover's single-minded dedication as a constant in the effort that saw thirty reactor-powered vessels at sea in 1962. The emphasis throughout is on technology, bureaucracy, and administration rather than on the strategic and tactical implications of nuclear-propelled warships.

RAYMOND G. O'CONNOR
University of Miami

CANADA

LOUISE DECHÊNE. *Habitants et marchands de Montréal au XVII^e siècle*. (Civilisations et mentalités.) Montreal: Plon. 1974. Pp. 588.

Louise Dechêne's work has already received one significant accolade: the Governor-General's Prize for History, French-language section, despite the fact that her thesis was written under the direction of Robert Mandrou of the Sorbonne and was published in France rather than in Quebec. Evidently, Québécois cultural colonialism is far from dead.

Another facet of this colonial mentality is the slavish adherence to current French forms of historical methods, producing in effect, a methodological colonialism. The author, with less rather than more success, has tried to apply to the analysis of a new society, specifically the establishment and development of Montreal in the seventeenth century, the sophisticated perceptions characteristic of Mandrou's work on prerevolutionary France. The series in which Dechêne's work appears is entitled "Civilisations et mentalités," surely an odd series for the history of a settlement in the wilds of North America. Dechêne also uses French monetary values rather than colonial ones as well as kilometers instead of the usual leagues.

The anacronisms expose the basic weakness of the work: the book is concerned with the society of Montreal looked at from France in the twentieth century rather than from the colonial society itself in the seventeenth and eighteenth centuries.

One presumes that the author's original intent was to study the "mentalities" of the social components of early Montreal along the lines developed by her thesis master, Mandrou. If this was Dechêne's purpose, then the ends have not been achieved. There are nevertheless a vast number of data in the book, much new information, and valuable and valid insights into the parts of a nascent colonial society. One reads the almost 600 pages, however, and asks oneself: what has been accomplished?

CAMERON NISH
*Concordia University,
Montreal*

RICHARD ARTHUR PRESTON, editor. *For Friends at Home: A Scottish Emigrant's Letters from Canada, California and the Cariboo, 1844-1864*. Montreal: McGill-Queen's University Press. 1974. Pp. xii, 338. \$12.00.

James Thomson (1823-95), a literate Aberdeenshire baker's apprentice, wrote the letters printed here, in most cases, to his Scottish relatives. Diaries of his travels, also included in the book, fill out the story. Thomson migrated to Montreal in 1844, where he worked at his trade for a few months before moving to southeastern Upper Canada (Ontario). There, he baked for Irish cannallers on the St. Lawrence River. In 1849, Thomson found himself in Chicago, on his way to the California gold fields where he arrived in 1850. After a visit to Scotland in 1853, he returned to Upper Canada. His only other travels took him on a misguided venture, via the Isthmus of Panama, to the Cariboo in 1862. On returning to his Upper Canadian home he successfully established himself as a farmer, a bookkeeper in a starch factory, and an elected municipal officer.

The documents give the views of a workingman and not the impressions of the usual descriptive writer. Thomson was an abstemious and devout adherent of the Free Church of Scotland and the cheering effect of his religious convictions runs through the pages. He wrote about things of which he knew and which interested him: methods and costs of travel, costs of food and equipment, details of the life of a steerage passenger, wages, and conditions of work. The documents, with Preston's introduction and editorial notes, were well worth printing.

JAMES J. TALMAN
The University of Western Ontario

ROBERT E. CAIL. *Land, Man, and the Law: The Disposal of Crown Lands in British Columbia, 1871-1913*. Vancouver: University of British Columbia Press. 1974. Pp. xv, 333.

Land, Man, and the Law, a study of government land policy in British Columbia, will be of interest to historians of public-lands policy in North America. Its section on Indian lands should evoke a broader interest.

Cail begins with a short chapter on land policy in the period before British Columbia's 1871 union with Canada. Governor James Douglas emerges as the wise legislator, hampered only by practical circumstances and a parsimonious Colonial Office. Subsequent chapters trace the story of governmental provisions for alienating agricultural and pastoral land to 1913 and are followed by chapters on surveys, mining and timber legislation, and water rights. A central section deals with the complications which arose from the "railway belt"—the land turned over to the federal government to further the Pacific railway—and the provincial government's own grants to railway companies. A final section treats government policy toward Indian land claims and reserves, still a sensitive and unresolved question. A short conclusion separates the text from excessive appendices of documents and tables.

A good thesis does not make a good book. An old thesis tends to make a bad one. Originally written in 1955 for a M.A. degree, *Land, Man, and the Law* could not be maturely revised because its author was killed in a 1958 accident. The book has many of the features which characterize a thesis—and many of the faults. It is marred by excessive legal description, by a lack of comparative perspective, and by an unconcern with politics or even administration. The author consulted only published sources. Some sections have been superseded by later research and like most theses, it makes dull reading. The book is, however, a useful synopsis of aspects of settlement, resource, and Indian land policy in British Columbia.

DOUGLAS COLE
Simon Fraser University

IRVING MARTIN ABELLA. *Nationalism, Communism, and Canadian Labour: The CIO, the Communist Party, and the Canadian Congress of Labour, 1935-1956*. Toronto: University of Toronto Press. 1973. Pp. vii, 256. Cloth \$15.00, paper \$4.50.

This timely work of scholarship makes skillful use of voluminous union records as well as of lengthy interviews with the leaders who played decisive roles in this seminal period for Canadian labor (1935-1956). Abella's detailed examination of the relations between Canadian unionists and their

American counterparts is a convincing rebuttal to earlier views that the CIO refrained from interfering with the Canadian Congress of Labor and the aspirations of more nationalistic Canada. "Right from the beginning the CIO was insistent upon showing the flag in Canada—the American flag, that is." Abella also shows how Canadians themselves, notably Charles Millard of the United Auto Workers, edged out both the more militant Canadian nationalists and the highly regarded and experienced Canadian communist labor leaders. Canadian workers seemingly cared little about foreign control of their unions so long as they achieved better working conditions.

At times I grew weary of the detailed descriptions of the interunion squabbles and yearned instead for more on the general economic picture. The study, after all, covers the last half of the depression and ends in the middle of the Cold War. Readers unfamiliar with modern Canadian history might wonder how and why leftist labor leaders gained and held such wide support in the face of persistent and ever-mounting pressure from the rest of Canadian society. This points to a different Canadian experience, one that becomes less distinctive as American unions followed their country's branch plants into Canada.

Minor cavels, perhaps, for what is an excellent study.

RICHARD WILBUR
Concordia University,
Montreal

DAVID R. MURRAY, editor. *Documents relatifs aux relations extérieures du Canada/Documents of Canadian External Relations*. Volume 7, part 1, 1939-1941. Ottawa: Department of External Affairs. 1974. Pp. xcix, 1167. \$15.00.

The Canadian series of handsome volumes containing official documents on foreign policy has settled down to a standard format that is useful for scholars. Also valuable for general reference, it now provides the file numbers for the original documents; and the indexing, editing, organization, and introduction are convenient and helpful. This seventh volume, covering relations with Commonwealth members, allies, and neutrals and also various other war-related problems that are carefully grouped topically, excludes papers concerning relations with the United States. Nevertheless, as the size of the book shows, the war had brought at least a tenfold increase in the business of Canada's Department of External Affairs. Despite the deliberate omission of American matters, this volume, covering only two years, is larger than any of its predecessors.

The dominance of the American presence for Canada is such that despite the planned exclusion, United States influence intrudes at various places. In a section relating Canada's concern about the security of cryolite mines in Greenland after the Nazi occupation of Denmark, American suspicion that this was motivated by more than strategic interest brought a virtual veto. This incident, which has been written about by the official military historians in both countries, and also by James Eayrs in his *In Defence of Canada*, provides a classic example of the way in which jealous care for national interests can generate friction, and in which relative power decides issues even between friendly countries. In this case, as in many others, though Prime Minister Mackenzie King was determined at all costs to avoid upsetting American opinion, the State Department magnified one Canadian artillery officer in mufti into a military invasion. It showed no similar sensitivity about the implications of dispatching American troops. The documents presented here do not throw new light on the origin of the concern about Greenland, or whether the concern came from Britain or from the Aluminum Company of Canada. It is unlikely that the inclusion of more low-level policy papers from the Department, which some reviewers have demanded, would have cleared that matter up. Moreover, to go into that kind of detail would require larger volumes that would be prohibitively costly.

This volume demonstrates yet another Canadian neurosis in addition to hypersensitivity about the American relationship. The documents in the series are published in French or English, whichever was used in the original in the Department. There are few in French. Quite properly for a country that strives to stress its bilingualism, titles, headings, introductions, and indexes are in both languages, but translation of everything into English or French would be wasteful. In this volume, however, as in many Canadian federal usages, the French is always placed before the English. The adoption of diplomatic usage that followed alphabetical order in either language would have reversed that order. What seems to be a gesture of Francophilism in fact may impede the use of this tool as a reference since, even for persons fully conversant in French, additional headings in a second language in a superior position tend to impede rapid access.

RICHARD A. PRESTON
Duke University

R. D. CUFF and J. L. GRANATSTEIN. *Canadian-American Relations in Wartime: From the Great*

War to the Cold War. Toronto: Hakkert. 1975. Pp. xiii, 205.

J. L. GRANATSTEIN. *Canada's War: The Politics of the Mackenzie King Government, 1939-1945*. New York: Oxford University Press. 1975. Pp. xi, 436. \$18.95.

JAMES EAYRS. *In Defence of Canada: Peacemaking and Deterrence*. (Studies in the Structure of Power: Decision-Making in Canada, Volume 6.) Toronto: University of Toronto Press. 1972. Pp. xiv, 448. \$17.50.

War has played a significant role in the evolution of Canadian-American relations. By war, I mean actual armed conflict between Great Britain (and therefore British North America, later Canada) and the United States in 1775 and in 1812, and the threat of armed conflict in 1838, 1861, 1866, and 1895. The two great wars of the present century, in which the former antagonists found themselves in alliance rather than in opposition, were even more important in determining the character of today's Canadian-American relationship. These two wars raised into bold relief the question of Canada's identity in relation to the United States and forced Canada to come to terms with its great neighbor in a way it had neither foreseen nor particularly desired. The late Harold Innis of the University of Toronto summed it up in the 1950s: "Canada moved from colony to nation to colony."

The three books under review deal with Canadian-American relations during and immediately after the Second World War. The least significant is the small volume by Cuff and Granatstein, the first a professor of American History and the second a professor of Canadian History at York University.

A collection of papers which have already appeared in various periodicals between 1969 and 1974, the book explores, although not in depth, various defense agreements between Canada and the United States—the Ordnance Department Agreement of 1917, the Hyde Park Declaration of 1941, the formation of NATO—and concludes with a warning of the dangers to Canada of seeking exemptions from American economic regulations in order to avoid the costs of national independence. The authors' conclusions are similar to those normally found among Canadian scholars of the nationalist left who argue that the principal threat to Canadian independence, since 1945, has come, not from the north but from south of the frontier.

Equally provocative and more lively is Granatstein's book on Mackenzie King, that enigmatic politician who, as Prime Minister of Canada, controlled the destiny of the country longer than any other party leader. Granatstein has not written a

biography; rather he has chosen to study certain aspects of the national effort from 1939 to 1945, examining the way in which King and the Liberal Government grappled with the basic political, financial, economic, and racial issues of the time. It has long been a popular academic sport in Canada to poke fun at King's foibles, to criticize his caution and to accuse him of failing to defend his country against encroachments from the south. It is clear enough, from a reading of Granatstein's book, that some of this criticism is justified; but it is also clear that on occasions King was more aware of the problems arising from Canada's close economic and political ties with the United States than were his colleagues. King's policies may not have been exciting or satisfying, but they were effective and successful. That is why, practically alone among wartime governments, his continued to enjoy public support after as well as during the Second World War.

Peacemaking and Deterrence is the third volume which, under the general title of *In Defence of Canada*, James Eayrs has contributed to the series "Studies in the Structure of Power: Decision-Making in Canada," sponsored by the Social Science Research Council. Eayrs' first two volumes attracted considerable attention, because they opened doors to historical sources closed to the academic community. This third volume is well up to the standard of the earlier works in terms of scholarship, mass of detail, and good, dull, academic prose.

Eayrs' book is a detailed study of Canadian government thought and action during the later years of the Second World War and the early years of the Cold War. In it the author, a professor of Political Economy at the University of Toronto, discusses the *dramatis personae*—the cabinet ministers, the civil servants, the generals, admirals, and air marshals—and the policies they espoused. He touches upon a multitude of topics, including the reorganization of the Canadian armed forces after the war, Canada's involvement with the United Nations, the Canadian role in the production of the atomic bomb, and the construction of the Mid-Canada and DEW radar lines. The book was scheduled to appear in 1966, but the stuffiness of a sensitive Department of External Affairs delayed publication until 1972. I do not think that Eayrs has much to complain about. The department has accorded him privileges other historians have not been granted, and his material is still as fresh as anything in his previous volumes. It is all here, with copious notes and references and lengthy excerpts from official documents—enormously useful to the student, but not very lively, for the point of view is pretty much that of the

department. Perhaps the officials of the department did know what was best for Canada, although some Canadians suspected that External Affairs was acting as if it were the Ottawa branch of the State Department.

GEORGE F. G. STANLEY
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Sackville, N.B.

LATIN AMERICA

ROBERT BRENT TOPLIN, edited with an introduction by. *Slavery and Race Relations in Latin America*. (Contributions in Afro-American and African Studies, number 17.) Westport, Conn.: Greenwood Press. 1974. Pp. xiv, 450. \$17.95.

The progeny from Frank Tannenbaum's seminal *Slave and Citizen* continue to be born. This collection of the hitherto unpublished results of original, often archival research into a number of Latin American societies is testimony to the rich materials available for the study of social history when the questions are sharply drawn. Editor Robert Toplin, who also has an original essay on the results of abolition for Brazilian blacks, briefly but often incisively introduces each of the fourteen essays, placing it in historiographical and comparative context. Like other recent works, these essays disagree almost unanimously with Tannenbaum. And even the single exception by Norman Meikeljohn, on the implementation of protective legislation for slaves in eighteenth-century New Granada, concedes that it was an uphill fight for crown officials to compel slaveowners to protect the slaves' humanity.

Not all Latin American societies are represented, though some are examined by more than one writer. (All but two of the authors are historians of Latin America.) Of major countries, neither Mexico nor Argentina is discussed, though Chile, the other South American society that "lost" its black population, is treated in an essay on Chilean blacks by William Sater, who emphasizes miscegenation in accounting for the disappearance. Only two Caribbean societies, Cuba and Puerto Rico, are discussed, in three essays that deal not only with slavery, but subsequent and contemporary race relations as well. The authors also make the point that the influence of the United States reinforced racial prejudice in Cuba and Puerto Rico, a point echoed in an essay on Venezuela.

Several countries have more than one essay devoted to them. Robert Conrad's piece on Brazilian slavery is a gem of interwoven fact and inter-

pretation, while Arthur F. Corwin's essay on modern Brazilian race relations is fresher and fuller than Florestan Fernandes' essay on a closely related subject. New Granada and Venezuela are discussed in two essays apiece, while Colombia is covered in a singleton by William Sharp, discussing the Chocó during the seventeenth century. Sharp's piece is valuable on the ease of manumission and the color discrimination against freedmen, but he becomes unnecessarily defensive about the paucity of slave revolts. John V. Lombardi, in writing on the abolition of slavery in Venezuela, also notes the absence of revolts over a period of some fifty years, but without any uneasiness. Thanks to Lombardi's piece and an excellent essay on racial attitudes in twentieth-century Venezuela by Winthrop R. Wright, this book expertly fills an important gap in the literature in English on slavery and race in South America.

The similarities in slavery and race relations north and south of the Rio Grande are pressed upon one by these new studies. Franklin Knight's essay notes, for example, that out of fear of slave revolts in the nineteenth century, Cuban authorities jailed visiting free Negro sailors, just as Alabama and South Carolina authorities did. Venezuela's constitutions have long prohibited the immigration of free blacks, a discrimination that even the United States has not practiced; Cuba's denominating Asian immigrant laborers as "whites" is less restrictive racially than United States policy, though no less anti-black. At the same time, the greater prevalence of miscegenation in Latin America and the absence of legal segregation certainly set apart Latin American race relations from those of the United States. Race prejudice is absent from no society in the western hemisphere, and as one essay points out, not even from Castro's Cuba. Yet the manifestations and virulence of prejudice against color certainly vary from society to society and even from period to period in the same society.

This book shows that the sharp contrast between North and South American race relations that was Tannebaum's contribution has now been transcended. Diversity in race relations among all New World societies within a general context of anti-black prejudice is the new and difficult challenge to historical and social explanation.

CARL N. DEGLER
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GABRIEL DEBIEN. *Les esclaves aux Antilles françaises (XVII^e-XVIII^e siècles)*. Basse-Terre: Société d'Histoire de la Guadeloupe; Fort-de-France: Société d'Histoire de la Martinique. 1974. Pp. 529.

Gabriel Debien's latest study on slaves in the French West Indies during the seventeenth and

eighteenth centuries makes a fitting climax to his long career of scholarly research based on plantation records. All his readers have been waiting for this book, and it will not disappoint his many admirers. Debien himself has almost disarmed criticism by very honestly pointing out in his introduction the main defects of the work, which are, in effect, the limitations of his essential sources—the plantation papers found in public and private archives.

These papers supply the basis for his research on eighteenth-century conditions among the slaves, especially at Saint-Domingue, where the documentation is richest. He has supplemented them with planters' guides, contemporary travel books, and other descriptive accounts. For the seventeenth century, he has not neglected the invaluable help of Dutertre and Labat, and he has also used those records of the administration which throw light on the state of the slave population in the Antilles.

From these sources, Debien has brought together a wealth of detail about the lives of the plantation slaves, ranging from their African origins, through their seasoning, to the various ranks of slaves and their cadres of management. He discusses the slaves' material conditions, their work, food, housing, and clothing, their health, high death rates, and low rates of natural reproduction, their religious state, their responses to slavery, including running away and other forms of resistance, their manumissions, and finally the question of whether their lot in life was ameliorated in the later eighteenth century.

Although most of these subjects have been discussed before, Debien has made a unique contribution by his tireless search through the plantation records. When he writes that about half of the new slaves died in the seasoning, we know that this conclusion results from careful statistical analysis. When he notes that underfeeding was the great shame of the colonial regime, and a great cause of running away, we know that this is what the plantation papers reveal. When he concludes that amelioration of slave conditions was dominated by considerations of revenue, we know that his findings are based on discriminating assessment of the evidence. While this is the great merit of all his work, it also accounts for some important omissions, particularly his failure to examine retention of African religious practices among the slaves. Where so much has been brought to light, however, what we now know with certainty in part compensates for Debien's characteristic reluctance to venture beyond those areas of research which his documents can readily illuminate.

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NICOLAS CHEETHAM. *New Spain: The Birth of Modern Mexico*. London: Victor Gollancz Ltd.; distributed by International Publications Service, Collings Inc., New York. 1974. Pp. 336. \$15.00.

This work, written by a British scholar and diplomat, is in this reviewer's opinion, the best synthesis of early Mexican history in English. The author has digested, synthesized, and organized most of the relevant secondary and some primary source material. He is familiar with and makes excellent use of both English- and Spanish-language material. Yet, despite the general excellence and readability of the book, he provides no new material for professional historians interested in the history of Mexico.

The work is divided into five parts. The sections include pre-Colombian nations, the conquest, the early period of Spanish rule under Cortés, viceregal government, and the attempts of the Spanish government and church authority to bring about a cultural, social, and political integration of the Indian subjects. The weakest chapters are the early ones on pre-Colombian civilization and the conquest. Much of this material has been covered in greater detail in other works. Reading Cheetham's book is no substitute for reading Bernal Díaz's account or Robert Padden's study of several years ago. The section on Spanish colonial government and the Spanish attempts at cultural integration contains much material that is otherwise unavailable to the lay reader. The author not only uses the most recent scholarly material to explain this period, but he uses it in a remarkably exciting way. His approach to the Aztec government and that of the Spanish colonial authority is even-handed and scholarly. His general interpretation is obviously influenced by Hanke, Gibson, and Ricard.

This is a very welcome and useful edition to the general literature on New Spain. I would recommend it to anyone going to Mexico or to anyone with a general interest in Mexican history, colonialism, or the anthropology of indigenous peoples. It would be very helpful for this work to be made available in paperback so that it could be used in general courses on Mexican and Latin American history.

ALBERT L. MICHAELS
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Buffalo

J. I. ISRAEL. *Race, Class and Politics in Colonial Mexico, 1610-1670*. (Oxford Historical Monographs.) New York: Oxford University Press. 1975. Pp. xiii, 305. \$25.00.

We are gradually learning more about the seventeenth century in Mexican history, and with this

impressive contribution by J. I. Israel scholars have a major contribution in both information and interpretation. The terms race, class, and politics in the title convey only a partial idea of the book's intricate and intermeshing topics, which include ecclesiastical hierarchies, the seventeenth century's economic crises, illegal profit-making by viceroys, and many others. With respect to the title the dates 1621-1664 more accurately reflect the book's contents than 1610-1670.

Race and class are first analyzed in separate chapters on Indians, Spaniards, and others, with a valuable chapter on some minority groups that have been generally overlooked: Basques, Portuguese, Italians, and Jews. The author then moves to the section on political life, especially the upper-level political life of the viceregal capital. Here the controversies that so frequently disturbed the life of the colony, together with the rumors, threats, scandals, and related incidents, are recounted in detail. Not since H. H. Bancroft—who receives praise as a "great" historian—has this kind of material been taken so seriously. But what Israel does that Bancroft failed to do is to impose order and system on a history that has seemed to most students a miscellany of unrelated or superficial squabbles. Conflicts between viceroy and archbishop are shown to have an unexpected continuity over time, with the secular clergy, largely Creole, functioning as the viceroy's opposition. The tumult of 1624, which a generation of historians, following C. L. Guthrie, has attributed to shortages and high prices, is assigned here to political causes. The development of topics in the local Mexican scene is clarified, wherever possible, by reference to the particular circumstances of the parent country. Thus readers learn, for the first time so far as this reviewer is aware, of the connection between the Portuguese independence movement of 1640 and the rivalry in Mexico between the Duque de Escalona and Juan de Palafox y Mendoza. There are many other examples.

In recent decades the tendency among historians of Mexico has been toward social, socioeconomic, or even anthropological interpretations. Israel's is a socio-political interpretation, with emphasis on the political, and its revisionary implications are far-reaching.

CHARLES GIBSON
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A. J. R. RUSSELL-WOOD, editor. *From Colony to Nation: Essays on the Independence of Brazil*. (The Johns Hopkins Symposia in Comparative History, volume 6.) Baltimore: The Johns Hopkins University Press. 1975. Pp. xi, 267. \$12.50.

The years 1750-1822 were of decisive importance for Portugal and Brazil, beginning with the ascendancy of the strong-minded reformer and fu-

ture Marquis of Pombal and ending with political separation. On the sesquicentenary of Brazilian independence an international symposium of scholars met at Johns Hopkins University to reassess the preconditions and meaning of Brazilian independence. Of the eight papers published here, three have already appeared elsewhere.

The stage is set with an overly long, somewhat repetitive introduction by A. J. R. Russell-Wood, well-known Brazilianist and organizer of the symposium, who analyzes the nature of "the colonial pact" between Brazilians and the metropolitan regime and provides a detailed survey of the socioeconomic development of late colonial Brazil. Emilia Viotti da Costa examines the breakdown of the colonial pact during the Court's thirteen-year residency in Brazil (1808-1821). As she perceptively demonstrates, Regent and later King John VI was torn between a desire to dismantle long-standing restrictions upon Brazilian economic activities and a need to safeguard traditional Portuguese interests in his nation's most valuable colony. Another Brazilian scholar, Maria Odilia Silva Dias, emphasizes that the independence movement was neither revolutionary nor nationalistic and did not produce a national consensus until mid-century. Stanley E. Hilton attacks the not very widely held "myth" that between 1808 and 1824 Brazil and the United States shared common ideals and values by demonstrating that the contrary was true.

The next pair of essays focus upon social themes. Stuart B. Schwartz contends that after 1750 the social structure of Brazil became increasingly complex, in part because of the growth of an independent peasantry, and that a major task of the Brazil-

ian empire after 1822 was to find a satisfactory formula "to accommodate the social tensions created in the late colonial era" (p. 134). In a characteristically graceful and informative paper Richard M. Morse tells us much about the urban development of Brazil from the sixteenth to the late nineteenth century but nothing about the communities' involvement in the imperial crisis of 1820-1822.

The last essays are concerned with intellectual topics. In his elegantly written study Manoel Cardozo analyzes the significance of the passing of the Baroque mood in Portugal and seemingly believes that Brazilian independence was hastened by the declining influence of the Church and the nobility and by unwise Luso-Brazilian acceptance of untried liberal dogmas. In an equally well-documented essay E. Bradford Burns assesses the influence of Brazilian urban intellectuals who were leading advocates of change. Well read though they may have been, it is difficult to see from Burns' discussion how such a group helped engineer Brazil's independence.

It is unfortunate that the editor did not indicate major areas of agreement and divergence among his authors or summarize ways by which our knowledge of the causes and consequences of Brazilian independence has been advanced by these essays. It is likewise regrettable that the essayists did not examine more closely internal developments within Portugal between 1807 and 1820 or the impact upon Brazil of revolutionary movements in Spanish America and in Europe. A useful but not vital volume.

DAURIL ALDEN
University of Washington

Communications

A communication will be considered only if it relates to an article or review published in this journal; publication of such a communication or of any part of it is solely at the editors' discretion. Limitations of space require that a communication concerning a review be no longer than the review to which it refers and in no case longer than 500 words. Communications concerning articles or review articles may be no more than 1,000 words, and the editors reserve the right to impose a lower limit. The schedule of publication and the time needed to send a communication to the author of the article or review in question for such reply as he may care to make virtually preclude the possibility of publication in the issue following that in which the original article or review appeared. Unless, in the editors' judgment, some major scholarly purpose is served, rejoinders will not be published.

TO THE EDITOR:

In her review of Fawn Brodie's, *Thomas Jefferson, an Intimate Portrait* (December 1975), Lois Banner confines herself to discussing the author's "highly speculative" psychological analysis. Opinions may vary on the merits of such speculation; but we should look also at the author's treatment of the facts of Jefferson's life. Facts can be judged objectively. Furthermore, speculation that disregards facts is hardly worthy of serious discussion. If her treatment of the period June–September 1776, with which I am familiar, is representative, Fawn Brodie's performance is appalling:

"We do not even know for certain if Jefferson signed [the Declaration] on the second of July, when the Declaration was formally voted, or on the fourth, as he later insisted" (p. 124). As Jefferson's Declaration of Independence was "formally voted" on the fourth, no one could have signed it on the second. Fawn Brodie has confused July 2, the day on which the Lee resolution passed, with August 2, the day the Declaration was signed. The uncertainty to which she refers is over whether the Declaration was signed on July 4, or only on August 2.

"But Jefferson had enough of anxiety. Without waiting to the end of the year, as he had earlier promised, he resigned his seat in the Congress on September 2, 1776 . . ." (p. 126). The year referred to is not the calendar year, but the year of Jefferson's term as a delegate. It expired in early August, and he planned to leave on the eleventh. Fawn Brodie thinks that Jefferson left Philadelphia four months early, when, in fact, he stayed an extra three weeks.

JAMES MUNVES
New York, N.Y.

FAWN BRODIE REPLIES:

The error pointed out by James Munves concerning the date the Declaration of Independence was signed has long since been corrected in my volume. The facts were originally written correctly, but in repeated typings and revisions the error crept in and was not, alas, caught before the first printing.

As for the second error, let me point out that Jefferson wrote to Edmund Pendleton June 30, 1776, "I shall with cheerfulness continue in duty here till the expiration of our year. . . ." Munves is correct in interpreting our year to mean "the year of Jefferson's term as delegate," not the calendar year. If I have made errors no more "appalling" than this I shall count myself more fortunate than most authors.

TO THE EDITOR:

As coauthor of the U.S. Army's history of the China-Burma-India Theater of World War II, I am taken aback by some of the assertions and suggestions of Walter La Feber in his "Roosevelt, Churchill, and Indochina: 1942–45" (December 1975). If one may judge by his footnotes and text, he did not familiarize himself with the 1944 Chiang–Roosevelt correspondence, nor with the chronology and course of operations in China and

Burma that same year, nor yet with the full U.S. response to the 1945 Japanese seizure of Indo-China. For 1944, he has relied heavily on a December 4, 1944, memo from Roosevelt to an Admiral Brown. The results are not happy. La Feber writes (p. 1287):

By the summer of 1944 the president's Indochina plans were in danger. British pressure, French determination, and State Department opposition were building, *but the immediate threat arose from a crisis in China* [emphasis added]. During the spring, the Japanese attacked Stilwell's [sic] forces, gravely endangering the entire Allied position in South China. Roosevelt pleaded with Chiang to use the ten [sic] American-equipped divisions in Yunnan. . . . *On April 3, as the situation worsened* [emphasis added], Roosevelt again implored Chiang to move against the Japanese. The Chinese leader never acknowledged the note. As a White House analysis of this affair concluded [sic], "there was no solution to this impasse and the President stopped trying to prod Chiang Kai-shek into action."

One may note that the Japanese offensive in East China, ICHIGO, began April 17. It could not have affected FDR's April 3 message. FDR's pleas to Chiang to use his U.S. equipped Yunnan divisions in Burma actually began March 17, 1944. Roosevelt discussed the current Japanese offensives against the British IV and XXX Corps in Manipur State and XV Corps in the Arakan plus Stilwell's successes in North Burma. Roosevelt saw all this as opening the way for Chiang's Yunnan divisions to cross the Salween into Burma to help end the Japanese blockade of China. FDR's April 3 again urged Chiang to seize the opportunity FDR saw in Burma. Neither message discussed a crisis in China. As yet, there was none.

Though one may write that Chiang never acknowledged Roosevelt's April 3 radio, it is not correct to suggest he did not comply with FDR's request. On April 10, General George C. Marshall made explicit a hint in the April 3 message that there would be no more lend-lease for the Yunnan divisions if they did not attack into Burma. Stilwell's chief of staff so informed the Chinese. On April 14 the Chinese Army's chief of staff, Gen. Ho Ying-chin, agreed to attack. The Chinese made good on his promise May 11 and began the 1944-45 Salween Campaign. This act was Chiang's response to FDR's April 3 radio.

La Feber next deals with Roosevelt's attempt to persuade Chiang to let Stilwell command the armies of the Central Government and of the Communists. He writes (p. 1288): "In mid-summer Roosevelt asked that Stilwell be given control [sic] of China's armies in the war area [sic]. Chiang again refused to reply directly." FDR's request was delivered to Chiang on July 6. Chiang's immediate, direct reply was sent July 8. The full texts,

precisely cited, were published in 1956 in *Stilwell's Command Problems* (pp. 383, 385). Today the originals are in the National Record Center and the National Archives, respectively. The correspondence which followed is dealt with in *Stilwell's Command Problems*.

Finally, I cannot reconcile La Feber's suggestion on pages 1294-5 that "Roosevelt was finally defeated . . . by his own advisers and the Chinese crisis of 1944. On the Indochina issue he utterly failed as head of government, for he proved unable to impose his policy upon either the State Department or the military." This does not accord with the record. As I have indicated, La Feber does not grasp the course or the content of the China crisis of 1944. Regarding FDR's control of the military concerning Indochina I cite but one example.

Though La Feber describes Roosevelt in March 1945 as resisting "for several days" (p. 1293) French pleas for air support in Indochina, he shows no awareness that in April, a full month later, both the Joint Chiefs of Staff and the U.S. theater commander for China, Lt. Gen. A. C. Wedemeyer, were asking FDR if anyone, British or American, could give air support to the French (*Time Runs Out in CBI* [pp. 259-61]). This, after Roosevelt had allegedly surrendered to a military desire to support the French! And this request for orders from the men Roosevelt allegedly could not control! This exercise of civilian control must have been one of Roosevelt's last acts.

RILEY SUNDERLAND
Bar Harbor, Maine

WALTER LA FEBER REPLIES:

I value the attention Riley Sunderland has given my essay, for like many other scholars I have admired and benefited from his past work. Of course I consulted both *Stilwell's Command Problems* and *Time Runs Out in CBI* before writing the essay, and I have seen the FDR-Chiang correspondence. Indeed, I examined most of that correspondence at the Roosevelt Library where the staff was good enough to declassify documents from the exchanges as I worked. On the other hand, as far as I can tell, Sunderland's volumes made little use of the FDR Library materials, relying largely on Roosevelt's messages to Chiang that were sent only through U.S. Army channels (*Time Runs Out in CBI*, [p. 16]). Sunderland's work could not exploit British Foreign Office documents, sources vital to my argument and essential to any work on Roosevelt or on general diplomacy and military strategy in these years. With these factors in mind, and having reconsidered the evidence, I see no reason to retract any part of my essay.

Riley Sunderland's detailed criticisms will be dealt with below. More important, we should be

clear that fundamental issues of American policy in 1944-45 are at stake in this exchange. The most important part of his attack, therefore, is in his final two paragraphs, not in the first paragraph he selected from my essay. (In fact, attacking this paragraph from my essay makes little sense, for it could have been reduced to the single irrefutable sentence, "by early autumn of 1944 FDR no longer had confidence that Chiang could act as the policeman for American interests in Asia," and not one other word of my thesis would have had to be changed.) Sunderland apparently does not believe, for example, that the Chinese crisis was a major reason why Roosevelt had to ditch his plans for Indochina. Yet Sunderland cannot question my assertion that before 1944 the president looked to China as the policeman for Indochina. Nor can he question Roosevelt's turn against Chiang after the 1944 Sino-American crisis and the concomitant turn in FDR's Indochina policy.

In his final paragraphs Sunderland argues that Roosevelt did impose his Indochina policies on the U.S. military. Two points should be made. First, Sunderland's last assertion (that the military chiefs asked FDR for orders in April) is irrelevant. I never claimed the chiefs did not, nor is the point interesting, for the crucial decision to aid the French was made in January and repeated in March, as noted in my essay. It would have been helpful if Sunderland had discussed Roosevelt's decisions in January and March in *Time Runs Out in CBI* on pages 259-61; unfortunately, he did not.

Second, I know of no scholar who, after examining the evidence, has argued that Roosevelt imposed his trusteeship policy for Indochina on the U.S. military. I cited Secretary of War Stimson's statement to Roosevelt on November 17, 1944, (again found in the FDR Library) to support my point. Scholars may argue, but I prefer the Secretary of War's evidence in 1944 to Sunderland's assertion of 1976. Moreover, and of primary importance, Sunderland cannot contradict my argument that the Joint Chiefs' determination to gain sole control of Japanese-held bases in the Western Pacific after the war helped undercut Roosevelt's hope for a meaningful trusteeship plan. If the Americans would not tolerate an international trusteeship agency overseeing these areas, Roosevelt could hardly have argued that the French should allow such an agency into Indochina. The British, among others, fully appreciated at the time the effect the U.S. military policy would have in weakening Roosevelt's trusteeship plans.

Finally, Sunderland's more detailed, if less important points can be considered. First: The December 4, 1944, memo from FDR to Admiral Brown on the Chinese situation is a crucial document, and one I could not find cited in Sunder-

land's two volumes on this subject. A long account of the Sino-American crisis of 1944, the document is critical since it was drawn up by George Elsey and represented the White House view of the crisis. Roosevelt read it and wrote "excellent" on the report. My essay attempted to see Asia from FDR's viewpoint; for this reason, the document is more important for the understanding of the president's policies than histories which apparently do not use the document.

Second: The Japanese offensive referred to in my essay did not begin April 17 but with the attack on Imphal in March (see *Stilwell's Command Problems*, [pp. 191 ff.]).

Third: The resulting crisis for Stilwell and the overall American position in CBI is well described in Barbara Tuchman, *Stilwell and the American Experience In China* (particularly pp. 439-43), in *Stilwell's Command Problems* (pp. 174-75, 191 ff.), and especially in the Elsey memorandum of 1944.

Fourth: To claim that Chiang complied with Roosevelt's plans simply by sending men across the Salween is a gross exaggeration. The thinness of these forces is described in Tuchman (p. 452). Roosevelt had much more in mind than this response by Chiang, and Elsey's memorandum accurately characterized Chiang's foot-dragging.

Fifth: Sunderland thinks Chiang's reply of July 8 was a "direct" answer to FDR. This view, I fear, results from a misunderstanding of what I meant by "direct." I meant that Chiang refused to tell FDR either yes or no. The reply was a masterpiece of prevarication; it held out the carrot of unspecified cooperation while sidestepping the entire issue by asking for "an influential personal representative." Sunderland noted in 1955, when describing this exchange: "Later events suggest that the Generalissimo resolved that Stilwell should on no account hold command in China Theater, and that he rallied all his diplomatic resources to the task of avoiding any such outcome of the crisis" (*Stilwell's Command Problems* [p. 385]). I find his 1955 views compelling.

In conclusion, therefore, I reaffirm the points of my essay: Roosevelt's necessary change of policy on Indochina in late 1944 and early 1945, his growing break with Chiang as a cause of that change, and his failure to impose his original trusteeship idea on pivotal Washington officials. On these issues I find no reason to accept Riley Sunderland's views.

It must further be suggested that if my interpretation of Roosevelt is to be challenged, at least use must be made of both the extensive sources in the Roosevelt Library (especially Elsey's summary stressing the developing crisis in Sino-American relations before the Japanese offensive of April 17) and the British records of FDR's conversations and actions.

Recent Deaths

VERNER WINSLOW CRANE, emeritus professor of history in the University of Michigan, died at Ann Arbor on December 11, 1974, at the age of 85. Born at Tecumseh, Michigan, into a family devoted to scholarship—the late Ronald S. Crane was his older brother—he obtained his baccalaureate degree from the University of Michigan and was elected to Phi Beta Kappa. He was a graduate student at Harvard and at the University of Pennsylvania, from which he received his doctorate in 1915. Appointed as instructor in the following year at Michigan after postdoctoral study, he served in that institution during his entire teaching career except for a decade after 1920, when he joined the staff of Brown University. He returned to Ann Arbor in 1930 to succeed his mentor and friend, Claude H. Van Tyne. He was a visiting lecturer at Harvard, Brown, and University College, London. During the years 1928–34 he was a member of the editorial board of the *Review* and later served in a similar capacity for the *Mississippi Valley Historical Review*. He achieved eminence as a scholar with the publication of his pioneering study, *The Southern Frontier, 1670–1732* (1928).

Acquiring an international reputation as a specialist in colonial American history, Crane devoted more than four decades to research on Benjamin Franklin, publishing *Benjamin Franklin, Englishman and American* (1936), *Benjamin Franklin and a Rising People* (1954), and his edition of *Benjamin Franklin's Letters To The Press, 1758–1775* (1950), in which he added substantially to the Franklin canon by identifying pseudonymous writings. It is hoped that a manuscript concerning the circle of Franklin and his friends in London, which he had virtually completed before his death, will soon be published.

There was in Verner Crane an element of the careful antiquarian befitting a historian whose researches were concentrated in early American history—he was a corresponding member of the Colonial Society of Massachusetts and an honorary member of the Old Colony Historical Society, as well as an active member of the Board of Governors of the William L. Clements Library. But his interests were broad, his outlook generous. A most

meticulous scholar, he did not neglect the teaching of undergraduates. He excelled in the direction of graduate students. He was in his mature years a person of dignified appearance, and his piercing gaze—in part a product of his spectacles—was most disconcerting to students who had done less than their full duty. He was highly respected by his colleagues because of his maintenance of high standards and his devotion to principle, hence his appointment in 1958 as Henry Russel lecturer, the highest honor bestowed by the University of Michigan faculty upon one of its members. Not so well known was his unfailing kindness, especially to those in whom he placed trust, for he did not seek popularity. Always a gentleman, he possessed a lively sense of humor, and he dearly loved an amusing anecdote. He was gracious and witty in the last days of his final illness.

JOHN R. ALDEN
Duke University

The death of CONSTANCE McLAUGHLIN GREEN on December 5, 1975, at the age of 78, takes from the historical profession one of its more delightfully colorful members. The daughter of Pulitzer Prize winning A. C. McLaughlin of the University of Chicago, Connie Green, as she was known to her large circle of friends, turned to history belatedly. After raising three children, she completed her doctorate under the direction of Ralph Gabriel at Yale where she published a seminal dissertation, *Holyoke, Massachusetts: A Case History of the Industrial Revolution in America* (1939), her initial foray into urban history. She was teaching at Smith College when the coming of World War II led her to accept a post as historian of the Army Arsenal at Springfield, Massachusetts. Later she moved to Washington where she collaborated in writing the official history of the U.S. Army Ordnance Department.

Connie Green's gracious eighteenth-century home directly behind the Supreme Court building in Washington soon became a gathering place for visiting historians. As she put it, the old house had "classical frontage and Bohemian rearage." But

whatever the prospect, one was sure to find there both generous hospitality and, even more rewarding, a lively salon where Connie herself presided over the exchange of ideas with wry good humor and trenchant comment.

In 1964 Connie Green matched her father's performance when she too won a Pulitzer Prize with the first of her two-volume social history of the District of Columbia, *Washington, Village and Capital, 1800 to 1878* (1962-64). From this triumph she went on to break new ground with *The Secret City: A History of Race Relations in the Nation's Capital* (1967). Her success in tackling urban history did not, however, lead her to abandon her long-standing interest in the history of technology. When nearly 70 she responded to a request from the National Aeronautics and Space Agency and plunged into the intricacies of the space age to write, with Milton Lomask, *Vanguard, A History* (1971), an account of the nation's first satellite. For the advocates of second careers for women after raising families, Connie Green's performance will stand as vindication and inspiration.

I. B. HOLLEY, JR.
Duke University

The death of WILLIAM GREENLEAF on December 17, 1975, at the age of 58 saddened the lives of colleagues, students, and scholarly associates alike. Born in Brooklyn, New York, he was educated in the New York public school system and the City College where he early demonstrated his exceptional proficiency as a writer. His master's essay and Ph.D. dissertation, both written at Columbia University, revealed an extraordinary combination of talents in research, analysis, and writing. The dissertation, a study of the Selden Patent Suit, litigation so central to the automotive industry, was a model in its comprehension of both the technology and the intricate legal issues involved. Subsequently published under the title *Monopoly on Wheels* (1961), it is regarded as one of the outstanding dissertations of the Columbia History Department. A research associate of the Ford Motor Company History Project directed by Allan Nevins, he collaborated on the multivolume biography of Henry Ford. He joined other historians in preparing several editions of the *Encyclopedia of American History* (1953-76), a documentary history, *The Rise of Industrial America, 1840-1900*, and a survey text, *U.S.A.: The History of a Nation*. At the time of his death, William Greenleaf was busily engaged in two disparate projects, testimony to his versatility as writer and historian—a history of the Civil War in the North for the *New American Nation Series* and a study of the early history of the motion picture industry in America.

Greenleaf's integrity as a scholar was matched by his generosity of time and assistance to others and the warm and self-effacing quality which marked his relations with students and colleagues both at Colorado State University, where he served briefly, and at the University of New Hampshire where he taught since 1958. There, in addition to earning a reputation as an outstanding classroom interpreter of recent and contemporary United States history, he initiated seminars and colloquia on various aspects of the economic history of the New Deal while carrying concurrently an active graduate program. In recent years he served as New Hampshire's representative on the University Press of New England. He is survived by three sons, Peter, Eric, and Allan, to whom his devotion is legendary.

RICHARD B. MORRIS
Columbia University

S. HARRISON THOMSON, editor, scholar of international renown, dedicated teacher, and leader in the historical profession, died at the age of 80 in Boulder, Colorado, on November 19, 1975. Born in California, he pursued his studies in modern languages, literature, philosophy, theology, and history at Princeton, Oxford, and Charles University in Prague, Czechoslovakia. Prior to his professorship in medieval history at the University of Colorado, Boulder, where he taught from 1936 until 1964, Thomson had teaching positions at Princeton, California Institute of Technology, and the University of Chicago. He was also visiting professor at Indiana University (1950-51) and the University of Washington (1964-65) and held the Duke Chair of History at UCLA (1965-66).

Harrison Thomson's contributions to Latin paleography and to the history of the late middle ages and the Western Slavs include numerous articles, reviews, and books. Of particular significance are his independent commentaries and text editions *Magistri Johannis Hus Tractatus Responsivus*, *Magistri Johannis Wyclif Summa de Ente Tractatus Primus et Secundus*, *The Writings of Robert Grosseteste*, and *Magistri Johannis Hus Tractatus de Ecclesia*. His pioneer study *Czechoslovakia in European History* (1943) won him the State Prize of the Czechoslovak Republic in 1944, while *Europe in Renaissance and Reformation* (1963) appeared also in a German edition (1969). For the *Latin Bookhands of the Later Middle Ages* (1969), he was awarded the Haskins Medal of the Mediaeval Academy of America, and the American Association for the Advancement of Slavic Studies honored him with its Award for Distinguished Contributions to Slavic Studies in 1973.

In 1936, Thomson assumed the editorship of the *Progress of Medieval Studies in the United States and*

Canada. Five years later, he founded the *Journal of Central European Affairs*, the first English-language periodical devoted primarily to the specific problems of the area between the Russian and German lands. In 1943, he started *Medievalia et Humanistica*, editing, with the help of his wife, three periodicals for nearly a quarter-century. He also served on the editorial board of the *Slavic Review*.

As editor of important scholarly publications and with his meticulously researched writings set forth in an urbane yet forceful style, Thomson was a source of inspiration to both the established member of the profession and the novice. In his system of values scholarship ranked high, even though he was humbly aware of its limitations *sub specie aeternitatis*. Yet he took pride in his students' achievements, educating them with tact and example to become worthy of the name of historian. A master of classic and medieval Latin and half-dozen Indo-European languages, he felt challenged to learn Magyar in his late sixties.

Harrison Thomson left the privacy of his study in Boulder for frequent visits to archives and libraries all over the Western world. A leading expert on the religious and social turbulence of heretic movements, he could spend countless hours deciphering the handwriting of a scribe to determine the cultural milieu of a monastery. An admirable sense of proportion and catholicity of view in matters of learning made Thomson a respected cultural representative of the United States in Prague and Warsaw in 1945 and 1946 even while his international reputation as a research scholar continued to grow. He was a member of the Institute for Advanced Study at Princeton, a fellow and later first vice-president of the Mediaeval Academy of America, served as chairman and vice-chairman of the Western Slavic Conference, and actively participated in the affairs of the American Historical Association, the History of Science Society, the American Society for Church History, the Renaissance Society of America, the American Council of Learned Societies, and the Social Science Research Council. He was also an associate of the Société Philosophique de Louvain, corresponding fellow of the Polish Academy of Sciences in Cracow, honorary fellow of the Czech Academy of Arts and Sciences, a member of the Society for the Study of Medieval Languages and Literatures and the Anglo-Norman Text Society, both in Oxford, and a corresponding fellow of the Polish Academy of Arts and Letters in London.

To those former students and colleagues who knew him, Harrison Thomson was a reliable and humane friend. But the spiritual legacy he left goes beyond the boundaries of personal friendship. Indeed it represents American scholarship at its best, because it reflects the search for truth of a probing

mind and the humanitarian values of a great historian.

GEORGE BARANY
University of Denver

SIR JOHN W. WHEELER-BENNETT, GCVO, CMG, OBE, FBA, died on December 9, 1975, in St. Thomas Hospital, London at the age of 72. His passing brings to an end a life that not only spanned, but also was often intimately connected with the tumultuous events of the first three-quarters of the twentieth century. He seemed always to be in the right place at the right time. He was in China in the early 1930s; he left Berlin for Prague the day of the "night of the long knives," having just dined with some of its prominent victims; and he interviewed Kaiser Wilhelm II and Leon Trotsky shortly before their deaths. Accounts of some of these encounters appeared in an anthology of Wheeler-Bennett's historical studies, *A Wreath to Clio* (1967). During his long and active career he was attached to the British Library of Information, British Press Service, British Political Warfare Mission in the United States, and Allied Military Government in Europe. A close associate of Winston Churchill, Anthony Eden, and Harold MacMillan, Sir John edited *Action This Day: Working with Churchill* (1968), a collection of excerpts from pertinent memoirs. In 1945 he married a gracious Virginia woman, Ruth Risher.

Wheeler-Bennett was an Honorary D.C.L. of Oxford University, an Honorary Fellow of St. Anthony's College, Oxford, and an Honorary D.Litt. of New York and Birmingham Universities. He was knighted in 1959 for his *King George VI: His Life and Reign* (1958) and served as Historical Advisor to the Royal Archives until his death. He taught at Oxford University, New York University, the Universities of Virginia and Arizona, and other important institutions in Europe and the United States.

Although Sir John considered himself a German historian, he was a recognized expert on international relations as well. His first major book was *Wooden Titan: Hindenburg in Twenty Years of German History, 1914-1934* (1956), but he himself and many others judged the next, *The Forgotten Peace, Brest-Litovsk, March, 1918* (1939), to be his finest effort. Other important works included *Munich: Prologue to Tragedy* (1948), *Nemesis of Power: The German Army in Politics, 1918-1945* (1954), *John Anderson, Viscount Waverly* (1962), and *The Semblance of Peace: The Political Settlement After the Second World War* (1972). Sir John also published numerous articles, some of them dealing with the American Civil War, which was a topic of abiding interest to him, in *Foreign Affairs*, *Virginia Quarterly Review*, *History Today*, and other journals.

Those of us who studied under Sir John and

knew him, respected him as a scholar, venerated him as a teacher, and cherished him as a friend. He was exemplar, counselor, and "living history" to us. Wherever Sir John and Lady Ruth happened to be living there was a warm welcome with good company and continuing inspiration for us.

He was deeply loved, and the absence of his commentary, wit, and consideration will leave the profession and us all the poorer.

DENNIS REINHARTZ
*University of Texas,
Arlington*

Other Books Received

Books listed were received by the *AHR* between January 1 and March 1, 1976. Books that will be reviewed are not listed, but listing does not preclude subsequent review.

GENERAL

- BAKER, DEREK, editor. *The Materials, Sources and Methods of Ecclesiastical History: Papers Read at the Twelfth Summer Meeting and the Thirteenth Winter Meeting of the Ecclesiastical History Society*. (Studies in Church History, volume 11.) New York: Barnes and Noble. 1975. Pp. xii, 370. \$20.00.
- BANKS, ARTHUR. *A Military Atlas of the First World War*. With commentary by ALAN PALMER. New York: Taplinger-Publishing Company. 1975. Pp. xii, 338. \$29.95.
- BIRNBERG, THOMAS B., and RESNICK, STEPHEN A. *Colonial Development: An Econometric Study*. (Economic Growth Center, Yale University.) New Haven: Yale University Press. 1975. Pp. xiii, 347. \$20.00.
- BLUMENSON, MARTIN and STOKESBURY, JAMES L. *Masters of the Art of Command*. Boston: Houghton Mifflin Company. 1975. Pp. xiv, 393. \$12.50.
- CROSLAND, MAURICE, editor. *The Emergence of Science in Western Europe*. New York: Science History Publications. 1976. Pp. 201. \$18.00.
- DAVIS, DAVID BRION. *The Problem of Slavery in the Age of Revolution, 1770-1823*. Ithaca: Cornell University Press. 1975. Pp. 576. \$5.95.
- DJUVARA, NEAGU M. *Civilisations et lois historiques: Essai d'étude comparée des civilisations*. Paris: Mouton. 1975. Pp. 448, 65fr.
- DOBB, MAURICE, et al. *The Transition from Feudalism to Capitalism*. With an introduction by RODNEY HILTON. (Foundations of History Library.) Atlantic Highlands, N.J.: Humanities Press. 1976. Pp. 195. \$12.50.
- DUFF, WILSON. *Images Stone B.C.: Thirty Centuries of Northwest Coast Indian Sculpture*. Photographs and drawings by HILARY STEWART. Seattle: University of Washington Press. 1975. Pp. 191. \$17.50.
- EHRENBERG, RALPH E., editor. *Pattern and Process: Research in Historical Geography*. (National Archives Conferences, volume 9.) Washington, D.C.: Howard University Press. 1975. Pp. xv, 360. \$15.00.
- EISENSTADT, S. N., and AZMON, Yael, editors. *Socialism and Tradition*. (The Van Leer Jerusalem Foundation Series.) Atlantic Highlands, N.J.: Humanities Press. 1975. Pp. 262. \$10.00.
- FEHL, NOAH EDWARD. *Personality and Pattern in History*. Hong Kong: Chung Chi Publications. 1975. Pp. xiv, 171. \$8.00.
- FOWLER, PETER J., editor. *Recent Work in Rural Archeology*. Totowa, N.J.: Rowman and Littlefield. 1975. Pp. 160. \$13.00.

- GILLISPIE, CHARLES COULSTON, et al., editors. *Dictionary of Scientific Biography*. Volume XIII, *Hermann Staudinger-Giuseppe Veronese*. New York: Charles Scribner's Sons. 1976. Pp. xiii, 623. \$40.00.
- GOLDMAN, ISRAEL M. *Lifelong Learning Among Jews: Adult Education in Judaism from Biblical Times to the Twentieth Century*. With an introduction by LOUIS FINKELSTEIN. New York: Ktav Publishing House. 1975. Pp. xxii, 364. \$15.00.
- GRAETZ, HEINRICH, translator and editor. *The Structure of Jewish History and Other Essays*. (Studies in Jewish History, Literature, and Thought Series: Moreshet, volume 3.) New York: Jewish Theological Seminary of America. 1975. Pp. x, 325. Cloth \$15.00, paper \$4.95.
- LOWENTHAL, DAVID, and BOWDEN, MARTYN J., editors. *Geographies of the Mind: Essays in Historical Geosophy in Honor of John Kirtland Wright*. New York: Oxford University Press. 1976. Pp. 263. \$9.00.
- PISTONE, SERGIO, editor. *L'idea dell'unificazione europea dalla prima alla seconda guerra mondiale: Relazioni tenute al convegno di studi svoltosi presso la Fondazione Luigi Einaudi (Torino, 25-26 ottobre 1974)*. (Studi, 21.) Turin: Fondazione Luigi Einaudi. 1975. Pp. 243. L.3,500.
- ROBERTS, JOHN. *Revolution and Improvement: The Western World, 1775-1847*. Berkeley and Los Angeles: University of California Press. 1976. Pp. xii, 290. \$20.00.
- SMITH, PAUL, editor. *The Historian and Film*. New York: Cambridge University Press. 1976. Pp. viii, 208. \$13.95.
- STEPHENS, LESTER D., compiler and editor. *Historiography: A Bibliography*. Metuchen, N.J.: Scarecrow Press. 1975. Pp. vi, 271. \$9.00.
- WAGNER, ANTHONY. *Pedigree and Progress: Essays in the Genealogical Interpretation of History*. London: Phillimore and Co. 1976. Pp. 333. \$25.00.
- WESTMAN, ROBERT S., editor. *The Copernican Achievement*. (UCLA Center for Medieval and Renaissance Studies Contributions, 7.) Berkeley and Los Angeles: University of California Press. 1975. Pp. xvi, 405. \$14.50.
- WOODWARD, DAVID, editor. *Five Centuries of Map Printing*. Chicago: University of Chicago Press. 1975. Pp. xi, 177. \$17.50.

ANCIENT

- BONFANTE, LARISSA. *Etruscan Dress*. Baltimore: Johns Hopkins University Press. 1976. Pp. ix, 243. \$17.50.
- Byzantine Books and Bookman*. (Dumbarton Oaks Colloquium, 1971.) Washington, D.C.: Dumbarton Oaks Center for Byzantine Studies. 1975. Pp. x, 109.
- CARY, M., and SCULLARD, H. H. *A History of Rome Down to the Reign of Constantine*. 3d ed. New York: St. Martin's Press. 1976. Pp. xxvii, 694. \$14.95.
- EHRENBERG, VICTOR. *The People of Aristophanes: A Sociology*

- of *Old Attic Comedy*. New York: Barnes and Noble, 1974. Pp. xii, 385. \$19.00.
- FOSS, CLIVE. *Byzantine and Turkish Sardis*. (Archaeological Exploration of Sardis, 4.) Cambridge, Mass.: Harvard University Press. 1976. Pp. xvi, 216. \$12.50.
- GRINSELL, LESLIE V. *Barrow, Pyramid and Tomb: Ancient Burial Customs in Egypt, the Mediterranean and the British Isles*. (The World of Archaeology.) Boulder, Colorado: Westview Press. 1975. Pp. 240. \$18.50.
- HADINGHAM, EVAN. *Circles and Standing Stones: An Illustrated Exploration of Megalith Mysteries of Early Britain*. New York: Walker and Company. 1975. Pp. vii, 240. \$12.50.
- HAMMOND, N. G. L. *The Classical Age of Greece*. (History of Civilizations.) New York: Barnes and Noble. 1976. Pp. xi, 308. \$27.50.
- ROBERTSON, MARTIN. *A History of Greek Art*, in two volumes. New York: Cambridge University Press. 1975. Pp. xviii, 611; v, 613-835.

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- BARRACLOUGH, GEOFFREY. *The Crucible of Europe: The Ninth and Tenth Centuries in European History*. Berkeley and Los Angeles: University of California Press. 1976. Pp. 180. \$14.95.
- FOLDA, JAROSLAV. *Crusader Manuscript Illumination at Saint-Jean d'Acre, 1275-1291*. Princeton: Princeton University Press. 1976. Pp. xxix, 231. \$35.00.
- LEWIS, ARCHIBALD. *Knights and Samurai: Feudalism in Northern France and Japan*. London: Temple Smith. 1975. Pp. 101. \$7.75.
- Medievalia et Humanistica: Studies in Medieval and Renaissance Culture*. (New Series, number 6, *Medieval Hagiography and Romance*, edited by PAUL MAURICE CLOGAN.) New York: Cambridge University Press. 1976. Pp. xiii, 223. \$19.95.
- RAFTIS, J. A. *Assart Data and Land Values: Two Studies of the East Midlands, 1200-1350*. (Subsidia Mediaevalia, 3.) Toronto: Pontifical Institute of Mediaeval Studies. 1974. Pp. 169. \$8.50.
- STORM, GUSTAV, editor. *Latinske Kildeskrifter til Norges Historie I Middelalderen*. (Monumenta Historica Norvegiae, Latine Conscripta.) Reprint. Oslo: Norsk Historisk Kildeskrift-Institutt. 1973. Pp. lxii, 301.
- WALEY, DANIEL. *Later Medieval Europe from Saint Louis to Luther*. Rev. ed. New York: Longman. 1975. Pp. xiv, 306. \$7.50.
- YATES, FRANCES A. *The Valois Tapestries*. New ed. Boston: Routledge and Kegan Paul. 1976. Pp. xxvii, 150, xii. \$26.25.

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- BATTS, JOHN STUART. *British Manuscript Diaries of the Nineteenth Century: An Annotated Listing*. Totowa, N.J.: Rowman and Littlefield. 1976. Pp. xi, 345. \$25.00.
- BINGHAM, CAROLINE. *The Kings & Queens of Scotland*. New York: Taplinger Publishing Company. 1976. Pp. xi, 182. \$9.95.
- CLARKSON, LESLIE. *Death, Disease and Famine in Pre-industrial England*. New York: St. Martin's Press. 1975. Pp. 188. \$17.95.
- COOK, CHRIS, compiler. *Sources in British Political History, 1900-1951*. Volume 2, *A Guide to the Private Papers of Selected Public Servants*. With PHILIP JONES et al. New York: St. Martin's Press. 1975. Pp. 297. \$16.95.
- DARBY, H. C., and VERSEY, G. R. *Domesday Gazetteer*. New York: Cambridge University Press. 1975. Pp. viii, 544. \$72.50.
- DERRY, JOHN W. *Castlereagh*. (British Political Biography.) New York: St. Martin's Press. 1976. Pp. viii, 247. \$15.95.
- GLASSCOCK, ROBIN E., editor. *The Lay Subsidy of 1334*. (Records of Social and Economic History. New Series, 2.) New York: Oxford University Press, for The British Academy. 1975. Pp. xxxvii, 516. \$36.50.
- GODFREY, ELEANOR S. *The Development of English Glassmaking, 1560-1640*. Chapel Hill: University of North Carolina Press. 1975. Pp. xii, 288.
- GRAVES, EDGAR B., editor. *A Bibliography of English History to 1485*. New York: Oxford University Press. 1975. Pp. xxiv, 1103. \$52.00.
- HALL, A. RUPERT, and HALL, MARIE BOAS, editors and translators. *The Correspondence of Henry Olden*. Volume 10, *June 1673-April 1674, Letters 2241-2489*. London: Mansell; distrib. by International Scholarly Book Services, Beaverton, Ore. 1975. Pp. xxvii, 596. \$35.00.
- HALL, A. RUPERT, and TILLING, LAURA, editors. *The Correspondence of Isaac Newton*. Volume 5, *1709-1713*. New York: Cambridge University Press, for the Royal Society of London. 1975. Pp. li, 439. \$55.00.
- HANHAM, ALISON, editor. *The Cely Letters, 1472-1488*. (Early English Text Society, number 273.) New York: Oxford University Press. 1975. Pp. xxvii, 365. \$15.00.
- HILL, B. W., editor. *Edmund Burke on Government, Politics and Society*. New York: International Publications Service. 1976. Pp. 382. \$15.00.
- HILTS, VICTOR L. *A Guide to Francis Galton's English Men of Science*. (Transactions of the American Philosophical Society. New Series, volume 65, part 5.) Philadelphia: The American Philosophical Society. 1975. Pp. 85.
- KENNEDY, KIERAN A., and DOWLING, BRENDAN R. *Economic Growth in Ireland: The Experience Since 1947*. New York: Barnes and Noble, in association with The Economic and Social Research Institute, Dublin. 1976. Pp. xix, 345. \$22.50.
- KILLIP, MARGARET. *The Folklore of the Isle of Man*. Drawings by NORMAN SAYLE. (The Folklore of the British Isles.) Totowa, N.J.: Rowan and Littlefield. 1976. Pp. 207. \$15.00.
- LERUEZ, JACQUES. *Economic Planning & Politics in Britain*. Translated by MARTIN HARRISON; preface by J. E. S. HAYWARD. New York: Barnes and Noble. 1976. Pp. xi, 324. \$20.00.
- MCLEAN, IAIN. *Keir Hardie*. (British Political Biography.) New York: St. Martin's Press. 1975. Pp. viii, 183. \$12.95.
- MCNEILL, PETER, and NICHOLSON, RANALD, editors. *An Historical Atlas of Scotland c.400-c.1600*. Fife, Scotland: Atlas Committee of the Conference of Scottish Medievalists. 1975. Pp. x, 213. \$6.00.
- MINGAY, G. E., editor. *Arthur Young and His Times*. Toronto: Macmillan Company. 1975. Pp. 264. \$26.50.
- PARSONS, DAVID, editor. *Tenth-Century Studies: Essays in Commemoration of the Millennium of the Council of Winchester and Regularis Concordia*. London: Phillimore and Co. 1976. Pp. xiii, 270. \$25.00.
- PEELE, GILLIAN, and COOK, CHRIS, editors. *The Politics of Reappraisal, 1918-1939*. New York: St. Martin's Press. 1975. Pp. viii, 265. \$15.95.
- REID, R. R. *The King's Council in the North*. Reprint. Totowa, N.J.: Rowman and Littlefield. 1976. Pp. x, 532. \$30.00.
- ROBERTS, PHILIP, editor. *The Diary of Sir David Hamilton, 1709-1714*. New York: Oxford University Press. 1975. Pp. xlviii, 138. \$28.00.
- THOMAS, PETER D. G. *Lord North*. (British Political Biography.) New York: St. Martin's Press. 1975. Pp. viii, 176. \$12.95.

FRANCE

- CHURCH, WILLIAM F. *Louis XIV in Historical Thought: From Voltaire to the Annales School*. (A Norton Series: Histori-

- cal Controversies.) New York: W. W. Norton and Company. 1976. Pp. 127. Cloth \$6.95, paper \$1.95.
- COMTE, AUGUSTE. *Correspondance générale et confessions*. Volume 2, avril 1841-mars 1845. Compiled by PAULO E. DE BERRÉDO CARNEIRO and PIERRE ARNAUD. (Archives Positivistes.) Paris: Mouton. 1975. Pp. xxxvi, 461. 98 fr.
- SZAJKOWSKI, ZOSA. *Jews and the French Foreign Legion*. New York: Ktav Publishing House. 1975. Pp. 280. \$17.50.

SPAIN AND PORTUGAL

- ALMUIÑA FERNÁNDEZ, CELSO. *Teatro y cultura en el Valladolid de la ilustración: los medios de difusión en la segunda mitad del xviii*. With a prologue by L. M. ENCISO REGIO. (Colección de Publicaciones Municipales, number 3.) Valladolid, Spain: Ayuntamiento de Valladolid. 1974. Pp. xxxii, 245. 250 ptas.
- GÓMEZ-IBÁÑEZ, DANIEL ALEXANDER. *The Western Pyrenees: Differential Evolution of the French and Spanish Borderland*. (Oxford Research Studies in Geography.) New York: Oxford University Press. 1975. Pp. xiv, 162. \$16.75.
- GONZÁLEZ JIMÉNEZ, MANUEL. *La repoblación de la zona de Sevilla durante el siglo XIV: Estudio y documentación*. (Anales de la Universidad Hispalense, Serie, Filosofía y Letras, number 28.) Seville: Publicaciones de la Universidad de Sevilla. 1975. Pp. 164.
- HANSON, CARL A. *Dissertations on Iberian and Latin American History*. New York: Whitson Publishing Company. 1975. Pp. v, 400.
- RODRÍGUEZ GARRAZA, RODRIGO. *Tensiones de Navarra con la Administración Central (1778-1808)*. Pamplona: Diputación Foral de Navarra, Institución Príncipe de Viana, Consejo Superior de Investigaciones Científicas. 1974. Pp. 360.
- SÁNCHEZ-ALBORNOZ, NICOLÁS. *Los precios agrícolas durante la segunda mitad del siglo XIX*. Volume 1, Trigo y cebada. (Materiales para la Historia Económica de España.) Madrid: Servicio de Estudios del Banco de España. 1975. Pp. xii, 283.

LOW COUNTRIES

- The Correspondence of Erasmus: Letters 142 to 297, 1501 to 1514*. Translated by R. A. B. MYNORS and D. F. S. THOMSON. Annotated by WALLACE K. FERGUSON. (Collected Works of Erasmus, volume 2.) Buffalo: University of Toronto Press. 1975. Pp. xiii, 374. \$25.00.
- DEMOLEN, RICHARD L., editor. *Erasmus of Rotterdam: A Quincentennial Symposium*. New York: Twayne Publishers. 1971. Pp. 152.
- HOOPYKAAS, G. J., editor. *De Briefwisseling van J. R. Thorbecke*. Volume 1, 1830-1833. (Rijks Geschiedkundige Publicatiën. Short series, 42.) 's-Gravenhage: Martinus Nijhoff. 1975. Pp. x, 527.
- LIJPHART, AREND. *The Politics of Accommodation: Pluralism and Democracy in the Netherlands*. 2d ed., rev. Berkeley and Los Angeles: University of California Press. 1976. Pp. xiv, 231. \$3.85.
- VAN WINTER, P. J. *Oorlogsduur in Oorlogsnamen: Over het gebruik van getallen tot steun van historische voorstellingen*. (Verhandelingen der Koninklijke Nederlandse Akademie van Wetenschappen, afd. Letterkunde. New Series, volume 77, number 1.) Amsterdam: N. V. Noord-Hollandsche Uitgevers Maatschappij. 1972. Pp. 111. \$7.75.

GERMANY, AUSTRIA, AND SWITZERLAND

- BAUMGART, WINFRIED. *Vom Europäischen Konzert zum Völkerbund: Friedensschlüsse und Friedenssicherung von Wien bis*

Versailles. (Erträge der Forschung, volume 25.) Darmstadt: Wissenschaftliche Buchgesellschaft. 1974. Pp. x, 181.

- BUCHNER, RUDOLF. *Deutsche Geschichte im Europäischen Rahmen: Darstellung und Betrachtungen*. Göttingen: Musterschmidt-Verlag. 1975. Pp. xviii, 505.
- COING, HELMUT, editor. *Ius Commune*. (Veröffentlichungen des Max-Planck-Instituts für Europäische Rechtsgeschichte, volume 5.) Frankfurt: Vittorio Klostermann. 1975. Pp. 305.
- HAASE, CARL. *Die Archivalien zur deutschen Geschichte: In deutschen und einigen anderen Archiven mit kurzen Bemerkungen über Bibliotheken und andere Sammlungen*. Boppard am Rhein: Harold Boldt Verlag. 1975. Pp. 194.
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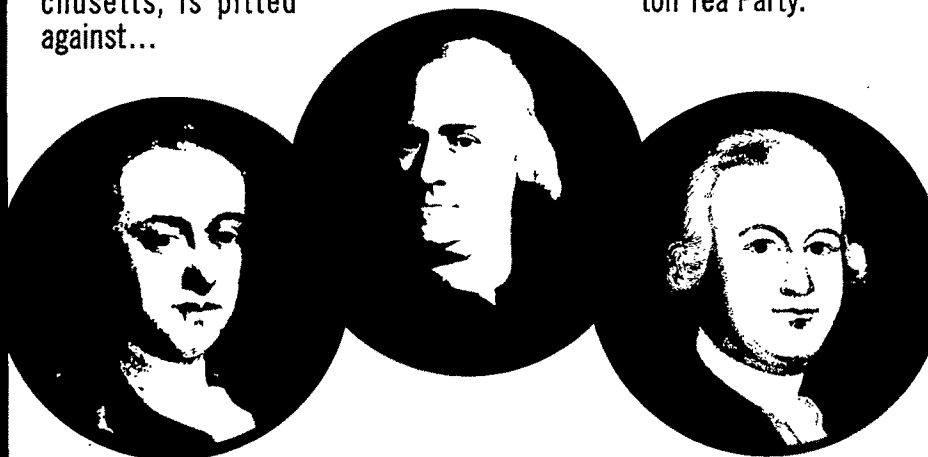
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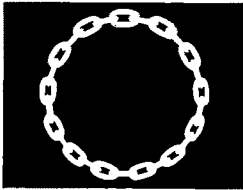
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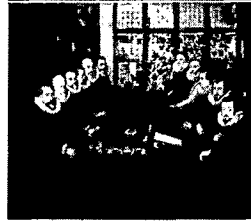
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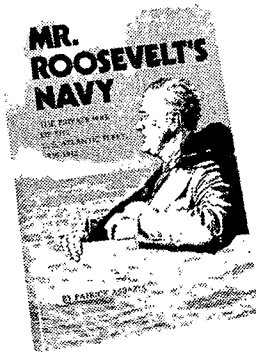
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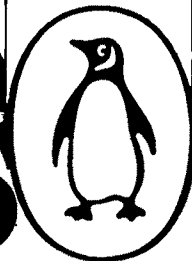
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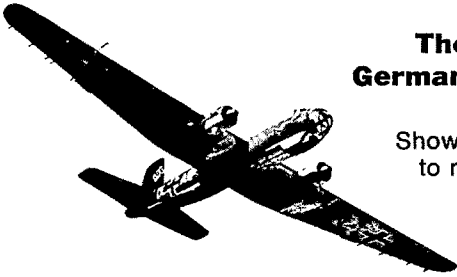
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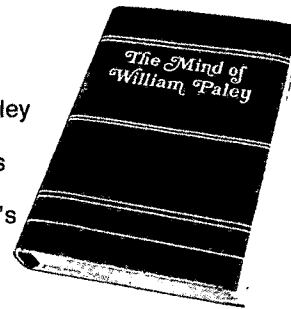
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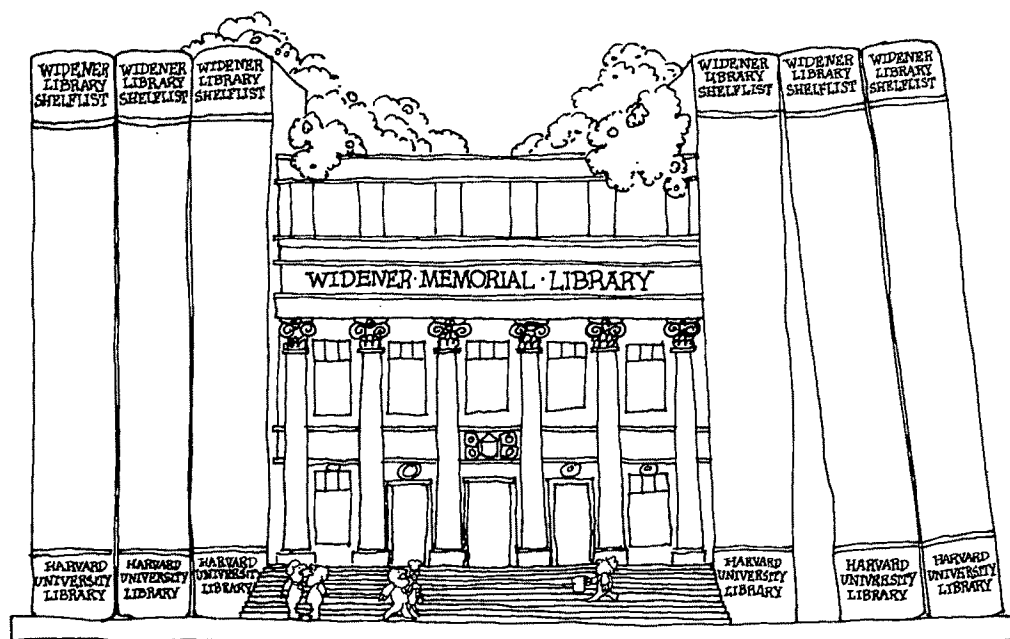
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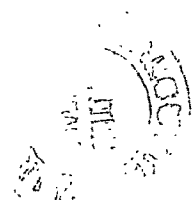
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Some Arguments in Defense of the Venetians on the Fourth Crusade

DONALD E. QUELLER
and
GERALD W. DAY

OF THE MANY MYTHS clouding Venetian history none has been more influential and persistent than the pejorative one concerning the city's role in the Fourth Crusade. The indictment of Venice was stated most eloquently by John Ruskin, who wrote that "the expedition which was commanded by the noblest of her princes, and whose results added most to her military glory, was one in which while all Europe around her was wasted by the fire of its devotion, she first calculated the highest price she could exact from its piety for the armament she furnished, and then, for the advancement of her own private interests, at once broke her faith and betrayed her religion."¹ Since Louis de Mas Latrie in the mid-nineteenth century first enunciated the classic scholarly theory of Venetian treason in the Fourth Crusade many historians have charged that Dandolo and the Venetians schemed to exploit the religious zeal of the crusaders to gain their own commercial ends.²

Donald M. Nicol in the new *Cambridge Medieval History* accurately sums up the prevailing view with which he ardently agrees: "In the interminable controversy over the diversion of the Fourth Crusade, many have doubted the ulterior motives of Philip of Swabia, of Boniface of Montferrat, and of Innocent III. But few have tried to exonerate the Venetians."³ Even scholars who are otherwise well disposed toward Venice, such as the distinguished Venetophile, Frederic C. Lane, often allow the myth of Venetian duplicity to intrude into their writings,⁴ and Venice's staunchest defender, Roberto Cessi, obviously strains to offer plausible vindication of her role in the great misadventure.⁵

¹ John Ruskin, *The Stones of Venice* (New York, 1884), 1: 6.

² Louis de Mas Latrie, *Histoire de l'île de Chypre sous le règne des princes de la maison de Lusignan* (Paris, 1852-1861), 1: 161-4. For a survey of the treason theorists to 1969, see Donald E. Queller and Susan J. Stratton, "A Century of Controversy on the Fourth Crusade," *Studies in Medieval and Renaissance History*, 6 (1969), 238-52.

³ Donald M. Nicol, "The Fourth Crusade and the Greek and Latin Empire, 1201-1261," *Cambridge Medieval History* (Cambridge, 1966), 4: 278.

⁴ Frederic C. Lane, *Venice: A Maritime Republic* (Baltimore, 1973), 45.

⁵ Roberto Cessi, "L'eredità di Enrico Dandolo," *Archivio veneto*, ser. 5, 67 (1960), 2, n. 2; "Venezia e la quarta crociata," *Archivio veneto*, ser. 5, 48-69 (1951), 11, n. 1; 26-67. His views have not been widely accepted.

Among the anti-Venetian assumptions of most historians of the Fourth Crusade, the following are prominent:

1. A religious crusading enthusiasm was foreign to the Venetian psyche.⁶
2. Venice compelled the crusading host to comply with its treacherous plans.⁷
3. The Egyptian target upon which the Venetians and the crusaders' envoys had agreed in early 1201 was disadvantageous to Venice's mercantile interests.⁸
4. Venice was suffering setbacks in Constantinople that focused her attention exclusively upon securing a stable monopolistic position in the Greek Empire.⁹

We are not trying to make the Venetians the heroes of the crusade, while transferring the charge of treason to the Germans, the pope, or some other candidate. We do intend to marshal evidence to prove that these four widespread assumptions are too shaky to sustain the weight of the anti-Venetian theories that scholars have built upon them. They do seem plausible, even self-evident, in the light of the eventual profit that Venice derived from the conquest of Constantinople; a close scrutiny of the evidence concerning the crusade and the overall Levantine economic situation at the beginning of the thirteenth century, however, will raise doubts about the culpability of the Venetians. We believe that they were the victims, and in the end the beneficiaries, like the other crusaders, of a series of mistakes, failures, and accidents that determined the course of the Fourth Crusade. The Venetians were caught up in a chain of unforeseen events that threatened to overwhelm the crusade, and it was they and their inimitable Doge Dandolo who consistently provided the leadership to rescue the crusading host.

There is a widely held misconception among historians of the Fourth Crusade that the Venetian interest in the crusading movement was radically different from that of the northern knights and morally and religiously inferior to it. While the northern crusader is usually portrayed as driven by sublime motives of self-sacrifice and even martyrdom, the Venetian is pictured as a greedy opportunist whose sole consideration was profit. In fact, the differences in background and viewpoint between the two groups are minor in contrast to the values they shared as members of Latin Christendom.

⁶ For example: Freddy Thiriet, "Le quart et demi de la Romanie," in Jacques Goimard, ed., *Venise au temps des galères* (Paris, 1968), 72; Gina Fasoli, "Nascita di un mito," in *Studi storici in onore di Gioacchino Volpe* (Florence, 1958), 462; Edgar H. McNeal and Robert Lee Wolff, "The Fourth Crusade," in Kenneth Setton, ed., *A History of the Crusades*, 2d ed. (Madison, 1969), 2: 161.

⁷ For example: Charles Diehl, "The Fourth Crusade and the Latin Empire," *Cambridge Medieval History* (Cambridge, 1936), 4: 416; Steven Runciman, *A History of the Crusades* (Cambridge, 1953-57), 3: 114.

⁸ For example: Mas Latrie, *Histoire de l'île de Chypre*, 1: 163-4. William Hazlitt, *The Venetian Republic: Its Rise, its Growth, and its Fall, A.D. 409-1797* (London, 1915), 1: 266-7.

⁹ The *locus classicus* for this assumption is Nicetas Choniates, *Historia*, ed. by Immanuel Bekker (Bonn, 1835), 713-4. It is also one of the main themes of Walter Norden's admirable work, *Der vierte Kreuzzug im Rahmen der Beziehungen des Abendlandes zu Byzanz* (Berlin, 1898). See also McNeal and Wolff, "Fourth Crusade," 161-2; George Ostrogorsky, *History of the Byzantine State*, trans. by Joan Hussey (Oxford, 1956), 368; A. A. Vasiliev, *History of the Byzantine Empire, 324-1453* (Madison, 1952), 452-3; Diehl, "Fourth Crusade," 417; Steven Runciman, "Byzantine Trade and Industry," *Cambridge Economic History* (Cambridge, 1941-1965), 2: 101.

We must, indeed, accept only with reservations the exaggerated religiosity of the knightly crusader. Although true devotion undoubtedly set many men on their warlike pilgrimage, the hope of earthly gain was certainly a significant motive for crusading leaders such as Baldwin of Boulogne, who carved out a personal lordship around Edessa, Bohemund of Taranto, who became prince of Antioch, and Raymond of Saint-Gilles, who spent the last years of his life in an unsuccessful attempt to make Tripoli his own. It is well known that at the very conception of the First Crusade Urban II aroused the cupidity of his audience at Clermont by urging the conquest of "the land of milk and honey."¹⁰

The crusaders' zeal for liberating relics also had its greedy aspect. The Fourth Crusade is especially notorious for the vast number of these venerable objects that were ecstatically carted off from Constantinople to the West.¹¹ In this light it seems somewhat ironical that Gunther of Pairis, who berated the Venetians for their greed, glorified his abbot's successful looting of Constantinople's hoard of precious relics.¹²

When compared to the worldly-wise creators of the crusading states and to the zealous souls who plundered relics, the Venetians cannot be regarded as unique. The factor distinguishing the knight, the ecclesiastic, and the merchant from one another in this respect was only what they considered to be of value. The feudal baron desired territorial dominion, the priest or monk a trove of sacred souvenirs, and the Italian merchant commercial advantage. The person brought up in a feudal or ecclesiastical milieu might well fail to understand and sympathize with the values of the parvenu bourgeois. Yet on the whole these values are not inherently inferior to feudal values or even to some of those for which the church had stood: in contexts other than the crusades historians of the High Middle Ages generally regard favorably the emergence of bourgeois standards. In fact, the negative attitude of the feudalism toward the Venetians probably has been exaggerated. A thorough reading of Villehardouin and Robert of Clari will turn up no instance of anti-Venetian feeling on the part of either author, although both of them were thoroughly imbued with the values of feudal society. Both chroniclers, in fact, are full of praise for their Venetian allies.

We do not deny, of course, that the Venetians could be strongly moved by commercial interests, but there is also evidence of their genuine religious enthusiasm for the Fourth Crusade. Where differing motives point toward the same goal it is an audacious and simplistic historian who insists upon one to the exclusion of others. Villehardouin, in fact, presents the Venetians as a

¹⁰ This phrase, taken from Exodus, 3:8, occurs in Robert the Monk's version of Urban's sermon in a context contrasting the rewards of Palestine with the future crusaders' poverty at home. *Recueil des historiens des croisades, historiens occidentaux* (Paris, 1844-95), 3: 728-9. For a brief discussion of the promise of worldly rewards mentioned in the various versions of Urban's sermon, see Dana C. Munro, "The Speech of Urban II at Clermont," *American Historical Review*, 11 (1906), 239.

¹¹ Paul Riant compiled three volumes of materials on such translations, *Exuviae sacrae Constantinopolitanae* (Geneva and Paris, 1877-1904).

¹² Gunther of Pairis, *Historia Constantinopolitana*, in Riant, ed., *Exuviae*, 1: 71, 85, 104-8, 121-2, 125-6.

people intensely moved by the religious aspects of the crusade. His hyperbolic account of their reception of the proposal to join the crusade is marked by their pious fervor:¹³

There the six envoys knelt at the feet of the people, weeping many tears. And the Doge and all the others burst into tears of pity and compassion, and cried with one voice, and lifted up their hands, saying: "We consent!" Then there was so great a noise and tumult that it seemed as if the earth itself were falling to pieces. And when this great tumult and passion of pity—greater did never any man see—were appeased, the good Doge of Venice, who was very wise and valiant, went up into the reading desk and spoke to the people and said: "Signors, behold the honour that God has done for you; for the best people in the world have set aside all other people, and chosen you to join them in so high an enterprise as the deliverance of our Lord."

Later, when the expedition had been prepared and was on the point of departure, the doge himself and many of his subjects took the crusading vow in Saint Mark's in an emotion-packed display of zeal for the pious mission:¹⁴

Before the beginning of High Mass, the Doge of Venice, who bore the name of Henry Dandolo, went up into the reading desk, and spoke to the people, . . . "Signors, you are associated with the most worthy people in the world, and for the highest enterprise ever undertaken; and I am a man old and feeble, who should have need of rest, and I am sick in body; but I see that no one could command and lead you like myself, who am your lord. If you will consent that I take the sign of the cross to guard and direct you, and that my son remain in my place to guard the land, then shall I go to live or die with you and with the pilgrims." And when they had heard him, they cried with one voice: "We pray you by God that you consent, and do it, and that you come with us!" Very great was then the pity and compassion on the part of the people of the land and of the pilgrims; and many were the tears shed, because that worthy and good man would have had so much reason to remain behind, for he was an old man, and albeit his eyes were unclouded, yet he saw naught, having lost his sight through a wound in his head. . . . Thus he came down from the reading desk, and went before the altar, and knelt upon his knees greatly weeping. And they sewed the cross onto a great cotton hat, which he wore, because he wished that all men should see it. And the Venetians began to take the cross in great numbers.

By this act the Italian merchants transformed themselves from purely secular dealers in goods and services to *milites Christi*, enjoying the religious perquisites and accepting the obligations of crusaders. J. Lestocquoy has made the point, which needs to be emphasized, that among the medieval bourgeois, "*la foi chrétienne n'était pas un décor.*"¹⁵

In response to the charge that Venice participated minimally in the crusades and only for her own profit, it can be shown that the Republic had invested considerable resources in the Holy War. It is true that Venice did not take part in the initial operations of the First Crusade, but this hesitancy was

¹³ Geoffroi de Villehardouin, *La conquête de Constantinople*, ed. by Edmond Faral (Paris, 1933-9), 1: 28-30. Translation by Sir Frank T. Marzials, *Memoirs of the Crusades* (New York, 1958), 8.

¹⁴ Villehardouin, *Conquête de Constantinople*, 1: 66-70. See especially 1: 68-70 for Venetian emotions. Trans. by Marzials, *Memoirs of the Crusades*, 16-7.

¹⁵ J. Lestocquoy, *Aux origines de la bourgeoisie: les villes de Flandre et d'Italie sous le gouvernement des patriciens (XI^e-XV^e)* (Paris, 1952), 125.

quite possibly not caused by religious apathy, but by extremely strong practical considerations that outweighed Venetian piety. Norman influence was strong in the First Crusade.¹⁶ The Venetians may well have feared, as did their Byzantine patrons, that the expedition would turn out to be a re-enactment of Robert Guiscard's abortive invasion of the Byzantine Empire only twenty years before. If such an eventuality had taken place with Venetian help, the city's vital friendly relations with Constantinople would have been seriously jeopardized. Any addition to Norman strength, moreover, would have increased the threat of Norman control of the Adriatic and the loss of unrestricted Venetian access to the Mediterranean.¹⁷ Once the Norman influence over the crusade had been diminished when Bohemund withdrew from the expedition to rule his new principality of Antioch, the Venetians in 1099 sent a large fleet under the doge himself that reprovisioned the destitute Christian army lingering at newly captured Jaffa.¹⁸ In return for commercial concessions in crusader territories, the Venetian force supported Godfrey of Bouillon's army with supplies, war engines, and men during the crusaders' successful sieges of the castle of Acheron and Haifa.¹⁹ A Venetian fleet of almost a hundred ships also helped Baldwin I conquer Sidon in 1110.²⁰

Venice's great contribution to the crusader states came in the early 1120s when a Venetian armada crushed the Fatimid navy at the battle of Ascalon and gave decisive support to the capture of Tyre.²¹ As the situation in Syria settled down to an uneasy coexistence between the crusaders and their Muslim adversaries, the need for further Venetian participation in the crusading movement vanished for a time. During the Second Crusade, the naval support that the Venetians were giving to the Byzantine Empire against Roger II of Sicily prevented the city from taking part in the Palestinian enterprise.²²

Venice dutifully came to the aid of Christendom again in the Third Crusade. The city quickly responded to the Muslim challenge by sending a large fleet to the Holy Land in 1188, although nothing is heard of the fleet in action.²³ In the next year, however, a joint Pisan and Venetian fleet sailed east and gave significant help to the small Christian army besieging Acre.²⁴ In order to

¹⁶ Sidney Painter, "Western Europe on the Eve of the Crusades," in Setton, ed., *History of the Crusades*, 1: 21.

¹⁷ Gino Luzzatto, *An Economic History of Italy: From the Fall of the Roman Empire to the Beginning of the Sixteenth Century*, trans. by Philip Jones (London, 1961), 73; James Westfall Thompson, *An Economic and Social History of the Middle Ages* (New York, 1928), 330-3.

¹⁸ The Venetians gave some supplies to the crusaders and sold them others. *Translatio Sancti Nicolai ad Venetiam*, in *Recueil des historiens des croisades, historiens occidentaux*, 5: 271. See also *Annales venetici breves*, in *M.G.H.*, SS., 14: 70, and Andrea Dandolo, *Chronica*, ed. by E. Pastorello, in *R.I.S.*, 12: 222-3. For a differing interpretation, see Freddy Thiriet, *La Romanie vénitienne au moyen âge* (Paris, 1959), 40.

¹⁹ *Translatio Sancti Nicolai*, 272, 275-7.

²⁰ Dandolo, *Chronica*, 264.

²¹ Runciman, *History of the Crusades*, 2: 166-7; Robert L. Nicholson, "The Growth of the Latin States, 1118-1144," in Setton, ed., *History of the Crusades*, 1: 421.

²² Virginia G. Berry, "The Second Crusade," in Setton, ed., *History of the Crusades*, 1: 486; Dandolo, *Chronica*, 243; G. L. Tafel and G. M. Thomas, eds., *Urkunden zur älteren Handels- und Staatsgeschichte der Republik Venedig* (Vienna, 1856-7), 1: 113-24.

²³ *Annales venetici breves*, 72.

²⁴ Dandolo, *Chronica*, 270.

concentrate the Venetian effort on aid to the Holy Land, the doge in November 1188 ordered all Venetians from various parts of the world to return to Venice by Easter.²⁵ Here is an example of the Venetians specifically and explicitly sacrificing their commercial activities in the interest of the crusade.

In the course of the Third Crusade, moreover, Venice remained aloof from the intense crusader factionalism between the Monferrine and Lusignan parties, while the Pisans and the Genoese, on the contrary, sought to enlarge their Syrian establishments by playing one side off against the other.²⁶ It is probably in part for this reason that the Venetians are infrequently mentioned in the non-Venetian sources for the crusade, which deal at length with this struggle.²⁷ All that Venice received for its services, and apparently all that it asked, was a confirmation of its pre-Saladinic privileges and properties outlined in the treaty of 1123 with Patriarch Warmund of Jerusalem.²⁸ In short, while there is little doubt that a major reason for Venetian participation in the Third Crusade was to regain what the republic had lost by Saladin's invasion, Venice alone of the major Italian mercantile cities did not exploit the plight of the Christian Holy Land and the ruinous strife among the crusaders themselves to enhance its existing commercial privileges.

As a final entry, prior to 1201, into this respectable record of aid to the Holy Land, Venice, once again unlike the other mercantile cities, responded favorably in 1198 to a papal legate sent to gain support for the new crusade.²⁹ This continuing accessibility to crusading overtures must have influenced the decision of the ambassadors who in 1201 came to Italy to find transportation for the new crusade that was being marshaled in the heart of France.³⁰

Envoys of the counts of Flanders, Champagne, and Blois concluded a contract for transportation of the crusaders with the Venetians in March or April of 1201.³¹ The agreement called for the passage of 33,500 Christian soldiers and 4,500 horses and their maintenance for one year at a total price of 85,000 Cologne marks. The Venetians also agreed to join the crusade them-

²⁵ *Ibid.* The document itself is published in Tafel and Thomas, eds., *Urkunden*, 1: 204-6.

²⁶ *Infra*, p. 733.

²⁷ Richard of London, for example, often mentions the Pisans and the Genoese, but never the Venetians. *Itinerarium peregrinorum et gesta regis Ricardi: auctore ut videtur Ricardo canonico S. Trin. Lond.*, ed. by William Stubbs (London, 1864), 61, 109, 136, 137, 174, 212, 227, 228, 321-3, 405, 413, 416 (for the Pisans); 84, 151, 174, 206, 321-3, 405, 413, 414, 416 (for the Genoese; Venice is mentioned only once, and then only as the destination of a part of the crusading force on its way east (152).

²⁸ Tafel and Thomas, eds., *Urkunden*, 1: 84-9.

²⁹ *Gesta Innocentii Papae III*, Migne, PL, CCXIV, xc.

³⁰ On this embassy, see D. E. Queller, "L'évolution du rôle de l'ambassadeur: les pleins pouvoirs et le traité de 1201 entre les croisés et les vénitiens," *Le Moyen Age*, 67 (1961), 479-501. Of the three major Italian commercial cities probably only Venice had the capability to transport the huge army of 33,500 men that was expected to assemble. Even so, Venice, the greatest of the maritime powers, had to strain all of its resources to meet its contractual obligations. *Infra*, pp. 724-6. The likelihood of Genoese or Pisan participation was slight because of their war against each other. *Gesta Innocentii*, xci.

³¹ The date of the treaty has not been satisfactorily solved. Villehardouin, *Conquête de Constantinople*, 1: 30-2, says that the treaty was concluded during Lent, that is, prior to March 25, but the extant treaty itself bears a date of April 1201. For attempted reconciliations, see C. Klimke, *Die Quellen zur Geschichte des vierten Kreuzzugs* (Breslau, 1877), 83, n. 2; Faral, in Villehardouin, *Conquête de Constantinople*, 1: 219; Jean Longnon, *Recherches sur la vie de Geoffroy de Villehardouin, suivies du catalogue des actes des Villehardouin* (Paris, 1939), 69; Queller, "L'évolution du rôle de l'ambassadeur," 494. *Contra*, see Colin Morris, "Geoffrey de Villehardouin and the Conquest of Constantinople," *History*, 53 (1968), 30-1.

selves with fifty galleys, and it was further provided that they should receive one-half of all conquests.³²

The tragedy of the Fourth Crusade is found in the gross overestimation of the size and resources of the crusading army. The projected force would have been three times the size of the one mustered by Philip Augustus for his most crucial battle, Bouvines, in 1214, and the price was double the annual revenues of the king of England or the king of France.³³ It is true, of course, that the count of Flanders, who ruled a highly industrialized area, and the count of Champagne, in whose domain the profitable fairs took place, were two of the wealthiest French nobles, but events were to prove the incapacity of the crusaders to muster such a large force.

The Venetians have been accused of trapping the gullible crusaders in a contract that they knew the northerners could not fulfill so they might have the army at their mercy, but this argument will not stand.³⁴ Even though Villehardouin puts into the mouth of Doge Dandolo the first specific proposal of terms for the contract, it is inconceivable, as Colin Morris says, that the envoys did not provide the estimate of the number of crusaders to be accommodated.³⁵ The envoys of the three counts, moreover, were not such dolts as Hellwig makes them out to be. Geoffrey of Villehardouin, the spokesman, was a mature man of about fifty, marshal of the prosperous county of Champagne. He enjoyed then and continued to enjoy an excellent reputation for his political *savoir faire*.³⁶ It is unlikely, moreover, that envoys from the most economically advanced parts of northern Europe were ignorant of financial negotiations and contracts. Hellwig's view rests upon an obsolete concept of economic life at the beginning of the thirteenth century.

If the crusaders, as would normally be expected, stated the number of men for whom transportation was desired, the Venetians presumably suggested the price, which, it has been charged, was extortionate. Fortunately there are two extant transportation contracts for crusaders from the late twelfth century that shed some light on the cost of the contract of 1201. In 1184 Genoa agreed to carry thirteen knights, twenty-six horses, and twenty-six squires provisioned for eight months at a cost of eight and a half marks for each theoretical unit of one knight, two horses, and two squires.³⁷ In 1190 Philip Augustus contracted with Genoa to ship his crusading army of 650 knights, 1,300 horses, and 1,300 squires and to maintain them for eight months for a total price of

³² Tafel and Thomas, eds., *Urkunden*, 1: 365-7.

³³ Lane, *Venice*, 37.

³⁴ Martin Hellwig, "Die ritterliche Welt in der französischen Geschichtsschreibung des vierten Kreuzzuges," *Romanische Forschungen*, 52 (1938), 1-40, provides the clearest statement of the position.

³⁵ Villehardouin, *Conquête de Constantinople*, 1: 22-4; Morris, "Geoffrey de Villehardouin," 30-1.

³⁶ Faral, in Villehardouin, *Conquête de Constantinople*, 1: vii. During the crusade and after we find him charged with many diplomatic and military responsibilities. For a list of his missions, see Queller, "L'évolution du rôle de l'ambassadeur," 482-3, n. 11, although no. 2 should be properly attributed to Geoffroy de Joinville, rather than to Villehardouin.

³⁷ Auguste Jal, *Memoire sur quelques documents Genoïses relatifs aux deux croisades de Saint Louis* (Paris, 1842), 41. The numerical comparisons were made, although outside a diversion context, by Ronald P. Grossman, "The Financing of the Crusades," (Ph.D. dissertation, University of Chicago, 1965), 17-8.

5,580 marks.³⁸ In this instance, each theoretical unit of the same sort cost Philip nine marks. Extending these eight-month contracts to a year to make them comparable to the treaty of 1201 would have raised the price of the contract of 1184 to twelve and three-quarters marks per unit and that of Philip Augustus to thirteen and a half marks.

The contract of 1201 stipulated transportation and a year's provisions for 4,500 knights, 4,500 horses, 9,000 squires, and 20,000 foot soldiers for 85,000 marks. This price figures out to two marks per man and four marks per horse.³⁹ Under these terms one knight, two horses, and two squires would cost fourteen marks. Hence, the Venetian price was only half a mark per unit higher than the Genoese charge of ten years earlier. The French, moreover, had not been satisfied with Genoese performance,⁴⁰ while our sources have only praise for the fleet prepared by Venice.⁴¹ Though higher than the Genoese price, the Venetian charge does not seem extortionate; it may only have taken into account the additional cost required to assure satisfactory service.

The preparation of a fleet of this magnitude represented a massive effort on the part of the Venetians. We may estimate the size of the fleet needed and, in fact, ready and waiting for the crusaders in June 1202 as about five hundred vessels, not counting petty auxiliary craft.⁴² To appreciate the magnitude of this undertaking, it is instructive to contrast it with the vastly inferior capabilities of the famed Venetian Arsenal in the fourteenth and fifteenth centuries.⁴³ Although the Venetian peacetime fleet was large, many additional transports and galleys had to be built.⁴⁴ The horse transports probably ran up the cost, moreover, because of the size of the animals and the special outfitting required to load and unload them and to keep them from being pitched about with the rolling of the ships.⁴⁵ In addition, many of these vessels, especially the

³⁸ Cesare Imperiale di Sant' Angelo, ed., *Codice diplomatico della repubblica di Genova* (Rome, 1926-42), 2: 364-8.

³⁹ A higher price was assigned to the horses because of their size and the special accommodations required for them. *Infra*, pp. 724-5.

⁴⁰ H. Vriens, "De kwestie van den vierden kruistocht," *Tijdschrift voor Geschiedenis*, 37 (1922), 61.

⁴¹ "Et li navies que il orent appareille fu si riches et si bels que onques nus hon crestiens plus bel ne plus riche ne vit; si cum de nes et de galies et de uissiers bien a troiz tanz que it n'aust en l'ost de genz." Villehardouin, *Conquête de Constantinople*, 1: 58. "Quant le pelerin furent tot asanle en Venice et il virent le rike navie que faite estoit, les rikes nes, les grans dromons et les uissiers a mener les chavax et les galies, si s'en merveillierent molt et de le grant riqueche que il troverent en le vile." Robert of Clari, *La conquête de Constantinople*, ed. by Philippe Lauer (Paris, 1924), 9.

⁴² The fleet actually used numbered about 200 ships. The *Devastatio Constantinopolitana*, ed. by Charles Hopf in *Chroniques Gréco-Romanes* (Berlin, 1873), 87 (also in *M.G.H.*, SS, 16: 10); and Hugh of St. Pol, *Epistola*, in Tafel and Thomas, eds., *Urkunden*, 1: 306, are very near to that number. Nicetas Choniates, *Historia*, 714, estimated slightly over 240. Villehardouin informs us that there were ships ready for three times as many crusaders as actually appeared in Venice. *Conquête de Constantinople*, 1: 58. The fifty Venetian galleys were not for transport, of course, so there were about 150 transports in the actual fleet, and, if Villehardouin is correct, some 450 transports prepared for men and horses—plus the 50 galleys.

⁴³ Frederic C. Lane, *Venetian Ships and Shipbuilders of the Renaissance* (Baltimore, 1934), 132-5. I have corresponded with Lane concerning my calculations, and I believe that he finds them reasonable, although he cautions that Villehardouin's "three times" may be a landlubber's guess. Villehardouin, unlike many medieval writers, however, customarily uses numbers carefully and accurately.

⁴⁴ Before leaving Venice in the spring of 1201 the crusaders' envoys borrowed money from a Venetian bank to enable Venice to begin construction of the fleet. Villehardouin, *Conquête de Constantinople*, 1: 32.

⁴⁵ Michel Mollat, "Problèmes navals de l'histoire des croisades," *Cahiers de civilisation médiévale*, 10 (1967), 352-3.

horse transports, were not likely to be of subsequent use. It might be years before an expedition of comparable size would sail by way of Venice, and in the meantime these wooden ships would either rot away or require extensive repair. Lane estimates the average life expectancy of a Venetian vessel at ten years.⁴⁶ Very few, if any, of the Fourth Crusade ships, therefore, would have been usable even for the Fifth Crusade. The rapid decay of ships precluded also the use for the Fourth Crusade of those ships that had been built for the Third Crusade.

Robert of Clari gives two glimpses of the sacrifices that the Venetians made to fulfill their contractual obligations. He relates that Venice had to conscript one-half its manpower to arm and man the galleys and the transports.⁴⁷ If we accept Carile's estimate that a fleet of this size required a complement of 17,264 men,⁴⁸ and we consider that the population of Venice, young men and old, women and children, was probably less than 100,000,⁴⁹ Robert's account gains credence.

Robert also mentions twice an interdiction of a year and a half that the Venetians imposed upon their trade to prepare for the crusade.⁵⁰ Although Robert is unreliable for events before his own arrival in Venice, the second mention of the interdiction comes after that time when the doge was complaining to the crusaders about their inability to pay their debt. This prohibition represents a considerable sacrifice for a city that lived by its trade, but such a suspension, as we have seen, was not an unheard-of measure. In 1162 and 1163 Genoa called off all trade with the Levant to prepare for an attack in alliance with Barbarossa against Norman Sicily, although the emperor later backed out of the undertaking.⁵¹ More to our point, in 1188-89 the Venetians cancelled trading ventures in preparation for the Third Crusade.⁵² Later, in 1215, the Fourth Lateran Council attempted to call off Levantine commerce for four years to ensure sufficient shipping for the Fifth Crusade.⁵³

The Venetian state was accustomed to regulating its merchant fleets.⁵⁴

⁴⁶ Lane, *Venetian Ships and Shipbuilders*, 263.

⁴⁷ *Conquête de Constantinople*, 9.

⁴⁸ Antonio Carile, "Alle origini dell'impero de'Oriente: analisi quantitativa dell'esercito crociato e ripartizione dei feudi," *Nuova rivista storica*, 56 (1972), 287-8. We think it may be high, but, in any case, the burden was very heavy.

⁴⁹ Lane, *Venice*, 36. Thiriet estimates the Venetian population at 50,000. *Romanie vénitienne*, 66, n. 1. In all fairness we must add that Venice also drew manpower from its Adriatic possessions, although we have no way of knowing the numbers. C. Manfroni, *Storia della marina italiana dalle invasioni barbariche alla caduta di Costantinopoli* (Livorno, 1899), 1: 317-8.

⁵⁰ *Conquête de Constantinople*, 8 and 10.

⁵¹ Yves Renouard, *Les hommes d'affaires italiens au moyen âge* (Paris, 1968), 68-9.

⁵² Dandolo, *Chronica*, 270. For the actual order requiring all Venetian merchants to return to Venice by Easter 1189, see Tafel and Thomas, eds., *Urkunden*, 1: 204-6.

⁵³ Hans Eberhard Mayer, *The Crusades*, (Oxford, 1972), 208.

⁵⁴ Lane, *Venice*, 49-50. The Venetian contracts concerning the ship *Paradiso*, sailing to Syria in the summer of 1201, however, indicate that the prohibition was not absolutely enforced. Raimondo Morozzo della Rocca and Antonio Lombardo, eds., *Documenti del commercio veneziano nei secoli XI-XIII* (Turin, 1940), 1: 451-2; Raimondo Morozzo della Rocca and Antonio Lombardo, eds., *Nuovi documenti del commercio veneto dei secoli XI-XIII*, in *Monumenti Storici della Deputazione di Storia Patria* (Venice, 1953), 7: 59-60. There are several possible explanations for this discrepancy. The possibility that Robert may have been wrong is conceded, but just as likely, the *Paradiso* could have sailed unlawfully, or especially, considering the huge size of this ship, it may have been allowed to sail if preparations for its departure had been in progress when the doge announced his ban. It is worth noting in regard to the last possibility that the *Paradiso* was one of the great

What the cessation probably amounted to was the cancellation of one or both voyages of 1201. If the spring convoy, which usually set out sometime between Easter and the end of May, had not yet departed in 1201 before the conclusion of the treaty, then it was cancelled. The summer convoy, which should have departed between June 24 and the end of September, was surely kept home.⁵⁵ Voyages apart from the convoys, of course, were also cancelled. Since the sailing date fixed by the treaty was June 29, 1202,⁵⁶ although the fleet did not actually set out until the beginning of October,⁵⁷ the crusade itself took the place of the spring sailing, and perhaps also the summer convoy of that year. The loss of revenue from the cancelled voyages would have been a severe drain upon the Venetian economy if it could not be made up in some way.

The Venetian claim to half the conquests to be made by the crusade was hardly excessive. This reward was in return for the fifty galleys and Venetian participation as crusaders, above and beyond the provision of transportation.⁵⁸ The Venetians drafted half their adult males to go on the expedition.⁵⁹ When the crusade finally got under way there were probably approximately three Venetians for every two crusaders.⁶⁰ In the fighting before Constantinople in 1203 and 1204 the Venetians acted as the equal allies of the northern crusaders, as they were, according to the terms of the treaty.⁶¹

Since Venice had sacrificed so much in its expenditure of labor, material, money, expectation of profits, and men, one can easily understand Dandolo's anger when the host could not pay the agreed carrying fee. The doge spoke to the crusaders assembled in Venice, stressing his city's prodigious commitment, and he threatened to cut off their supplies unless they paid up. Rather than being part of an overall plan to force the crusaders to submit to Venetian direction, Dandolo's threat represents simply the angry reaction of a man who saw his city's efforts and investment perishing for nothing. Like the honorable and wise man that he was, however, when his temper abated he forgot his vain threat, and he began searching for a more realistic solution.⁶²

A solution, indeed, had to be found quickly. The crusaders were gathered on the island of San Nicolò di Lido in a sort of limbo brought on by their penury. They could neither pay to sail nor afford to stay. From the high prices commanded for food, one can conclude not only that individual Venetians were profiteering,⁶³ which they probably were, but that Venice was having a

transports on the crusade and it played a hero's role. Villehardouin, *Conquête de Constantinople*, 2:44. We owe the reference to the *Paradiso* and to the commercial researches to Louise Buenger Robbert.

⁵⁵ For the customary dates, Wilhelm Heyd, *Histoire du commerce du Levant au moyen âge* (Amsterdam, 1959), 1: 180-1.

⁵⁶ Tafel and Thomas, eds., *Urkunden*, 1: 366.

⁵⁷ Villehardouin, *Conquête de Constantinople*, 1: 76; *Devastatio*, 87 (*M.G.H.*, SS, 16:10); *Gesta episcoporum Halberstadensium*, ed. by L. Weiland, *M.G.H.*, SS., 23: 117. There is a discrepancy of as much as a week in the date.

⁵⁸ Villehardouin, *Conquête de Constantinople*, 1: 24.

⁵⁹ Robert of Clari, *Conquête de Constantinople*, 9.

⁶⁰ *Supra*, p. 725, for 17,264 Venetians. *Supra*, n. 41 for one-third of the anticipated 33,500 crusaders.

⁶¹ For example: Villehardouin, *Conquête de Constantinople*, 1: 162, 174-8; 2: 10, 34, 44.

⁶² Robert of Clari, *Conquête de Constantinople*, 10.

⁶³ *Devastatio*, 87 (*M.G.H.*, SS., 16:10). Poor harvests in recent years also undoubtedly affected the price of food. Robert of Auxerre, *Chronicon*, ed. by O. Holder-Egger, *M.G.H.*, SS., 26: 261.

difficult time supplying the army without breaking into the year's provisions intended for the journey and the campaign.⁶⁴ Though the crusaders seemed to be imprisoned on the island,⁶⁵ the Venetians were not responsible. The crusaders were locked in by their own inability to pay. Robert of Clari stated, moreover, that it was they themselves who had chosen to camp on the Lido,⁶⁶ although the anti-Venetian *Devastatio* complains that they were deliberately isolated by their hosts.⁶⁷ Whoever made the choice, it was a reasonable one. Venice proper certainly could not house an anticipated army more than one-third the size of the population of the city, and that number of soldiers with nothing but time on their hands would have constituted a considerable threat to the peace of the citizens.

The encampment on the Lido would have been suitable for the short sojourn that had been anticipated, but the crusaders' stay was protracted for months, while the early arrivals awaited the latecomers. The scheduled date of departure, June 29, passed. The papal legate did not arrive until July 22.⁶⁸ Near the end of July Abbot Martin of Pairis also arrived at the head of 1200 Germans.⁶⁹ The bishop of Halberstadt came on August 13.⁷⁰ The chief of the crusade, Boniface of Montferrat, did not reach Venice until August 15.⁷¹ Within three weeks of the arrival of the marquis of Montferrat a solution had been found, a make-shift plan to extricate the army from the Lido while regaining Zara for Venice.⁷²

Critics of the Venetians' role in the Fourth Crusade seem to feel that they should simply have absorbed their losses and sailed off on a wave of crusading enthusiasm, a suggestion that is neither realistic nor just. It is true, of course, that Venice had a strong interest in the reconquest of Zara and that the assistance of the crusaders against the city was made a *quid pro quo* for putting the crusade to sea with 34,000 marks of the debt to Venice outstanding.⁷³ Zara was not intended, however, as a substitution for the crusading goal, but as an excursion on the way. Such attacks against Christian enemies were common for Italian crusading fleets. The Pisan armada of 1099 under Archbishop Daimbert on its way to the Holy Land raided the islands off the Heptanese—Corfu, Leucas, Cephalonia, Zante, and Cyprus.⁷⁴ The Venetian fleet of

⁶⁴ The problem of feeding the crusaders before the embarkation may account for the discrepancy between the provision of the treaty of 1201 that the Venetians would supply food for a year after embarkation (Tafel and Thomas, eds., *Urkunden*, 1: 365) and Villehardouin's statement that they would be provided for nine months (*Conquête de Constantinople*, 1:22).

⁶⁵ *Devastatio*, 87 (*M.G.H.*, SS., 16:10); *Gesta episcoporum Halberstadensium*, 118.

⁶⁶ *Conquête de Constantinople*, 9.

⁶⁷ *Devastatio*, 87 (*M.G.H.*, SS., 16:10).

⁶⁸ *Ibid.*

⁶⁹ Gunther of Pairis, *Historia Constantinopolitana*, 67 and 70. Francis Swietek has convinced us that this figure should be used with caution.

⁷⁰ *Gesta episcoporum Halberstadensium*, 116.

⁷¹ *Devastatio*, 87 (*M.G.H.*, SS., 16:10).

⁷² Villehardouin, *Conquête de Constantinople*, 1:66; Robert of Clari, *Conquête de Constantinople*, 11–2.

⁷³ Villehardouin, *Conquête de Constantinople*, 1: 64. Robert of Clari, who is extremely unreliable on numbers, says 36,000 marks. *Conquête de Constantinople*, 11. Perhaps he is including the 2,000 marks borrowed from Venetian bankers by the crusading envoys after the conclusion of the treaty. Villehardouin, *Conquête de Constantinople*, 1: 32.

⁷⁴ Bernardo Maragone, *Annales Pisani*, ed. by M. Lupo Gentile, in *R.I.S.*², 6, pt. 2: 7. Runciman, *History of the Crusades*, 1: 299–300.

1099 lingered at Zara, for what reasons we are not told, and it fought Daimbert's fleet later in the Cyclades before the Venetians finally arrived in Palestine.⁷⁵ A later Venetian armada raided Corfu on its way to the battle of Ascalon and the capture of Tyre in 1124.⁷⁶ Richard the Lion Heart is well known, of course, for his extracurricular exploits in Sicily and Cyprus. In any case, Dandolo was correct in his assertion that the voyage to Egypt so late in the year was impractical.⁷⁷ Given the cruel situation of the crusaders on the Lido and the dangers to Venice from this large foreign army, it was a reasonable course to get under way for the interim destination of Zara, where supplies would be plentiful, and to winter there until the Mediterranean would again be safe for sailing.⁷⁸ The dictates of necessity coincided with Venetian self-interest in reacquiring Zara.⁷⁹

The delta of the Nile was a legitimate objective upon which the crusaders had had their eyes for at least forty years. In the 1160s King Amalric of Jerusalem led five unsuccessful expeditions there.⁸⁰ Renaud of Chatillon, lord of Transjordan, incensed Saladin in 1183 by his foray into the Red Sea area.⁸¹ Richard I of England had planned an expedition to Egypt as part of his crusade.⁸² Some fifteen years after the Fourth Crusade, the delta was invaded by the army of the Fifth Crusade and later was the scene of St. Louis' first crusade. Western strategists realized that the crusading states would always be in danger as long as the Muslims were able to mount an attack from Egypt upon their precarious position. If the crusaders could split Islamic power by a conquest of Egypt, on the other hand, Jerusalem would easily fall into their hands.⁸³

Other factors too tended to favor the Egyptian goal. In the first place, the Levantine Franks did not want a crusade in the Holy Land. The zealous

⁷⁵ *Translatio Sancti Nicolai*, 256-8.

⁷⁶ *Historia ducum Veneticorum*, ed. by H. Simonsfeld, *M.G.H.*, SS., 14: 73.

⁷⁷ Robert of Clari, *Conquête de Constantinople*, 12. The Venetian fleet for Constantinople, according to an act of 1255, was supposed to have departed by August 15. It wintered abroad. Lane, *Venice*, 69-70.

⁷⁸ Robert of Clari, *Conquête de Constantinople*, 11-2. Riant has charged the Venetians with deliberately delaying the arrival of the fleet in Zara, so that it would be too late to sail overseas. "Innocent III, Philippe de Souabe, et Boniface de Montferrat," *Revue des questions historiques*, 17 (1875), 369. He overlooks the announcement prior to sailing that they would winter at Zara. He was also unaware that the time between departure from Venice and arrival at Zara was taken up with the gathering of supplies and the enlistment of men owed by the various Venetian dependencies in the northern Adriatic. Manfroni, *Storia della marina italiana*, 1: 317-18.

⁷⁹ We do not subscribe to Cessi's view that the crusaders were not to participate in the attack on Zara, which was strictly a Venetian undertaking. "Venezia e la quarta crociata," 24, n. 1 and 27, and "L'eredità di Enrico Dandolo," 6, n. 1. Responsible crusading leaders could not have failed to recognize that they could not accompany the Venetians to Zara without becoming involved. We know of no scholar who has accepted Cessi's view.

⁸⁰ William of Tyre, *Historia rerum in partibus transmarinis gestarum*, in *Recueil des historiens des croisades, historiens occidentaux*, 1: 890-4, 902-13, 948-58, 962-9, 971-9.

⁸¹ Runciman, *History of the Crusades*, 2: 436-7. He dates the expedition to 1182, although other authorities give 1183. Hamilton A. R. Gibb, "The Rise of Saladin," in Setton, ed., *History of the Crusades*, 1: 582; Joshua Prawer, *The Crusaders' Kingdom: European Colonialism in the Middle Ages* (New York, 1972), 28. Ernoul-Bernard is the only western source to mention the expedition. Ernoul and Bernard le Trésorier, *Chronique*, ed. by Louis de Mas Latrie (Paris, 1871), 69-70.

⁸² Imperiale, ed., *Codice diplomatico*, 3: 20-2.

⁸³ J. J. Saunders, *Aspects of the Crusades* (Christchurch, New Zealand, 1968), 38.

Renaud of Dampierre, who by-passed the rendezvous at Venice in favor of a direct passage from Apulia to Syria, discovered that the Kingdom of Jerusalem was living under a truce with its infidel neighbors and had no desire for war-minded newcomers to upset the tenuous *modus vivendi*.⁸⁴ Another factor contributing to the decision to attack Egypt was the famine that had fallen over the Nile region for five years.⁸⁵ It certainly must have diminished Egypt's defensive strength. Tessier argued that the scarcity of food would have deterred the crusaders from an Egyptian campaign, but he seems to have forgotten that the crusaders were to be supplied by the Venetians in contrast to the usual method of living off the land.⁸⁶ The Egyptian famine, therefore, made the country a promising target for Western attack.⁸⁷

Unfriendly critics have accused the Venetians of diverting the crusade from this legitimate goal to Christian Constantinople for the sake of protecting their Egyptian commerce. The chronicle of Ernoul and Bernard the Treasurer, a source from the Latin Kingdom, claimed that the Egyptian government negotiated with the Venetians to this end.⁸⁸ The editor of the chronicle, Louis de Mas Latrie, picked up the argument,⁸⁹ and a few years later Carl Hopf appeared to have discovered the treaty by which Venice agreed to divert the crusade.⁹⁰ In 1877, however, Gabriel Hanotaux showed that Hopf's documentation consisted of well-known commercial treaties that could not be dated prior to the Fourth Crusade.⁹¹ Although there has been considerable reluctance to abandon the idea of collusion between Venice and the sultan,⁹² it does not have a sound foundation.

Actually, Venice stood to profit enormously from a Latin conquest of Alexandria. Egypt was a very important center for medieval Mediterranean commerce. The land was at once the terminus and the distribution point for the exotic goods from the Orient via the Red Sea. At Cairo the major trade artery divided into three branches: one branch followed the North African coastline to Tunis, Tripoli, and Mehadia on to Muslim Spain and Sicily; the second went to Alexandria, where goods were transported to Ascalon, Acre, Tyre, Tripoli, Lattakiah, or Constantinople; the third route went overland

⁸⁴ Villehardouin, *Conquête de Constantinople*, 1: 56; Ernoul-Bernard, *Chronique*, 340-1; Gunther of Pairis, *Historia*, 70.

⁸⁵ Gunther of Pairis, *Historia*, 71.

⁸⁶ Jules Tessier, *La quatrième croisade: la diversion sur Zaza et Constantinople* (Paris, 1884), 48.

⁸⁷ The famine was followed by a devastating plague in 1201 and 1202, but this calamity was in the future after the agreement to attack Egypt had been made. It has been reported that the plague was the second greatest demographic disaster in Egypt's medieval history. E. Ashtor, *A Social and Economic History of the Near East in the Middle Ages* (Berkeley, 1976), 238. Is it possible that the plague became a factor in the later decision to go to Constantinople? There is no evidence.

⁸⁸ Ernoul-Bernard, *Chronique*, 345-6.

⁸⁹ Mas Latrie, *Histoire de l'île de Chypre*, 1: 161-4.

⁹⁰ Carl Hopf, *Geschichte Griechenlands vom Beginn des Mittelalters bis auf unsere Zeit*, in Ersch-Gruber, *Encyclopädie*, vols. 85-6 (1867-8), 188.

⁹¹ Gabriel Hanotaux, "Les Venitiens ont-ils trahi la Chrétienté en 1202?" *Revue historique*, 4 (1877), 74-102. The pacts are published in Tafel and Thomas, eds., *Urkunden*, 2: 184-93.

⁹² Runciman, not unaware of Hanotaux's article, still appears reluctant to give up a Veneto-Egyptian plot. *History of the Crusades*, 3: 113-4 and n. 2.

across the Sinai peninsula to the Islamic cities of the Syro-Palestinian interior.⁹³

The disadvantage to Western merchants in obtaining their precious goods from the Orient in such intermediate markets as Sicily, Constantinople, or the Christian Levantine ports was, of course, the payment of an inflated price. Using quotations of pepper prices in the Cairo Geniza documents, Goitein has demonstrated that prices doubled between Cairo and other points along the trade routes, and he estimates that as much as 80 percent of the increase was pure profit for Egyptian merchants.⁹⁴ For a long time, however, the Latins had to be content with picking up the products of the East in these intermediate ports.

In the late twelfth century the merchants of Pisa, Genoa, and Venice opened a significant direct trade with Egypt, although it was still a small part of the total overseas commerce. We know that the Pisans, in spite of a papal ban, were supplying the Egyptians with war material in 1154.⁹⁵ For Genoa and Venice we have some quantitative evidence. Prawer reports that the notarial records of the Genoese Giovanni Scriba list 58 voyages to Egypt between 1154 and 1164 out of a total of 335 contracts, i.e., 17.3 percent.⁹⁶ Louise Buenger Robbert has kindly provided us with the following figures based on published Venetian commercial documents. Of 247 surviving twelfth-century contracts for Venetian business in the Eastern Mediterranean, only thirty concerned Egypt, or 12.3 percent.⁹⁷ Attention might also be drawn to the papal grant of the Venetian request in 1198 that the Republic might carry on trade with Muslim Egypt in nonstrategic goods.⁹⁸

Although the Venetians, at least, had certainly engaged in direct trade with Egypt since the ninth century, when two merchants from the lagoons pilfered the body of Saint Mark, it was Saladin who really opened his land to western merchants. Part of the sultan's overall policy of strengthening Egypt was to rebuild his navy to its Fatimid splendor, but Saladin's plans depended upon the importation from the West of ship-building materials scarce in Egypt, such as wood, pitch, and iron. To entice Italian merchants to bring him naval supplies, and to draw their attention away from the crusader states, he offered them trading concessions.⁹⁹ The Pisans signed a commercial agreement with

⁹³ Heyd, *Histoire du commerce*, 1: 378-84; S. D. Goitein, *A Mediterranean Society: The Jewish Communities of the Arab World as Portrayed in the Documents of the Cairo Geniza* (Berkeley, 1967), 1: 212; S. Y. Labib, *Handelsgeschichte Ägyptens im Spätmittelalter (1171-1517)*, in *Vierteljahrschrift für Sozial- und Wirtschaftsgeschichte*, Beiheft (Weisbaden, 1965), Karte I; Adolf Schaube, *Handelsgeschichte der romanischen Völker des Mittelmeergebiets bis zum Ende der Kreuzzüge* (Munich and Berlin, 1906), 22.

⁹⁴ Goitein, *Mediterranean Society*, 1: 222.

⁹⁵ Robert-Henri Bautier, *The Economic Development of Medieval Europe*, trans. by Heather Karolyi (London, 1971), 101.

⁹⁶ Prawer, *The Crusaders' Kingdom*, 399.

⁹⁷ Her figures are based on Morozzo della Rocca and Lombardo, eds., *Documenti del commercio veneziano*, and Morozzo della Rocca and Lombardo, eds., *Nuovi documenti del commercio veneto*.

⁹⁸ Tafel and Thomas, eds., *Urkunden*, 1: 234-5.

⁹⁹ A. S. Ehrenkreutz, "The Place of Saladin in the Naval History of the Mediterranean Sea in the Middle Ages," *Journal of the American Oriental Society*, 75 (1955), 100-1; Gibb, "Rise of Saladin," 583-4; Roberto Cessi, *Le colonie italiane medievali in Oriente* (Bologna, 1942), 1: 75-6.

Saladin in 1173 that gave them free trade and security throughout Egypt; additional negotiations in 1177 and 1179 further enhanced their position.¹⁰⁰ The Venetians gained their privileges in 1175,¹⁰¹ and the Genoese obtained theirs in 1177.¹⁰² Saladin was therefore able to boast to the caliph in Baghdad that "treaties of peaceful intercourse have been negotiated with them all."¹⁰³

The embryonic Latin establishments at Alexandria, however, were quite precarious. The westerners had recourse neither to friendly political authority nor to religious sanctions if they fell afoul of the Muslim government. We have considerable evidence of their troubles in Egypt. In 1195 the Egyptians released some Venetians who had been held captive.¹⁰⁴

It appears that the Pisans also had difficulty, for an Egyptian edict from the end of the twelfth century restored the Pisan *fondaco* at Alexandria and promised to protect their goods and privileges there.¹⁰⁵ In 1200 the Genoese tried unsuccessfully to obtain the release of some of their fellow citizens imprisoned by the sultan.¹⁰⁶ A few years after the Fourth Crusade in 1208, Pisan and Venetian merchants whom al-Adil had detained were requesting that the sultan allow them to return home.¹⁰⁷

Although Venice improved its position in Egypt a few years after the Fourth Crusade by concluding a series of commercial agreements with the Ayubbid government,¹⁰⁸ the city nevertheless would have gained much more from a successful crusade against the delta. These treaties contained little more than confirmation of existing property rights and the customary vague promises of security for Venetian merchants in Egypt except for the gain of a second *fondaco* and a Venetian *podestà* at Alexandria.¹⁰⁹ The treaty with the crusaders, however, allocated to the Venetians one-half of all the conquests as their own colonies.¹¹⁰ This provision would have entailed, of course, control over taxation, Venetian courts and law, and whatever else political authority might do to serve the interests of Venetian merchants.

Had the Venetians gained such a colony in Egypt, they could have siphoned off the lucrative trade via the Red Sea where it first entered the Mediterranean area and where goods were cheapest. They could have consolidated their

¹⁰⁰ Reinhold Röhricht, ed., *Regesta regni Hierosolymitani* (Innsbruck, 1893-1904), 1: 132, 144, 152; Cessi, *Colonie medievali*, 1: 77.

¹⁰¹ *Historia ducum veneticorum*, 81.

¹⁰² L. T. Belgrano and Cesare Imperiale di Sant' Angelo, eds., *Caffari et continuatorum Annales Januenses* (Rome, 1890-1901), 2: 11.

¹⁰³ Quoted by Gibb, "R.se of Saladin," 584. Curiously, Benjamin of Tudela, a Jewish traveler in Egypt during the early years of Saladin's reign, does not include the Venetians in his list of Italian merchants doing business there. Although this omission does not prove that Venetians were not trading there, it does suggest that they did not play a dominant role among the European traders. Benjamin of Tudela, *The Itinerary*, ed. and trans. by A. Asher (London, 1840-1), 1: 157. Benjamin does mention "Valencia," which some think may be a corruption of "Venetia," although this conjecture seems to us a quite arbitrary emendation.

¹⁰⁴ Labib, *Handelsgeschichte Ägyptens*, 29.

¹⁰⁵ Röhricht, ed., *Regesta regni Hierosolymitani*, 1: 204-5. The document cannot be dated precisely.

¹⁰⁶ Belgrano and Imperiale, eds., *Annales Januenses*, 2: 79.

¹⁰⁷ Röhricht, ed., *Regesta regni Hierosolymitani*, 1: 223.

¹⁰⁸ *Supra*, p. 729 and n. 91.

¹⁰⁹ Tafel and Thomas, eds., *Urkunden*, 2: 189.

¹¹⁰ *Ibid.*, 1: 367.

trade there and rested easy from the unsettled political conditions affecting the city's far-flung entrepôts used in the twelfth century to gather goods from the East. The agreement with the northern crusaders to direct the Fourth Crusade toward Egypt, therefore, offered extremely enticing prospects to Venice.

The theory that Venice diverted the Fourth Crusade to Constantinople is based largely on the belief that the Byzantine capital had an overwhelming economic importance for the city on the lagoons. While one cannot doubt the crucial role that Byzantium played in Venice's commercial expansion during the eleventh and twelfth centuries, one can question whether the Queen of the Bosphorus still enjoyed unchallenged commercial supremacy on the eve of the Fourth Crusade and whether the Venetians, in fact, sought their commercial advantage by her conquest.

In the first place, while Byzantium reaped the fruits of its advantageous geographical position as an entrepôt for interregional commerce, it did not enjoy an overwhelmingly important industrial economy. In fact, one searches almost in vain to find anything produced or manufactured within the empire itself that was salable in quantity on the world market. It produced gold and silver articles, which were too expensive for volume trade; armaments, which by their very nature were not for sale to rivals; and silk garments, whose manufacture, sale, and export were strictly regulated by the state, so that silk could not contribute much to the overall economy.¹¹¹ In short, Byzantine wealth did not result from widespread industry.

Constantinople's economic prosperity had always rested upon its role as the marketplace of the Mediterranean. To the shores of the Bosphorus came the goods of the Islamic lands and the exotic Orient beyond for transshipment to the West. At the end of the twelfth century, however, the city's domination of interregional trade had weakened. We have no quarrel with M. F. Hendy's revisionist thesis that in the eleventh and twelfth centuries the Byzantine economy was expanding rather than declining.¹¹² Hendy treats the period en bloc, while we are concerned here with the years after the Third Crusade relative to the rest of the twelfth century. Hendy agrees with other economic historians, moreover, that the Italians dominated and exploited Byzantine commerce in the twelfth century.¹¹³ Once Constantinople's position had been weakened by the rise of other Levantine ports, its usefulness to the Italians diminished.

The establishment of the crusader states drained off a considerable amount

¹¹¹ Runciman, "Byzantine Trade and Industry," 103-7. See also Robert S. Lopez, "Silk Industry in the Byzantine Empire," *Speculum*, 20 (1945), 1-42.

¹¹² M. F. Hendy, "Byzantium, 1081-1204: An Economic Reappraisal," *Transactions of the Royal Historical Society*, ser. 5, 20 (1970), 31-52. Actually Hendy makes a number of points that tend to support our view. For an extreme expression of the traditional position, see Thompson, *Economic and Social History of the Middle Ages*, 414.

¹¹³ On the continued existence of commerce carried by Byzantines in the twelfth century, see Hendy, "Byzantium," 40, and Runciman, "Byzantine Trade and Industry," 102-3. We see no reason, nevertheless, to consider invalid the generally held view that Italians dominated the interregional trade of the later twelfth century.

of business from the once-proud Byzantine emporium. Many of the goods traded at Constantinople came through what were in the twelfth century crusader ports. Antioch (or more specifically, its ports of Lattakiah and St. Simeon) had become especially important, serving as the Mediterranean outlet for goods coming up the Euphrates to Aleppo from Baghdad and the East. Tyre and Acre both received wares from the East distributed at Damascus, besides possessing their own thriving industries.¹¹⁴ Antioch, Tripoli, Tyre, and Acre were now in the hands of western barons who had granted liberal concessions, indeed, in the course of the Third Crusade, virtual autonomy, to the Italians, whose sea power protected the fragile Latin kingdom.

Genoa, in return for giving aid to the residual crusader forces defending Tyre against Saladin, received from Conrad of Montferrat extensive property, tax exemptions, and a court at Tyre.¹¹⁵ King Guy at the siege of Acre, apparently in an attempt to draw support away from his rival, Conrad, as much as to assure the Italians' continued support of the siege, confirmed Genoa's pre-Saladinic possessions at Acre and included exemption from taxes.¹¹⁶ In order to outdo Guy, Conrad later added an extraterritorial court at Acre to Genoa's privileges.¹¹⁷ Henry of Champagne, regent of the kingdom, ratified these agreements for Tyre in 1192 and for Acre in 1195.¹¹⁸

Much the same story holds for the Pisans. Conrad of Montferrat granted Pisa its pre-Saladinic possessions in the kingdom in 1187 and 1189 along with some additional property and a 2,000 bezant annuity from the port and market dues at Acre.¹¹⁹ King Guy, allied with the Pisans against Conrad and the Genoese, confirmed Conrad's grant to the Pisans.¹²⁰ Conrad reconfirmed Pisa's Tyrian possessions in 1191.¹²¹ Finally, Henry of Champagne in 1193 confirmed once more all Pisa's properties and privileges and added a tax exemption at Acre.¹²²

By the end of the twelfth century, moreover, the Armenian national leader, Roupen, had carved out a kingdom in southern Cilicia independent of Byzantium and hostile to her.¹²³ Italian merchants flocked to the new Armenian court to obtain trading concessions in the kingdom. In March 1201, the Genoese gained liberal commercial privileges at the ports of Mamistra, Tarsus, and Sis,¹²⁴ and in December the Venetians followed suit.¹²⁵ These two Italian cities were thus able to capture the commerce coming into the Medi-

¹¹⁴ Heyd, *Histoire du commerce*, 1: 164-182; Prawer, *The Crusaders' Kingdom*, 391-7; Thompson, *Economic and Social History of the Middle Ages*, 404.

¹¹⁵ Imperiale, *Codice diplomatico*, 2: 369-72.

¹¹⁶ *Ibid.*, 2: 374-6; 3: 22-3.

¹¹⁷ *Ibid.*, 3: 48-50.

¹¹⁸ *Ibid.*, 3: 87-9, 113-5.

¹¹⁹ Röhrich, ed., *Regesta regni Hierosolymitani*, 1: 177, 178, 180.

¹²⁰ *Ibid.*, 1: 182.

¹²¹ *Ibid.*, 1: 188.

¹²² *Ibid.*, 1: 191.

¹²³ Sirarpie der Nersessian, "The Kingdom of Cilician Armenia," in Setton, ed., *History of the Crusades*, 2: 643-4.

¹²⁴ *Liber iurium rei publicae Genuensis*, vol. 1, ed. by Ercole Ricotti, in *Historiae patriae monumenta*, 7: 468-70.

¹²⁵ Tafel and Thomas, eds., *Urkunden*, 1: 381-5.

terranean through Armenian ports that had before been shipped by sea or overland to Constantinople for world purchase.¹²⁶

Cyprus had always been a focal point for the trade of the eastern Mediterranean, lying as it did on the crossroads of commerce coming from the Syrian littoral or Egypt to Constantinople. In 1191 Richard the Lion Heart conquered the island and sold it to the house of Lusignan as a lordship, which later became a kingdom in 1197.¹²⁷ This kingdom provided the Italians with another friendly location where they could establish alternative trading centers to Constantinople.

To sum up the diplomatic and commercial situation in the eastern Mediterranean for the period immediately prior to the Fourth Crusade, the Italian cities, and especially Genoa and Pisa with their profitable exploitation of the Third Crusade and its factions, were gaining control of the sources of the trade upon which Constantinople's economic well-being depended. Hendy has remarked: "By its very geographical situation Constantinople straddled a main north-south trade axis, and as long as the Empire held most or all of the strategic ports of south Asia Minor, Cyprus, Crete, and the southern Peloponnese, it stood to derive similar benefits from the main east-west one."¹²⁸ By the end of the twelfth century, however, Byzantium no longer exercised such hegemony, and new entrepôts for the trade from the East had arisen on the Syrian coast. It is conceded that at this early date the empire's Russian and Black Sea trade remained flourishing and free of Italian domination. Venice had the strongest position in Constantinople, but it was at the very moment when the Byzantine capital was fast losing commercial advantage. Far from trying to plunder a sinking ship, the Venetians needed to transship to a sturdier vessel, or at any rate, to diminish their reliance upon the one that was listing.¹²⁹ An argument, therefore, can be made that the Venetians would have preferred a successful expedition to Egypt over one to Constantinople.

Another well-known argument of the anti-Venetian school, derived largely from the Byzantine chronicler Nicetas Choniates, is that Venice intended to divert the crusade to Constantinople because the emperors had been consistently favoring Genoa and Pisa as counterweights to Venetian commercial

¹²⁶ Heyd, *Histoire du commerce*, 1: 367.

¹²⁷ Sidney Painter, "The Third Crusade: Richard the Lionhearted and Philip Augustus," in Setton, ed., *History of the Crusades*, 2: 62-4, 81-2; Edna Chapin Furber, "The Kingdom of Cyprus, 1191-1292," in Setton, ed., *History of the Crusades*, 2: 600-2, 604.

¹²⁸ Hendy, "Byzantium," 40.

¹²⁹ To illustrate how much Byzantine trade dominated the commerce of Venice in the twelfth century, we again rely upon Robbert's figures. *Supra*, n. 97. Out of 247 contracts, 170, or more than two out of three, involved Constantinople or some place else in the Byzantine Empire. Broken down into the periods 1150-1183 and 1184-1205, the figures show Byzantine Empire, 100 and 60, respectively; crusader states and Egypt, 47 and 32, respectively. For the first period 29 percent of Venice's commerce was non-Byzantine and for the second period 35 percent. Venetian commerce with the Levant outside of Byzantium, therefore, constituted only a small part of her total commerce and did not radically increase relative to that with Byzantium. The contracts concerning Egypt make up only 13 percent of the total for 1150-1183 (20 out of 155) and 11 percent for 1184-1205 (10 out of 92). Thiriet, with quite a different intention, has pointed out how very few Venetian contracts for commerce in the Byzantine Empire there were at the end of the twelfth and the beginning of the thirteenth centuries. *Romanie vénitienne*, 61, n. 1.

power. Fearing competition, the Venetians sought to gain monopoly by conquest.¹³⁰ A consideration of Venetian policies toward the Pisans and the Genoese in the capital after its conquest casts grave doubts upon this thesis. The pact concluded between the Venetians and the crusaders concerning the division of the spoils, if they should conquer Constantinople, does not name any group of merchants to be expelled. The treaty stipulates only that anyone at war with Venice is to be excluded from the empire until peace has been made.¹³¹ This clause is an example of the use of commercial leverage for political and military ends, rather than the contrary. Although it cannot be proved positively, the Genoese were probably expelled,¹³² but the cause of this measure was the skirmishing and near war that arose between Venice and Genoa concerning the disposition of Crete. Possession of the island fell to Boniface of Montferrat, who had intended to sell it to the Genoese, until the Venetians, anticipating the sale, purchased it from him.¹³³ The dismayed Genoese gave considerable support to a native Cretan noble, Count Henry Piscatore of Malta, who plagued Venice for several years in his attempt to win Cretan independence.¹³⁴ What is remarkable, however, is that in 1218, when Genoa and Venice patched up a treaty over Crete, Venice granted Genoa not only re-entrance into the empire, but also all the privileges the Genoese had enjoyed under Alexius III, precisely the status quo immediately prior to the conquest of Constantinople.¹³⁵

The Pisans, whom Nicetas names as rivals of Venice especially favored by Alexius III,¹³⁶ never lost their rights in the empire.¹³⁷ True most of the Pisan facilities had been destroyed in the fires accompanying the Latin conquest,¹³⁸ but the Constantinopolitan Pisans, who had fought staunchly against the crusaders and the Venetians, saved themselves from expulsion by joining the attackers on the eve of the final assault.¹³⁹ It does not appear that Venice had a concerted or permanent policy of driving out competition; its main concern was to protect itself from temporary political enemies.

Even the argument employed by the anti-Venetianists that Dandolo and his people were pursuing a policy of revenge and were seeking restitution for the seizure of their goods and persons in the empire in 1171 can be challenged.¹⁴⁰ There is no doubt that the Venetians suffered terribly, but an agreement for compensation had been reached in 1187,¹⁴¹ and the reparations had been

¹³⁰ Nicetas Choniates, *Historia*, 712-4.

¹³¹ Tafel and Thomas, *Urkunden*, 1: 448, 452.

¹³² Heyd, *Histoire du commerce*, 1: 292.

¹³³ J. K. Fotheringham, "Genoa and the Fourth Crusade," *English Historical Review*, 25 (1910), 38-41.

¹³⁴ Belgrano and Imperiale, *Annales Januenses*, 2: 100-1, 109-10, 114. *Historia ducum veneticorum*, 95, n. 3.

¹³⁵ Tafel and Thomas, eds., *Urkunden*, 2: 202-5.

¹³⁶ Nicetas Choniates, *Historia*, 713.

¹³⁷ Fotheringham says, probably erroneously, that the Pisans were expelled. "Genoa and the Fourth Crusade," 34. Heyd disagrees. *Histoire du commerce*, 1: 290. See also Schaube, *Handelsgeschichte*, 269.

¹³⁸ Nicetas Choniates, *Historia*, 730.

¹³⁹ *Ibid.*, 730-1.

¹⁴⁰ *Historia ducum veneticorum*, 78-80; Dandolo, *Chronica*, 250-1.

¹⁴¹ Tafel and Thomas, eds., *Urkunden*, 1: 206-11.

almost fully paid.¹⁴² The Venetians had not only regained their position in the empire by a series of chrysobulls from Andronicus and Isaac II,¹⁴³ but the one issued by Alexius III in 1199 gave them a situation better than the one they had hitherto enjoyed. It indicates the extensive penetration of the Venetians into the economic life of the empire and establishes those places in which imperial jurisdiction was limited. Venetian freedom in the Byzantine Empire was beginning to approach the favorable situation the Italians enjoyed in the crusader states, where they seem to have been practically immune from local authorities.¹⁴⁴

The treaty of mutual assistance of 1203 between the Venetians and the crusaders, on the one hand, and German envoys on behalf of the young Alexius, the refugee Byzantine prince, on the other, also fails to show the Venetians' interest in improving their position in Constantinople. The terms were as follows: in return for Venetian help in deposing his uncle, Alexius III, Alexius promised to submit the Greek Church to Rome, to pay 200,000 silver marks, to provide provisions for the crusading army, to join the crusade or to send 10,000 men for a year, and to maintain 500 knights in the Holy Land for the duration of his life.¹⁴⁵ Hugh of St. Pol specifies that half of the 200,000 marks would be received by the Venetians,¹⁴⁶ the distribution that would follow from the treaty of 1201. There is no evidence that the Venetians demanded anything more, and the main thrust of the agreement is clearly to aid the Church and the crusade. In fact, after Alexius III had been deposed the Venetians claimed no new privileges in the empire from the new emperor. It was at the request of Alexius IV that the crusaders and their fleet remained at Constantinople after his coronation.¹⁴⁷ Only the delays and the ultimate outright refusal by Alexius to fulfill the promises he had made in the treaty of 1203 prevented the crusaders and the Venetian fleet from proceeding with their intention of making Holy War.

It has not been our intention to whitewash the Venetians, to contend that their motives were pure, or to deny that they saw commercial advantage in the installation of a dependent young refugee upon the Byzantine throne. They were probably as greedy as feudal barons or pious prelates, or for that matter, modern corporate technocrats or teachers of history; no more, no less. By

¹⁴² For the clearest account of the history of the Byzantine indemnification to the Venetians, see Charles M. Brand, *Byzantium Confronts the West, 1180-1204* (Cambridge, Mass., 1968), 199-200. The question remains somewhat confused, but it is clear that payment was at least almost complete, and Dandolo's willingness in 1197 to accept less than full payment and the absence of any mention in the chrysobull of 1199 make one doubt whether nonpayment could have been a cause for Venetian aggression against the emperor.

¹⁴³ Tafel and Thomas, eds., *Urkunden*, 1: 175 (Andronicus, 1184), and 1: 178-203 (Isaac II, 1187). The result was a confirmation of the Venetian position as it had existed before the debacle of 1171.

¹⁴⁴ *Ibid.*, 1: 246-78. While Horatio F. Brown mentions "new privileges" contained in this chrysobull, historians in general have failed to note the significance of Alexius III's concessions. "The Venetians and the Venetian Quarter in Constantinople to the Close of the Twelfth Century," *Journal of Hellenic Studies*, 40 (1920), 88. For Venetian extraterritorial privileges in the crusader states, see Marsilio Zorzi's report of 1243. Tafel and Thomas, eds., *Urkunden*, 2: 351-98.

¹⁴⁵ Villehardouin, *Conquête de Constantinople*, 1: 92-4. The treaty itself is not extant.

¹⁴⁶ Hugh of St. Pol., *Epistola*, in *M.G.H., SS.*, 17: 812.

¹⁴⁷ Villehardouin, *Conquête de Constantinople*, 1: 196-200.

presenting some arguments contrary to those generally heard, however, we have aimed at redressing the one-sided view that pictures the Venetians as the villains of the conquest of Christian Constantinople. Bourgeois as they were, the Venetians possessed an honorable crusading tradition and shared the religious ideals of the northern knights. When their allies proved unable to pay for the contracted fleet, the Venetians were compelled to find a way to obtain compensation and to prevent the dissolution of the crusade. At the very end of the twelfth century and the beginning of the thirteenth, moreover, Alexandria possessed an increasing attraction for Venetian merchants which made the plan to attack it good commercial as well as military strategy. In the Byzantine capital the Venetians still held the most favored position, but the domination of Levantine trade by Constantinople was being challenged by the Italian colonies in the crusader states and in Egypt. All in all, we believe that these arguments call for a reappraisal of the Venetian role in the Fourth Crusade. They indicate, at least, that the widely held view of a cynical Venetian betrayal of the crusade for the sake of profit is one-sided, simplistic, and untenable.

The Right to Rule in England: Depositions and the Kingdom's Authority, 1327-1485

WILLIAM HUSE DUNHAM, JR.
and
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A SERIES OF FIVE DEPOSITIONS between 1327 and 1485 drove the English to conjure up arguments and principles justifying a change in kings. Five sovereigns—Edward II, Richard II, Henry VI, Edward V, and Richard III—lost the throne permanently; and four usurpers—Henry IV, Edward IV, Richard III, and Henry VII—claimed to be “lawfully seised and possessed of the said crown” in their stead. To sanction the removal of an unsatisfactory sovereign and his successor’s usurpation, men of law—both common and canon—devised apologias. These justificatory declarations, taken seriatim, created a doctrine of restraint upon the regal power that eventually became a part of England’s constitutional, or public, law.

On each of these occasions, God and His Englishmen had the chance to choose a new king. As they did so, their spokesmen defined further the right to rule and the authority to govern, thereby limiting the English monarchy, for to define is to curb. The deponents, to justify their deeds, alleged deceit, deviousness, distrust, and “uselessness” in those they removed; and sometimes irresponsibility, bad faith, and a breach of the coronation oath. The deposed kings had governed, they said, without good counsel and consent, or against England’s laws and customs. Positively, the usurpers claimed the crown through inheritance or by God’s will, conquest, or election. Thereby they brought into play and into public law God’s authority, the people’s, and the kingdom’s—authority that was expressed through the three estates of the realm and, finally, through parliament. These indictments of beaten kings and the vindications of their conquerors contained much bad history. In due season they made good law.

Again and again the official apologias raised basic questions about public law and governance. To whom was, or ought, the good king to be responsible? And for what and how much was he himself to be answerable, and to whom? To God alone? Yet that position was hardly satisfactory. To medieval men, the kingdom possessed authority of its own that in several ways complemented God’s, thus suggesting that the king was, in some measure, also

responsible to it and its people. Already in the thirteenth century men of law and politics were writing about the community (or communities) of the realm. Before 1399, drawing from the "estates literature," they had begun to use the phrase, "the estates of the realm," to designate the grades (*gradus*) in the hierarchy of society that were taken to be the components of the kingdom. This new term, estate, was to prove useful for depositional apologies, and in 1398 it was joined by another new one, "the authority of parliament," which eventually absorbed the kingdom's authority—and the people's too. Englishmen did not immediately resolve all the constitutional questions raised by the removal of a king, nor did they do so in 1689 when they erected the modern monarchy on medieval foundations that the Glorious Revolution refurbished. Yet at each deposition, their actions created precedents for future ones, and in their attempts to justify what they had done, they established and then reiterated principles that were, in the end, to form a constitutional doctrine legitimating a right to depose and a right to rule.

To decide just when and how to depose an erring monarch was the highest decision England's governing class had to make. Furthermore, those initiating a deposition had also to persuade a large majority of the men of power to go along with their decision. Otherwise the successor or usurper laid himself open, as did Edward IV and Richard III, to his own removal. For 164 years after 1485 not a single attempt at usurpation or deposition succeeded until Charles I's beheading in 1649; and after James II fled in 1688, no monarch was forced to leave the British throne for 248 years, until Edward VIII's deposition in 1936 drove him into exile.

The formula that men used to justify Edward II's removal in 1327 was brief and simple and in close conformity to law and precedent. Edward III's proclamation at his accession declares that his father "ousted himself from the government of the said realm and has granted and willed that the government of the said realm devolve upon Sir Edward, his eldest son and heir, and that he govern, reign, and be crowned king." Edward II, "formerly king of England," had done this "of his good will and by common counsel and assent of the prelates, earls, and barons, and other nobles, and the whole community of the kingdom." This neat formula rested upon two principles: first, the common law of inheritance by the eldest son, and then the century-old rule that the king acts on matters affecting the kingdom with the counsel and consent of the prelates, magnates, and community of the realm. Hostility toward Edward II personally was widespread, and very few, not even his half-brothers or his son, seemed to care about the wretched man. None would fight on his behalf, perhaps "because of his cruelty and personal faults," and his subjects were ready to give him up as a "useless king" (*rex inutilis*), one politically inept and personally incompetent.¹

¹ S. B. Chrimes and A. L. Brown, eds., *Select Documents of English Constitutional History, 1307-1485* (London, 1961), 38 (no. 24). For the *rex inutilis*—the useless, incompetent, inept, weak, and incorrigible king—and the connection between this concept and early medieval depositional theory, see Edward Peters, *The Shadow King: Rex Inutilis in Medieval Law and Literature, 751-1327* (New Haven, 1970), *passim* and esp. 217, 232, 237-41;

Before staging the final denouement, a number of prelates, earls, barons, and knights met on October 26, 1326, at Bristol with Queen Isabella. "By the assent of the whole community of the said kingdom present there, they unanimously chose the duke [Edward III] as keeper of the said kingdom"—but not yet as king.² King Edward II fled to Wales with the young Despenser, but was captured on November 16 and imprisoned at Kenilworth. A parliament, first summoned to meet on December 14, was prorogued until January 7, 1327, when an assembly—a few chroniclers called it a parliament, others did not—met in Westminster Hall. The details are significant, for they set the pattern of procedures and arguments that would be followed in subsequent depositions.

On Tuesday, January 13, the archbishop of Canterbury read to "all the baronage of the land" six "reasons" why Edward III should succeed his father. One was that Edward II's "person . . . was not sufficient to govern." Another, that evil counselors had misled him, and he had not listened "to good counsel." Instead, he had given himself over to unsuitable pastimes and had neglected "the needs of his kingdom." Besides, he had lost Scotland and lands in Gascony and Ireland. He had injured the Church, imprisoned its clergy, and dilapidated the realm. Above all, Edward II had not wished to do right to all his subjects, "as he is bound by his oath to do"; nor had he "kept the other points of the oath that he took at his coronation." When the archbishop had finished presenting these charges, "all the people assented that he should reign no more, but that his eldest son, the duke of Guienne [Edward III], should reign and bear the crown for him." Thereupon the assembly of prelates and magnates, of knights and burgesses (*a parlamentum* according to one chronicle but a *concilium generale totius cleri et populi* in another) constituted the kingdom's representative and appointed a commission "from the part of the whole kingdom" to go to Edward II at Kenilworth. These commissioners, as representatives of the Westminster assembly, were to speak "for the community of the land" and to ask the king whether he "would assent to his son's coronation and resign the rule." If not, they were to "render up their homage for all the land."³

At Kenilworth, the bishops of Winchester and Lincoln and the earl of Lancaster spoke secretly with the king and advised him to "resign the crown to his first-born son." After prevaricating and "adulterating the word of truth" somewhat, as *Baker's Chronicle* admits, these emissaries "threatened that unless he [Edward II] resigned, the people . . . would exalt as king some one other than of the royal family." In the end, Edward II "answered that he greatly grieved that the people of his realm should be so exasperated with him that they loathed his rule," but he was pleased "that his son was accepted by

Peters makes it evident that English canonists were familiar with this doctrine by 1327. The most recent study of the problems of Edward II's reign, with bibliographic review, is Charles T. Wood, "Personality, Politics, and Constitutional Progress: The Lessons of Edward II," *Studia Gratiana*, 15 (1972) [*Post Scripta*]: 519-36.

² Chrimes and Brown, eds., *Select Documents*, 33 (no. 18).

³ *Ibid.*, 35, 37, 36, 34; May McKisack, *The Fourteenth Century, 1307-1399* (Oxford, 1963), 89-91.

the people." The next day Sir Thomas de Blount, steward of the royal household, broke the virge as was done over a dead king's grave. Sir William Trussell, for the commissioners, returned "the homages and allegiances for the whole kingdom to Sir Edward of Carnarvon, recently king." Hereafter, his former subjects were to hold Edward, "father of the lord king who now is," to be but "a private person without any kind of royal dignity." Thereupon the commissioners went back to the "parliament at London" and "reported fully the king's answer—nay, rather more fully than it was made."⁴

In the meantime, the archbishop and two bishops had preached to the assembly in Westminster Hall. On Tuesday, January 13, the bishop of Hereford took for his text, "A foolish king shall ruin his people," and in this sermon "he dwelt weightily upon the folly and unwisdom of the king, and upon his childish doings." At the end, "all the people answered with one voice—'we will no longer have this man to reign over us.' " The next day, the bishop of Winchester preached on the words, "My head pains me." He then explained "with sorrow what a feeble head England had had for many years." On the third day, January 15, 1327, the archbishop of Canterbury used as the text of his sermon those words so dangerous to a kingly rule, "*vox populi, vox Dei*," to announce that "by the unanimous consent of all the earls and barons, and of the archbishops and bishops, and of the whole clergy and people, King Edward [II] was deposed from his pristine dignity, never more to reign nor to govern the people of England; and he added that all . . . , both laity and clergy, unanimously agreed that my lord Edward, his first-born son, should succeed." Edward III's accession was proclaimed in London on January 24, and his reign was dated from the 25th. A week later, February 1, he received unction and the crown. Thereafter, Edward II was moved from Kenilworth to Berkeley Castle to be kept safely "in perpetual prison" until September 1327 when the wretched man was "vilely murdered" in a most torturous way.⁵

Political success, rather than legal process, justified Edward II's deposition. Nonetheless, the sponsors of the 1327 dethronement deemed it expedient to cite authority to sanction one king's expulsion and another's succession. To the formula that Edward II "had ousted himself from the government," the victors added that he "was deposed . . . by the unanimous consent" of the lords and of "the whole clergy and people." Likewise, the queen's apologists claimed that Edward II had granted the crown to his son "by the common counsel and assent of the prelates," of the peers, and of "all the community of the kingdom." In this way they rooted the principles of counsel and consent still deeper in England's laws and customs. Further, the statement, "it is agreed that Sir Edward [III], the king's eldest son, should have the government of the realm and be crowned king," seems grounded in the principle of contract. Nevertheless, the deponents still assumed that Edward III, his father's "first-born" and "eldest son," succeeded by right of inheritance upon

⁴ Chrimes and Brown, eds., *Select Documents*, 34, 35, 38.

⁵ McKisack, *Fourteenth Century*, 89; Herbert Maxwell, trans., *The Chronicle of Lanercost* (Glasgow, 1913), 254-58; Chrimes and Brown, eds., *Select Documents*, 36.

his father's demise. In terms of the crown, that event occurred in January, when Edward II was transformed from a public person into a private one, a change that made his body politic dead in public law even though the body natural lived on into September. As a result, Edward III did indeed acquire a right to rule when he succeeded his father's body politic, but one should also recognize that insofar as "only God can make an heir," as Glanville had taught, it was really God through His authority who transmitted this right to the kingdom. And the new king became God's vicar and vicegerent, too, when the archbishop of Canterbury hallowed him with oil and crown, the rite that gave a monarch mystique and thaumaturgy.⁶

At Edward III's coronation, as at others in the fourteenth century, the people (*plebs*) signified their consent (*consencientibus*) by shouting "with a great and unanimous voice," *fiat, fiat*, and *vivat rex*. Therewith they sanctioned the king with the kingdom's mystic authority, one that was expressed in writings of the time through the use of such concepts as the community or communities of the kingdom, the estates of the realm, or simply the people. Just what men meant by the word, *people*, in 1327 or later, is not certain, but probably it contained some of the connotations of the Roman *populus*, which "referred to a group of rational beings associated by some form of consent expressed in laws, policy, or public ritual." The estates of the realm was a concept applied in 1327 when the prelates and magnates went to the London Guildhall and swore their oaths of allegiance to the new king. The Guildhall record listed the names of those taking the oath according to their rank, order, and degree; and, depending on how one counts, these form eight to twelve categories. In common parlance as well as literary usage, these were "the estates of the realm," and while their number in 1327 was uncertain, in 1420 the Treaty of Troyes with France would reduce it permanently to three.⁷ In 1399, the Lancastrian apologists would pick up the phrase and give it currency, for it was through use of that term that they invoked the kingdom's authority to justify their deposition of Richard II, an anointed king from whom not even "the rude sea" could "wash the balm."

A deep distrust of the man, Richard Plantagenet, impelled Henry of Bolingbroke and his adherents to depose their king, but the Lancastrians also held different views on England's polity. In 1386 Richard II had quarreled with the autumnal parliament when it impeached the retiring chancellor, the earl of Suffolk, and forced upon the king a commission with power to dismiss his

⁶ *Ibid.*, 38, 37; *Lanercost*, 255; G.D.G. Hall, ed. and trans., *The Treatise on the Laws and Customs of the Realm of England, Commonly Called Glanville* (London, 1965), 71; Francis Palgrave, ed., *Parliamentary Writs* (London, 1827-34), 1: 396 (no. 37); Barnaby C. Keeney, "The Medieval Idea of the State," *University of Toronto Law Journal*, 7 (1949): 65 and n. 50.

⁷ For discussion of the medieval meanings of the concept, *Populus* [People?], see Jeremy du Quesnay Adams, *The Populus of Augustine and Jerome* (New Haven, 1971), 2: "The consent involved was usually conscious and at least potentially subject to alteration; predetermining factors such as ethnic or linguistic affiliation were not closely or normally associated with it. . . . It was a word full of meaning and resonance, quite different from the diffuse and probably polysemic *people* which is less its heir than its descendant." Cf. 76, 109-19. In the fifteenth century, the three estates were the lords spiritual and temporal and the commons; centuries later they would become the king, lords, and commons.

servants. A few peers had threatened Richard with deposition, and the next year the king, not yet twenty-one, ordered the justices of the two benches to declare his rights. To them he put Ten Questions, and their answers provided this ruler with a rationale, if not a constitution, that authorized a concentration of power in the kingly office.⁸

Like their young king, the justices championed regality and declared that a 1386 statutory commission had infringed upon the king's "prerogative and the liberties of the crown." That statute, they said, was derogatory to Richard, "especially as it had been contrary to the king's wishes." Answers to the next questions stated the Ricardian view of parliament as the king's high court. Lords and commons were not to take up their matters first, nor first to require the king to assent to their decisions. Rather, "the king's articles ought to be proceeded with first" because "the king should have the rule of parliament." Moreover, the king might dissolve parliament whenever he pleased (as Richard's predecessors had done), and he could order the lords and commons to go away. "If anyone thereafter acts contrary to the king's will, as though he were in parliament," he should be punished as if a traitor. Nor should the lords and commons impeach (*impetere*) the king's officers and justices "contrary to the king's wish."⁹

The most dramatic of the Ten Questions, and politically the most inflammatory, was the ninth. How ought the man to be punished who had moved in the parliament of 1386 to "send for the statute by which King Edward [II] . . . had been adjudged in parliament?" The justices answered that this man and the one "who, under pretext of this motion, had brought that statute to parliament" should be punished "as traitors and criminals." Just which statute was meant is not evident, for in the strict sense, none such survives. The king's opponents in 1386 apparently mistook for a statute the entry on the *Rolls of Parliament* of Sir William Trussell's renunciation of allegiance to Edward II. Furthermore, at the parliament of 1386, Richard II's bellicose and ambitious uncle, Thomas, duke of Gloucester, had urged the king not to forget that the people, "by ancient statute and recent precedent," had a remedy for royal wrongs. All of this lends credence to the story that Gloucester and the lords appellant had, in December 1387, deposed Richard

⁸ S. B. Chrimes, "Richard II's Questions to the Judges, 1387," *Law Quarterly Review*, 72 (July 1956): 374. For studies of this deposition, see M. V. Clarke and V. H. Galbraith, "The Deposition of Richard II," *Bulletin of the John Rylands Library*, 14 (1930): 125-81; Gaillard Lapsley, "The Parliamentary Title of Henry IV," *English Historical Review*, 49 (1934): 423-49, 577-606, and "Richard II's 'Last Parliament,'" *English Historical Review*, 53 (1938): 53-78; H. G. Richardson, "Richard II's Last Parliament," *English Historical Review*, 52 (1937): 39-47; and "The Elections to the October Parliament of 1399," *Bulletin of the Institute of Historical Research*, 16 (1939): 137-43; Bertie Wilkinson, "The Deposition of Richard II and the Accession of Henry IV," *English Historical Review*, 54 (1939): 215-39, and *Constitutional History of Medieval England* (London, 1948-52), 2: 284-327; J. M. W. Bean, "Henry IV and the Percies," *History*, 44 (1959): 212-27; and, finally, Gerard Caspary, "The Deposition of Richard II and the Canon Law," *Proceedings of the Second International Congress of Medieval Canon Law*, Monumenta Iuris Canonici Subsidia, (Rome, 1966), 1: 189-201. For evidence of the distrust of Richard II, see [Jean Créton], *Histoire du Roy d'Angleterre, Richard [II]*, ed. and trans. John Webb, in *Archaeologia, or Miscellaneous Tracts Relating to Antiquity* (London, 1824), 20; esp. 137-41 where Créton, a supporter of Richard, reports that king's remarkably perfidious speech at Conway.

⁹ *Rotuli Parliamentorum*, ed. J. Strachey (London, 1767-77), 3: 224, 233. Hereafter cited as *RP*.

II and that he had remained “uncrowned” [*discoronatus*] for three days. Then after his opponents had removed Richard’s officers and councilors, they had “re-crowned” [*recoronant*] him king. Even Richard’s partisan, Jean Créton, described this parliament to that monarch as “your parliament, wherein you shall be highly crowned a sovereign king.”¹⁰ Whether the chroniclers were here retailing fact or fiction matters little, for their stories show that, whatever the case, some of Richard II’s subjects thought of Edward II’s deposition as a precedent and that circumstances might again rise that would remove the stigma of sacrilege from a king’s dethronement.

Nevertheless, Richard II’s deposition came only in 1399, after twelve years more of willful rule through his Cheshire archers, his Welsh pikemen, his over-eager bureaucrats, and sycophantic courtiers. He had given flesh and blood to his and his justices’ idea of what a king of England ought to be. The Lancastrians, whose *Record and Process of Richard II’s Renunciation and Deposition* denied that king’s rationale of his rule, no doubt exaggerated when they delineated the features of the bad king in their thirty-three articles of charges against Richard. They enriched their portrait with his personal defects and his professional faults. Not simply the conventional tyrant or despot of medieval political theology, he was a villain who embodied the vices and mistakes of all his predecessors on England’s throne. The offenses listed pertained to one or the other of the king’s two bodies, the body politic and the body natural. The “sentence” of deposition concluded that Richard personally, in his body natural, was “useless, unqualified, incompetent, and unworthy” for the “rule and governance” of Englishmen. Here the Lancastrians adopted, and much more explicitly than in 1327, the canonical doctrine of the *rex inutilis*—the useless, inadequate, inefficient, and inept ruler—a doctrine that distinguished between a king’s crimes and wrongdoings and his incompetence. The canonists held such a king “guilty of no sin except ignorance and inadequacy,” and back in 1245 Pope Innocent IV had pronounced King Sancho II of Portugal such a sovereign. Some canonists argued that a useless king should not be deposed, but that a coadjutor should do his work; when deposition seemed unavoidable, however, charges of dissoluteness were added. So the committee in 1399 charged Richard II with the crimes listed in the Decretals, to wit: “perjury, sacrilege, sodomy, plundering of subjects, reducing the people to servitude, insanity, and incapacity to rule”—“with all of which crimes,” Adam of Usk, a canon lawyer, chronicled, “King Richard was known to be tainted.”¹¹

Perjury was the count that the Lancastrian apologia brought most fre-

¹⁰ *RP*, 3: 233; McKisack, *Fourteenth Century*, 444; Chrimes and Brown, eds., *Select Documents*, 145; Créton, *Histoire*, 35. On one manuscript roll the renunciation is written between Edward III’s first and second statutes: H. G. Richardson and G. O. Sayles, *Rotuli Parliamentorum Angliae Hactenus Inediti* (London, 1935), 100.

¹¹ *RP*, 3: 422 (nos. 51, 52); Edward Peters, “*Rex Inutilis*: Sancho II of Portugal and Thirteenth-Century Deposition Theory,” *Studia Gratiana*, ed. Stephan Kuttner, IV, vol. 14 (1967): 300; Edward Maunde Thompson, ed. and trans., *Chronicon Adae de Usk, A.D. 1377–1421*, (2nd ed.; London, 1904), 29–30, 181; cf. *Corpus Juris Canonici, Liber Sextus Decretalium Domini Bonifacii Papae VIII*, Lib. II, Titulus XIV, sec. ii: *Papa Imperatorem deponere potest, ex causis legitimis*.

quently against the king, and ten of the thirty-three articles contain the word itself. John de Burgh's tract for priests defined perjury as confirming a lie under oath. Allegedly, Richard was guilty of this sin by breaking his promises to Henry, duke of Lancaster, about his exile and inheritance. The king had also violated his coronation oath when he broke laws, like the one limiting the sheriff's term of office or the antipapal statutes that he had promised to keep. Richard II had denied the liberties of the Church and taken money from the clergy, again contrary to his coronation oath. He had exiled Archbishop Arundel "without any reasonable or lawful cause, or any process of law, against the laws of the realm." After Richard had sworn on the cross of St. Thomas of Canterbury that Arundel would not lose his archbishopric, he had asked the pope to translate him. There were other frauds and deceits, like his promise to remit damages to the duke of Gloucester, whom he had murdered instead. The king had confiscated the lands of lords Warwick and Cobham, after their executions, "against justice, the laws of the realm, and his express oath." His opponents construed certain clauses in Richard's last testament as irrational and "repugnant to all right and reason." Furthermore, the king had made other erratic pronouncements: "that the life of any one of his lieges, and his lands, tenements, goods, and chattels are at his will without any [process of] forfeiture."¹² Richard had terrorized some men, like the justices who had answered the Ten Questions, by threats and fear of death. His troops raged through the countryside and committed robbery, rape, and murder, but the king refused to answer complaints or to do justice. He had deceived the people of seventeen counties; he had extorted money and had failed to pay his debts. Finally, he had ordered the *Rolls of Parliament* changed and records thereon deleted.

Beyond the seas, Richard II personally had made an impression that had damaged England's reputation "among strangers throughout the whole world." He had been "unstable and dissimulating," especially in writing to popes, kings, and lords; and "no living person knowing of his condition could or would trust him." Moreover, there were many who considered him "unfaithful and inconstant," and all this was "to the scandal of not only his person, but also of the whole kingdom."¹³ In short, the Lancastrian catalog of Richard II's personal defects depicted a mighty bad king.

More significant constitutionally were the things that Richard's body politic had done. It had acted "against the crown and royal dignity and against the statutes and liberties of the realm, . . . the laws of the land," and the laws of chivalry. Specifically, Richard had broken his coronation oath (recorded verbatim in the *Record and Process*), and he had violated c. 39 of Magna Carta. Some subjects who had spoken words in derogation of the king's person had been "taken and imprisoned and brought before the constable and marshal of England in military court." More offensive yet was Richard's request of the

¹² *RP*, 3: 418 (no. 21), 421 (no. 48), 420 (no. 43).

¹³ *Ibid.*, 420 (no. 42).

pope to confirm statutes enacted in the parliament of 1398 "although the crown of the kingdom of England and the rights of the same crown and the realm itself, have been from all past time free thereof." Furthermore, Richard II had "expressly stated, with an austere and shameless face, that his laws were in his mouth and at times in his breast, and that he alone could change and enact laws of his kingdom."¹⁴

Here the Lancastrians had cut to the root of the constitutional conflict between Richard II and his baronial opponents: Was the king of England to be the spokesman of the law, the law speaking, *lex loquens*? Or was the law of England to be what the king willed and said, it was to be, *rex loquens*? In this way, then, two competing authorities, the king's and parliament's, and two contradictory doctrines of the source of English law elevated what was in origin a family feud and an interbaronial struggle for political power onto the plane of principle.

The committee of "doctors, bishops, and others," such as Adam of Usk, had begun to draft the Lancastrian condemnation of Richard II only on September 21, after the king was safely in the Tower of London. Nine days later, September 30, those present in Westminster Hall used the authority of the estates of the realm and of the people to sanction Richard's deposition. The day before, a commission of sixteen interviewed the king. Included were lords spiritual and temporal, the two chief justices, two doctors of law, two notaries, and others learned "in civil and canon law as well as the laws of the realm." In a cheery tone, they reported back to the assembly the next day things both fanciful and fallacious—and a few authentic facts. The king had said "publicly with a happy countenance, that he was prepared to renounce and to cede" the throne to Henry "according to his promise"; and he had absolved all his liegemen from their oaths of fidelity and homage to him and "from every bond of allegiance and regality and lordship." Richard had asked the archbishop of York and the bishop of Hereford to announce this "cession and renunciation to all the estates of the realm and . . . his will and intention to the people." As a sign of his intent, Richard had taken off his gold signet ring and placed it on Duke Henry's finger and asked him to show it to all the estates. In the event, though, this attempt to designate the succession purely on the basis of the king's royal authority and "promise" was to prove insufficient.¹⁵

After the archbishop of York had described this scene in the Tower, he read

¹⁴ *Ibid.*, 419 (nos. 27, 29, 28, 33), 420 (nos. 43, 44), 421 (no. 47). For understanding Richard II's misrule, see Caroline M. Barron, "The Tyranny of Richard II," *Bulletin of the Institute of Historical Research*, 41 (1968): 1-18 and, good for specific details, though now dated, Reginald Rankin, *The Marquis of d'Argenson and Richard II* (London, 1901), 137-300. Much more sympathetic to Richard is Anthony Steel, *Richard II* (Cambridge, 1941).

¹⁵ *Chronicon Adae de Usk*, 29; *RP*, 3: 416 (nos. 10, 12, 13), 417 (no. 14). Although the insufficiency of Richard II's authority to designate his successor (so different from that of William the Conqueror) appears largely the product of practical caution on the part of the Lancastrians, by 1460 the inability of a king to designate the succession was more clearly the consequence of constitutional as well as political considerations: see below, at notes 20-5. Yet questions relating to the succession were already beginning to exceed a purely royal authority; even Richard II found it desirable to seek parliamentary recognition of his own designated heir, Roger Mortimer: S. B. Chrimes, *Constitutional Ideas in the Fifteenth Century* (Cambridge, 1936), 24, n. 1.

the cession and renunciation, first in Latin, then in English. Next, the archbishop of Canterbury asked "the estates and the people" whether they wished to accept the king's resignation "for their benefit and for the profit of the realm." Thereupon, "the same estates and the people thinking . . . this would be very expedient . . . unanimously and harmoniously accepted the renunciation and cession." This done, "all the estates" were asked "singly and in common" whether Richard's "crimes and defaults were sufficient and notorious enough to depose the same king." Once again, "all the estates consented unanimously." Thereupon, "the estates and communities" appointed deputies "to depose King Richard of every royal dignity, majesty, and honor, in the place, name, and authority of all the aforesaid estates, as has been observed in similar cases by ancient custom of the said kingdom."¹⁶

When the commissioners returned to the Tower on Wednesday, October 1, to depose Richard II formally, they again invoked the authority of the estates and of the people. To justify this dethronement, Chief Justice Thirning and his fellow procurators explained that "all the estates of this land" had "given them full authority and power and charged them for to say the words" that he would address to "Sir Richard, formerly king of England." He told how "all the estates and all the people," gathered at Westminster on Tuesday, had thought that the "articles of defaults in your governance" were "reasonable and cause for to depose you." The commissioners, "representing the peers and nobles, spiritual and temporal, of the realm of England and the communities of the realm and all the estates of the realm," had delivered "in open court" their "definitive sentence." Then the chief justice concluded: "We, procurators to all these estates and people aforesaid, . . . by their authority given us, and in their name, yield you up for all the estates and people aforesaid, homage liege and fealty and all the allegiance and other bonds, charges, and service that belong thereto." Thus in law the authority of the people and of the estates of the realm—that is, the *kingdom's* authority, not parliament's—triumphed over Richard II, his will and his willfulness, and over God's anointed.¹⁷

This apologia of the Lancastrians states or implies that the kingdom, the estates of the realm, and the people speaking through their self-designated proxies had a right to depose. What underlies all the charges against Richard II (and the earlier ones against Edward II as well) is a tacit assumption that the kingdom has authority, perhaps supreme, one sanctioned by those four potent little words, *vox populi, vox Dei*.¹⁸ In theory, then, the people's voice expressed the people's choice—and God's.

¹⁶ *RP*, 3: 417 (nos. 15, 16), 422 (no. 51).

¹⁷ *Ibid.*, 424 (nos. 58, 59, 60), 422 (no. 50). The phrase, "by authority of parliament," first appeared only in 1398 when the Shrewsbury parliament authorized the appointment of a committee to deal with petitions. Then in Henry IV's parliament of 1399, statutes were said to bind until they were revoked "by authority of another parliament." Chrimes, *Constitutional Ideas*, 138, n. 4, quoting from *RP*, 3: 419.

¹⁸ The right to depose in 1399 seems to have derived from the authority of the estates of the realm and/or the people, perhaps the kingdom as an abstract concept, but not from parliament's authority. By 1484, however, the authority of the estates was enhanced by their being "in" parliament, whose authority seems by then to have acquired a higher degree. The purpose of using the concept of estates in 1399 seems to have

But the reasons given to justify a king's deposition were not the same as those required to authorize his successor's right to rule. In 1399, however, the sources of authority were much the same, and Henry IV succeeded Richard II as God's choice, as the people's, and a bit less mystically as that of the estates of the realm. The representatives who spoke for these estates were for practical reasons the high officers of state—councilors, justices, and men of law—reinforced by those lords spiritual and temporal and the knights and burgesses who happened to be at Westminster because of Richard II's writs of summons to the king's parliament.

Before this assembly, the duke of Lancaster arose and spoke, in English. Henry claimed as his right "this realm of England and the crown with all the members and appurtenances . . . [i]n the name of the Father, Son, and Holy Ghost." He did so as "descended by right line of the blood" from Henry III and "through that right that God of His Grace hath sent me with the help of my kin and of my friends to recover it." When Henry had finished, "the lords spiritual and temporal and all the estates there present, singly and in common, were asked how they felt about this claim and vindication." Thereupon, the "same estates with all the people without any difficulty or delay unanimously consented . . . that the aforesaid duke should reign over them." When Henry had thanked "God and you, spiritual and temporal, and all the estates of the land," a parliament (a thing hitherto not mentioned) was announced for the next Monday, October 6, and the coronation a week later on the Feast of St. Edward the Confessor. Henry IV himself acknowledged the authority of the estates when he described his title to the Doge of Venice "as by right of birth and by the unanimous consent of the lords and commons, we possess the royal scepter." The Percies were to tell Henry IV in 1403 that "you crowned yourself king," but others, less doubtful, considered him a king "chosen [*electus*] by God," who had sanctioned his right to rule.¹⁹

The next critical date in the transit of ideas that shaped the right to rule was October 25, 1460. On that day King Henry VI contracted, and the lords of parliament agreed, that Richard, duke of York, should be his lawful heir and legitimate successor to the crown. Henry's failure to run an efficient government, to win the Hundred Years War, and to maintain law and order within the realm had quickened men's curiosity about his authority to govern. After the lords of parliament had adjudged York's claim to the crown superior to Henry VI's, the king "condescended to [an] accord to be made between him

been to demonstrate that those assembled in Westminster Hall were present as representatives of the kingdom—which parliament, a century later, was to become. In 1399 parliament was more the king's high court than the legislative or legislature—words that first appeared in 1689 and 1676 (see *O.E.D., sub verba*). In 1399 "the estates and the people" seem to have been a more comprehensive body, representative of the whole realm, as against the king's high court and his great council whose original functions were to give the king counsel and consent. By 1484, though, parliament had become such a body, so that it could, and did, comprehend the three estates, if not the people. See Charles T. Wood, "The Deposition of Edward V," *Traditio*, 31 (1975): 277–83.

¹⁹ *RP*, 3: 422–423 (nos. 53, 54, 56, 57); *Calendar of State Papers, Venetian*, 1: 39 (no 131); Bertie Wilkinson, *Constitutional History of England in the Fifteenth Century (1399–1485)* (New York, 1964), 45, quoting from Hardyng's *Chronicle*, ed. H. Ellis (London, 1812), 352–53; H. T. Riley, ed., *Johannis de Trokelowe, et Henrici de Blaneforde, Monachorum S. Albani, necnon Quorundam anonymorum Chronica et Annales* (London, 1866), 300.

and the said duke and to be authorized by the authority of this present parliament."²⁰ Here, it is submitted, was the crucial point of recognition that guaranteed for parliament and its authority a future, and for the monarchy a limitation that would put the kingship, and the king's servants too, under law.

Constitutionalism runs through the whole rationale that Richard, duke of York, used in his bargaining for the crown. The notion that the Yorkists pursued a doctrine of legitimacy seems much too modern a concept, in fact post-Metternichian, for the undefined and ambiguous rules of inheritance to the English kingship in the fifteenth century. After all, three separate Lancastrians had enjoyed the crown for sixty years, and Henry VI, unlike Richard II, had a direct blood heir. To counter this prescriptive right, the Yorkist lawyers in 1460 argued not legitimacy, but only that the descendants of Edward III's third son, Lionel, duke of Clarence, should precede those of the fourth, John of Gaunt; and, for good measure, that York also descended from the fifth son, Edmund, duke of York. Nevertheless, the elder line came down through two women, Philippa and Anne, and the legality of inheritance through one female, let alone two, had yet to be confirmed. Except for this possible bar feminine, though, the sanction of hereditary right, with its divine overtones, put Richard, duke of York, and his sons in a strong position, provided they could find an authority high enough to recognize the legal validity of their claim. In the event, Henry VI, of "his assent and agreement, of his free will and liberty," recognized the lords' conclusion, "authorized by the authority of this present parliament," and agreed that Richard of York was his heir apparent, thereby disheriting his seven-year-old son, Prince Edward of Lancaster.²¹

This accord or contract first recited York's genealogy. Then it explained that the duke, "for the weal, rest, and prosperity of the land," had "agreed and consented" that Henry VI, who had possessed England and the kingship so long, should be "reputed and taken" to be king "during his life natural." York and his two older sons, Edward, earl of March, and Edmund, earl of Rutland, were to swear oaths not to do anything "to the abridgement of the natural life of King Harry VI." In return, York was to be "called and reputed from henceforth, very and rightful heir to the crowns, royal estate, dignity, and lordship abovesaid." By this agreement, the duke was to receive, "by authority of this present parliament," lands, castles, and manors to the yearly value of 10,000 marks. The accord also declared York's person inviolable, and

²⁰ *RP*, 5: 378 (no. 18). For background, see J. R. Lander, "Henry VI and the Duke of York's Second Protectorate 1455 to 1456," *Bulletin of the John Rylands Library*, 43 (1961): 46-69; also Charles Ross, *Edward IV* (London and Berkeley, 1974), 12-29.

²¹ On the weak hereditary principle in England and its relationship to depositions, see Charles T. Wood, "Queens, Queans, and Kingship: An Inquiry into Theories of Royal Legitimacy in Late Medieval England and France," *Order and Innovation in the Middle Ages*, ed. W. C. Jordan, C. B. McNab, and T. F. Ruiz (Princeton, 1976), 385-400, 562-6. *RP*, 5: 378 (no. 18), 379 (no. 27). B. P. Wolffe has made out a strong case that Henry VI was more king than saint, but not successful as either. He maintains that Henry VI conducted an aggressive government under a "personal rule" that engendered "dissensions and mutual recriminations, accusations of deceit and bad faith"—all characteristics of the Bad King. B. P. Wolffe, "The Personal Rule of Henry VI," *Fifteenth-Century England, 1399-1509*, ed. S. B. Chrimes, C. D. Ross, and R. A. Griffiths (New York, 1972), 42.

anyone who plotted or compassed his death was to be judged guilty of treason. Finally, the lords of parliament were to take oaths to accept, worship, and repute Richard and his heirs "as above is rehearsed"; and the duke and the two sons were, in turn, to swear oaths "to help, aid, and defend the said lords." All that remained to complete these arrangements was for God to intervene and terminate Henry VI's "life natural."²² Then the crown would pass to a new king, one for the first time genuinely and legally made-in-parliament—and under the law of contract.

Before the lords of parliament and the men of law reached this conclusion, they had searched their souls and the records. The duke of York had submitted his claim to the crown on October 16, 1460, in parliament. Since it was one on which Henry VI alone clearly lacked the practical and legal authority to rule, king though he might be, the lords called "the king's justices into the parliament chamber" on the 18th "to have their advice and counsel." In the king's name they ordered them "to search and find all such objections" against York's claim and to fortify "the king's right." These judges, lacking the stuff supreme courts are made of, ducked the great constitutional issue with its risk of treason: to define, and hence to limit, the English kingship. They refused to determine the rightful inheritor of the crown, to perform a judicial review of regal rights, or, in short, to make public law. For the king's justices to have done so would have been to have placed themselves and their successors above their king, thus putting the crown under the judiciary. Instead, the judges reported back to the lords on Monday, October 20, that they would give no opinion because "they were the king's justices, and have to determine such matters as come before them in the law, between party and party, . . . and since this matter was between the king and the said duke of York as two parties," they would not give Henry VI counsel. Moreover, and here the constitutional issue came in, "the matter was so high and touched the king's high estate and regalie, which is above the law and passed their learning, wherefore they durst not enter into any communication thereof, for it pertained to the lords of the king's blood, and th'apparage of this his land, to have communication and meddle in such matters."²³ Not yet were the justices proper lions under the king's throne, or if they were, they did not care or dare to roar in Henry VI's behalf.

Instead, the lords of parliament, irked, sent for the serjeants-at-law and the king's attorney and ordered them in the king's name to "search and seek" for all things that would avail the king and defeat the duke's title. Two days later, Wednesday, October 22, these men of law stood on Monday's precedent and declined to communicate: since "the said matter was so high that it passed the learning of the justices, it must needs exceed their learning." Next, the chancellor, George Neville, tried to compel the serjeants to give advice or counsel because "they had their fees and wages" from the king. To this the serjeants made a remarkable statement about the king's authority: "They

²² *RP*, 5: 378–9 (nos. 20, 21, 22, 23, 25, 26).

²³ *Ibid.*, 375 (no. 10), 376 (no. 12).

were the king's counselors in the law, in such things as were under his authority, . . . but this matter was above his authority, wherein they might not meddle."²⁴

The king's own authority, that of the king's justices, and the authority of the law and its serjeants, each had failed to enable the lords to decide who had the right to rule and be king. They were forced to do it themselves. They had a free debate and agreed "that every lord should have his freedom to say what he would say without any reporting or maugre [ill-will] to be had for his saying." Thereupon they negotiated the compromise between Henry VI and Richard, Duke of York. The agreement, comparable to the treaty between King Stephen and Henry II, was an "act of accord, by the king and the three estates, in this present parliament assembled."²⁵ In politics, the advice and assent of the lords of parliament and of the commons validated the agreement; yet in law it carried the sanction of an authority only vaguely hinted at in 1399, but now made explicit: that of the three estates of the realm. The compromise was illogical and contradicted theory and theology. And yet it was so sensible. As the product of expediency, it offered a political solution to a legal problem. After all, York had arrived with 300 armed men, and the sanction of force and arms was never far in the background. The lords of parliament were too evenly divided in their partisanship to stop for finespun theorizing; yet what they said "as of record" on that October day about king and parliament was to be long remembered.

The very use of parliament and of the phrase, "by authority of parliament," to resolve this crisis gave precedent and principle for a parliamentary monarchy. Henry VI's parliament of 1460 had repealed the statute of 1406 that had declared the sons of Henry IV and their heirs successors to the throne. The lords of parliament in 1460 had declared such acts to be of "more authority than any chronicle, and also of authority to defeat any manner [of] title made to any person." And yet this same parliament, by repealing the statute of 1406, switched the order of succession to the Yorkist line, and so, by implication, indicated that a parliament might not be bound by its predecessors' acts. The duke of York, himself, agreed that after Henry VI's death his title would be one "entailed by authority of parliament," and in so doing he set a precedent for Henry VIII's three Succession Acts that determined the sequence of his assorted heirs.²⁶

Had Henry VI but died immediately, this agreement would have become

²⁴ *Ibid.*, 376 (no. 12).

²⁵ *Ibid.*, 376 (no. 12), 379-80 (nos. 29, 27).

²⁶ *Statutes of the Realm*, ed. A. Luders *et al.* (London, 1810-28), 2: 380; *RP*, 5: 377 (no. 15), 379 (no. 28); St. Thomas More, *The History of King Richard III*, ed. R. S. Sylvester (New Haven and London, 1963), 67. It should be noted, too, that over a year previous to the October parliament of 1460 the earl of Warwick had refused to yield up the captaincy of Calais in response to Henry VI's personal authority under the privy seal. The earl argued that "forasmuch as he was made by authority of parliament, he would not obey the privy seal." Wilkinson, *Fifteenth Century*, 287, quoting from Davies' *English Chronicle*. It would appear, then, that 1460 was not the first year in which the authority of parliament was employed to justify a position hostile to the wishes of the then-reigning king, thus implying that parliament's authority might be superior to that of the king.

operative. Unfortunately, God's will was otherwise, and events destroyed the logic of the settlement. The wager of battle at Wakefield, December 30, 1460, went against the duke of York; he was killed, and Henry VI survived. Nevertheless, he too was soon defeated, but not killed, at Towton in Yorkshire at the end of March 1461. The contractual agreement still held, for it applied equally to York's heirs. Had Henry VI been slain in battle, then Edward IV would have succeeded under the terms of the contract of 1460, a written one signed and sworn to and sanctioned "by authority of parliament." This would have made Edward IV a purely parliamentary sovereign. But unhappily for England's public law and contractual monarchy, God let Henry VI live on and flee with Queen Margaret to Scotland, thence to France where they maintained a government-in-exile.

Real life being different from theory, Edward IV thus won his crown by military victory, not in parliament. The Lord God of Hosts had shown him His favor and had expressed His choice of king. So with the victor in possession of the throne, God's servants confirmed men's maneuverings through episcopal connivance and ecclesiastical ritual on June 28 at Edward's coronation. Though the new king had proclaimed his accession on March 4, 1461, he did not call a parliament to meet until November 4. On that parliament's *Rolls* "of record" are explanations of Edward IV's title, of his right to rule, and of his authority to govern. The sanctions that the Yorkists invoked derived from God, from the law in its various emanations, from the realm, and from parliament. The kingdom's authority, expressed through the three estates of the realm, was in the process, not yet complete, of being transformed into "the authority of parliament." Edward, with "the advice and assent of the lords spiritual and temporal and of the commons," declared himself king "by God's law" of inheritance, with parliament inscribing his royal pedigree on its *Rolls*.²⁷ The commons' petition thanked God for His decision and Edward for his willingness to assume the burdens of the English crown.

Since Henry VI lived on, King Edward IV could not make his case on the basis of the agreement of 1460 between King Henry and his father. So Edward cleared the way for his inheritance by an act of parliament that declared the three Henrys to have been kings *de facto* but not *de jure* because of Henry IV's "intrusion and usurpation." The Yorkists did not denigrate Henry VI personally as Henry IV had done to Richard II. Instead, the commons condemned the "extortion, murder, rape, effusion of innocent blood, riot, and unrighteousness" that had flourished without punishment during Henry VI's "usurped reign." Their petition contrasted Henry VI's "abusion of the laws" with the benefits of a good king's "plenty, peace, justice, good governance, policy, and virtuous conversation." To achieve these ends, the good king should rule by an authority external to his person. Moreover, such a king was expected to be responsible to something more than to God and to his own conscience—perhaps to the people, certainly to the law. Edward IV's right to rule was said

²⁷ *RP*, 5: 464 (no. 10).

to be "by God's law, man's law, and the law of nature." Above all, the good king and his ministers would govern through procedures duly recognized as lawful—what Henry IV's council had described in 1408 as "due process of the law." Finally, this petition in 1461 endorsed the agreement of 1460 with the duke of York and declared Edward "in right, from the death of . . . his father, very just king of the said realm of England."²⁸ So much for God and the law.

If those who drafted Edward IV's apologia overlooked the people's authority, contemporary chroniclers did not. Several tell how the people enjoyed a vociferous role in Edward's usurpation when they bestowed, in March 1461, their authority on his succession. On three occasions, when asked if they wanted Edward, the people shouted "Yea"; but the chroniclers do not mention any election by the estates of the realm. Edward IV and his sponsors held meetings in London, one on March 1 in St. John's field. There "the people" and an army—some "3,000 or 4,000 men"—had gathered, and "certain articles and points were read to them, in which King Henry VI had offended against the realm." First, the people were asked whether the said Henry was still fit to reign, and they cried "Nay." Then they were asked if they would have Edward, Earl of March, to be their king, and they cried "Yea." Thereupon certain captains and many people went to Baynard's Castle, and there they told the Earl of March "that the people had chosen him to be king; and he thanked them . . . and granted that he would take it upon him." Three days later, on March 4, the people were invited to meet Edward at St. Paul's at nine o'clock, and there the bishop of Exeter gave a sermon and "set forth the right and title of the Earl of March to the crown; and he demanded of the people if they would have the earl to be their king, as due to his right, and they cried 'Yea.' " At St. Paul's, Edward was "elected and admitted for king."

All the people then went with him to Westminster Hall, and there Edward "was sworn before the archbishop of Canterbury, the chancellor of England, and the lords, that he would truly and justly keep the realm and maintain its laws, as a true and just king. They put on him the king's robes and the cap of estate; and he went and sat on the throne as king. It was demanded of the people if they would have him to be their king, and would maintain, support, and obey him as true king; and the people cried 'Yea.' "²⁹

²⁸ *Ibid.*, 464 (no. 10), 463 (no. 7); Harris Nicolas, ed., *Proceedings and Ordinances of the Privy Council in England* (London, 1834–37), 1: 309. To do all this was necessary because Edward IV, like his father, had sworn to keep the terms of the contract of 1460, which allowed Henry VI to reign for his natural life. In order to circumvent the "convention, concord, and agreement between Henry and the duke," Edward IV's parliament had "declared and judged, by the said advice, assent, and authority," that Henry VI had, "against good faith, truth, conscience, and his honor brake the said convention and concord, and departed therefrom of wilful malice." Therefore, the petition concluded, "the breach thereof, on his part, discharged" Edward from "the keeping thereof of any partice or point after the said breach." *RP*, 5: 466 (no. 14). The extent to which all kings were responsible to authority external to their persons, especially the law, was also emphasized by Edward's Lancastrian opponent, Sir John Fortescue: cf. David S. Berkowitz, "Reason of State in England and the Petition of Right, 1603–1629," *Staatsräson: Studien zur Geschichte eines politischen Begriffs*, ed. Roman Schnur (Berlin, 1975), 168–72. Strikingly, though, Berkowitz demonstrates (p. 170) that Fortescue allows for and justifies "royal power" as "derived not from the prince's authority but solely from the law of nature."

²⁹ Wilkinson, *Fifteenth Century*, 173–4.

The notion that the people chose the king was current in 1461. The duke of Milan's emissary to France reported to Francesco Sforza "how the people of London, the leaders of the people of the island, together with some other lords, full of indignation had created a new king. . . . [H]e was chosen, so they say on all sides, as the new king by the princes and people at London." Nevertheless, the crucial decision was made at a council of lords and bishops in the smoke-filled rooms of Baynard's Castle "where they agreed and concluded that Edward himself, the duke of York, should then be king of England." After this, Edward IV received the vulgar sanction of the people's will (or willingness), and presumably their authority. Thus God and the people, the lords spiritual and temporal, and Edward IV's armed retainers too, participated in creating a new king of England according to the laws of God, man, and nature. Eight months later, in November 1461, the action was confirmed "by the authority of parliament."³⁰

When Edward IV's brother, Richard, duke of Gloucester, intercepted the crown as it was descending upon his twelve-year-old nephew, Edward V, in June 1483, he followed a scenario drawn from the earlier usurpations.³¹ Edward IV's last wishes had made Richard Protector of the realm with the authority to govern. But becoming afraid of losing his power to Elizabeth, the queen-mother, and her Woodville faction, Gloucester aimed to get possession of the young king and his brother. Edward V's coronation had first been scheduled for May 4 by the Woodvilles, but the Protector then postponed it until June 22. He gained control over the king's person on April 30, but did not get his brother into his hands until June 16. Perhaps on the 17th, certainly by June 22 when the "process of deposition" had begun, Richard decided to usurp the throne in order to preserve the power of governance that the Protectorship allowed.³² So the boys went to the Tower; they were never seen again outside its walls. By December 1 rumor of their deaths had reached Vienne on the Rhone.

Richard III's motivation came more from fear (see his portraits) than from villainy. As long as another person, even a boy, was the true and living sovereign, one whose will a rival counselor might declare, Richard lacked the right to give forevermore "the last and highest commandment." So to clear obstructionists from his way to the throne, in mid-June Richard had the bishops, Rotherham and Morton, dismissed from the council and imprisoned in Wales; at the same time, his recent aide and ally Lord Hastings, Edward IV's chamberlain and sworn man, was murdered; and on June 25, the queen-mother's brother and son, Earl Rivers and Lord Richard Grey, were executed

³⁰ *Calendar of State Papers and Manuscripts Existing in the Archives and Collections of Milan*, ed. Allen B. Hinds (London, 1912), 58 (no. 76); Wilkinson, *Fifteenth Century*, 176.

³¹ See C. A. J. Armstrong, "The Inauguration Ceremonies of the Yorkist Kings, and Their Title to the Throne," *Transactions of the Royal Historical Society*, 4th series, 30 (1948): 51-73 for a comparison of Richard III to Edward IV; Wood, "Edward V," 277-82 for one to Henry IV. See also J. R. Lander, "Marriage and Politics in the Fifteenth Century: The Nevilles and the Wydevilles," *Bulletin of the Institute of Historical Research*, 36 (1963): 119-152.

³² E. F. Jacob, *The Fifteenth Century, 1399-1485* (Oxford, 1961), 614. For the circumstances that provoked Richard, duke of Gloucester, to usurp the crown, see Wood, "Edward V," 248-68.

at Pontefract as "conspirators of Richard's death."³³ Back in London, Richard's agents put on a travesty concocted from accounts of the three previous depositions. Dr. Shaw, the mayor's brother, acted the role played by bishops in 1327 and 1461. He preached on Sunday, June 22, not in St. Paul's Cathedral, but outside at Paul's Cross. His text came from Wisdom iv: 3: "Bastard slips shall not take deep root." Thereupon, he proceeded to bastardize Edward IV and his sons and daughters; and he declared Richard, "who altogether resembled his father," to be "the legitimate successor." Unlike the meeting held in 1461 at St. Paul's where the people gave their "yeas" for Edward IV, at Dr. Shaw's meeting only "a small number of listeners cried 'yea!' . . . more for fear than for love." Thomas More later explained this failure as the result of bad timing, for Richard was scheduled to "have come in among the people to the sermonward, to the end that those words meeting with his presence, might have been taken among the hearers, as though the Holy Ghost had put them in the preacher's mouth, and should have moved the people even there, to cry King Richard, King Richard, that it might have been after said, that he was specifically chosen by God and in manner by miracle." Unfortunately, Dr. Shaw spoke too fast—or Richard tarried too long and missed his cue—for the doctor had "entered into other matters ere the Protector came"; and then, flustered by the Protector's arrival, he repeated Richard's claim "out of all order, and out of all frame."³⁴

This histrionic fiasco did not, however, deter the conspirators. On Tuesday, June 24, the duke of Buckingham, Richard's ally, stage manager, and rhetorician (only to become his rebellious subject and rival in October), orated at the Guildhall. Those present " marvelled and said that never before that day had they heard any man, learned or unlearned, make such an exposition or oration as that." The next day, the 25th, Buckingham led a few noblemen, knights, and gentlemen, along with the mayor and aldermen of London, to Baynard's Castle, the home of Richard's mother. There Richard, like Edward IV before him, awaited the people's invitation to be king. Buckingham, on behalf of his companions, "made humble petition" that Richard take "upon him the crown and governance of this realm." On Thursday, June 26, Richard rode from Baynard's Castle to Westminster Hall where he received a bill of petition and "was set in the king's throne or place where all kings first take possession." Seated on the king's bench in the royal chair, Richard acted out the scene from Edward IV's script:

[H]e called before him the judges, commanding them straightly, justly, and duly to administer his law without delay or favor. . . . [H]e then proceeded into the Abbey. At the church door, he was met by a procession, and the abbot or his deputy delivered to him the scepter of St. Edward. He then proceeded into the shrine and there made

³³ Dominicus Mancinus, *The Usurpation of Richard the Third* (*Dominicus Mancinus ad Angelum Cantonem de Occupatione Regni Anglie per Riccardum Tercium Libellus*), ed. and trans. C. A. J. Armstrong (Oxford, 1936), 149 and n. 72, 152 and n. 82, 117 and n. 90.

³⁴ Wilkinson, *Fifteenth Century*, 189–90; James H. Ramsay, *Lancaster and York* (Oxford, 1892), 2: 488; More, *History*, 68.

offerings. He was next led into the choir and sat there whilst a *Te Deum* was beautifully sung by the monks. When these ceremonies were finished, he returned into the king's palace and was there lodged.³⁵

On July 6, a reluctant archbishop of Canterbury, Thomas Bourchier, anointed Richard III and crowned him king of England. Even without the moral opprobrium of the princes' murder in the Tower (Edward V may have died a natural death from osteomyelitis), Richard III went to his coronation with lies on his lips and blood on his hands.

The ways of Providence in 1483 may have mystified some men present at Richard's coronation, but more coolheaded prelates and legists took this opportunity to clarify the coronation oath. First of all, they wrote it in English, and the copy now at Lambeth Palace makes it quite certain which laws King Richard III was to "grant and promise" to keep: "Do you grant the rightful laws and customs to be held and promise you after your strength and power such laws as to the worship of God shall be chosen by your people . . . ?"³⁶ A king who made this promise, under oath, was certainly under the law—and virtually alongside his people.

Unfortunately, though, many Englishmen (including Richard, duke of Gloucester) had sworn oaths of fealty and had done homage to Edward V. The Protector himself had received the oaths of the mayor and notables of London. Richard III's lawyers were able to fix that. On June 28, they drew up "articles of instruction" to invalidate these premature oaths. If the mistake was easily pointed out and as easily rectified, credibility was harder to establish. Many "great estates and personages, being ignorant" of Richard III's title, had taken the oath of allegiance to Edward V, but "now every good true Englishman is bound upon knowledge had of the said very true title to depart from the first oath so ignorantly given . . . , and thereupon to make his oath of new and owe his service and fidelity to him [Richard] that good law, reason, and the concord assent of the lords and commons of the realm have ordained to reign upon the people. . . ." The coronation ritual, too, recognized Richard III's election by the three estates. When the archbishop of Canterbury presented "the king to the people" in Westminster Abbey, he designated him as "inheritor by the laws of God and man to the crown and royal dignity of England . . . elect, chosen, and required by all three estates of the same land to take upon him the said crown and royal dignity." Note, though, that in 1483 the word "elect" meant to choose, not to ballot, as it still did in 1526 when thirty of Henry VIII's great horses were to "be elected and chosen out of the best" for the royal stables.³⁷

³⁵ Wilkinson, *Fifteenth Century*, 190; More, *History*, 77–8.

³⁶ Chrimes and Brown, 355. In view of Wood's argument ("Edward V," 283–5) that Richard III's act of succession had the effect of extending the authority of parliament to spiritual affairs, it is significant that his coronation oath made him "grant and promise . . . such laws as to the worship of God shall be chosen by your people." Given the changes in authority then taking place, this development should scarcely be surprising.

³⁷ Chrimes and Brown, 353–4; *Rutland Papers*, ed. William Jerdon (London, 1842), 13; *A Collection of Ordinances and Regulations for . . . the Royal Household*, Society of Antiquaries (London, 1790), 160 (cap. 76); see also p. 147 of this last work where yeoman ushers were "to be taken and elected."

Far more significant than what actually happened in 1483 (for Richard's reign and life proved short) were the fabulous legal fictions in the people's petition, a purported version of which was entered on the *Roll of Parliament* in January 1484. This enrollment made its contents not only true and right in law, but operative in politics. Most notable were the constitutional fiction (revived in 1689) of an election by the three estates of the realm and their identification with parliament. The parliamentary version of the petition based Richard's right to rule upon three sanctions: "lawful election," "true inheritance," and "God's grace." There had been no formal wager of battle (though force had been used and blood shed) to enable God to make evident His choice or to admit justification by conquest, but the petitioners presumed to know His will. In the good old days when "our Lord God was dreaded, loved, and honored," England's rulers had sought "the common and politic weal of the land." The nation then had enjoyed "great prosperity," for England's kings had "followed the advice and counsel" of persons with "policy and experience, dreading God." In contrast, Edward IV in his "rule and governance of this land" had "followed the counsel of persons insolent, vicious, and of inordinate avarice, despising the counsel of good, virtuous, and prudent persons." Therefore, "felicity was turned into misery, and prosperity into adversity, and the order of policy, and of the law of God and Man confounded."³⁸

Proof that God willed Richard's inheritance to the crown to be the "true" one was the allegation that Edward IV had "stood married and troth plight" to Eleanor Butler by "a precontract of matrimony." This technical point of canon law permitted Richard to deduce that Edward IV had lived "sinfully and damnably in adultery" with Elizabeth, his queen. Moreover, the "pretensed marriage" had been made by sorcery and witchcraft and "without the knowing and assent of the lords of this land." This supposed adultery was, of course, "against the law of God and of His Church" and "the laudable custom of the Church of England." Finally, it perverted "all politic rule" and "the laws of nature and of England." Dr. Shaw had bastardized Edward IV himself, alleging a different paternity, but someone had the delicacy to delete this charge and so save the duchess of York from this indecency. Richard's other older brother, George, duke of Clarence (d. 1478) had left issue, but they were barred from claiming the crown "by an act made in the same parliament" that had attainted Clarence of high treason. Thus several kinds of law—divine and natural, canon, common, and statute—cleared the way for Richard to become the true heir to the English crown. Finally, the three estates prayed that "our Lord God, King of all Kings, . . . lighten your soul and grant you grace . . . so that after great clouds, troubles, storms, and tempests, the Son [sun?] of justice and of grace may shine upon us, to the comfort and gladness of all true Englishmen."³⁹

A nascent nationalism also supported Richard's succession, for he had been "born within this land." Edward IV was born at Rouen, and so Richard

³⁸ *RP*, 6: 240, 241.

³⁹ *Ibid.*, 241.

would be "more naturally inclined to the prosperity and common weal of the same." His birth was excellent, for he was "descended of the three most royal houses in Christendom, that is to say, England, France, and Hispanie." In addition to all this, the three estates, according to their petition, reinforced Richard's right to rule with their authority expressed through their election, one that Thomas More later called a "mock election." Yet More himself had King Richard III declare to the estates that to his title as "right heir . . . is now joined your election, the nobles and commons of this realm, which we of all titles possible take for most effectual."⁴⁰

No king of England did so much—on paper—as did Richard III to raise parliament's position in the frame of government. He exalted its authority in theory—and theory, Maitland remarked, "is more permanent than practice." The parliament of 1484 explained "how that the court of parliament is of such authority, and the people of this land of such nature and disposition, as experience teacheth, that manifestation and declaration of any truth or right, made by the three estates of this realm assembled in parliament, and by authority of the same, maketh, before all other things, most faith and certainty; and quieting men's minds, removeth the occasion of all doubts and seditious language." Such a declaration put parliament well on the road toward supremacy or sovereignty, for by implication it imposed upon Richard III a responsibility to that high court as his Maker. Evidence for this view dates from before the usurpation. The dying Edward IV had named his brother Gloucester to the Protectorship in early April, but Richard had found in practice that even this appointment needed approval by the council on May 10 before it carried legal weight. Moreover, this recognition, too, was soon regarded as insufficient. When Edward V's chancellor, Bishop John Russell, drafted a sermon with which to open parliament on June 25 (a meeting that Richard's *coup* prevented), he firmly asserted that the duke's "power and authority" as Protector had still "to be assented [to] and established by the authority of this high court." The ensuing summer and autumn were filled with signs of opposition to Richard's rule, like Buckingham's rebellion and Henry Tudor's first abortive invasion. Small wonder, then, that the parliament of January 1484 should have recorded that doubts and ambiguities had arisen in some men's minds about the validity of the new king's title, because "neither the said three estates, neither the said persons which in their name presented and delivered . . . the said roll [asking Richard to assume the crown] . . . were assembled in form of parliament." Or that this parliament should have ordained and established that "the tenor of the said roll . . . delivered to . . . the king in the name and on the behalf of the said three estates out of parliament, now by the same three estates assembled in this present parliament [1484], and by authority of the same, be ratified, enrolled, recorded, approved, and authorized."⁴¹

⁴⁰ *Ibid.*, 240, 241; More, *History*, 82, 80.

⁴¹ F. W. Maitland, *Constitutional History of England* (Cambridge, 1946), 197; Chrimes, *Constitutional Ideas*, 178; *RP*, 6: 240-1; Wood, "Edward V," 249, 251, 255-7.

The *Roll of Parliament* further stated that everything in the petition was to "be of like effect, virtue, and force, as if all the same things had been so said . . . in a full parliament and by authority of the same accepted and approved." Here the potent little words, "as if," indicate clearly that things done "in a full parliament," whether by or for the three estates, carried the highest possible earthly authority. Thus parliament's action in 1484 gave legality, if not legitimacy, to Richard III's "lawful election" by the three estates back in June 1483. At that time, the estates had "chosen in all that in us is, and by this our writing choose you, high and mighty prince, into our king and sovereign lord. . . . We humbly desire, pray, and require your said noble grace, that, according to this election of us the three estates of this land, as by your true inheritance, you will accept and take upon you the said crown and royal dignity." In return for Richard's acceptance of the crown, the three estates made a promise—or contract—with the new king: "We promise to serve and assist your highness, as true and faithful subjects and liegemen, and to live and die with you in this matter, and every other just quarrel."⁴² Thus did the subject's oath of allegiance join with the king's coronation oath to give the English monarchy its contractual nature.

Under Richard III, then, the three estates of the realm spoke for the kingdom. In 1483 they had made their pronouncements "out of" parliament; but when a continuing opposition to the regime demonstrated that this sanction was insufficient, the estates were compelled to repeat their declaration "in" parliament in January 1484 to give it both practical effect and legality. If what the three estates had said in June continued to be taken for legal truth, this was only because parliament in January affirmed it true. The legal fiction used in June was now to be accepted "as if" declared "in a full parliament and by authority of the same." Thus in law the idea of the three estates proved convenient and useful for Richard III. It was an idea, however, that his successor, Henry VII, tried initially to avoid, for its clear statement of "election" implied a limitation on the kingship. The fiction of the three estates put upon the right to rule a restriction scarcely compatible with Henry Tudor's exalted regal intentions.⁴³ When he arrived on Bosworth Field in August 1485 to depose Richard III, he found that the king had, quite literally, hung his crown upon a hawthorn limb as the prize for the victor. Nevertheless, Henry VII's victory alone did not seem to provide him sufficient title.

The battle won, King Henry entered London on September 3 with the crown, that circlet of gold and precious stones, firmly in his possession. His formal coronation took place on October 30, and he met his first parliament on November 7, 1485. The first Tudor king may or may not have let the

⁴² *RP*, 6: 240, 241.

⁴³ Once Henry VII's right to rule was established, he showed no hesitation in using the fiction of the three estates when it suited his convenience: for example, the renewal in 1499 of the Treaty of Étampes of 1492 in which the king's letters patent to Louis XII of France state that "ratification . . . has been made by the three estates of the realm of England of the said peace." In fact, though, the signatories were simply thirty-five members of the king's whole or great council, parliament not then being in session. *Letters and Papers Illustrative of the Reigns of Richard III and Henry VII*, ed. James Gairdner (London, 1863), 2: 338.

archbishop at his coronation speak of his election by the three estates of the realm as the revisers of Richard III's coronation order had proposed. But to prevent any ambiguities about the validity of his right to rule, "the communities [*communitates*] of the realm of England presented to the king in parliament" a bill in English. It consisted of a single, long, but very straightforward, sentence:

To the pleasure of Almighty God, the wealth, prosperity, and surety of this realm of England, to the singular comfort of all the king's subjects of the same, and in avoiding of all ambiguities and questions, be it ordained, established, and enacted, by authority of this present parliament, that the inheritance of the crowns of the realms of England and of France, with all the preeminence and dignity royal to the same pertaining, . . . be, rest, remain, and abide in the most royal person of our now sovereign Lord King Harry the VIIth, and in the heirs of his body lawfully comen, perpetually with the grace of God so to endure, and in none other.

This bill was read, heard, and understood "by the assent of the lords spiritual and temporal, being in the said parliament, at the request of the said community, and by authority of the same parliament." Henry VII's answer, in French, was that the king "wills it in all points."⁴⁴ Thus did Henry VII seal the bond between the kingdom and the crown: in political theory through the three estates of the realm, but in political practice through parliament. This contract, based solely on an act of parliament, and not on any appeal to the estates meeting "out of" parliament, gave to the king and his lawful heirs the right to rule. Here, surely, was a true and proper parliamentary title for the first of the Tudor dynasty. Its wiser members, despite their intimacy with the Deity, were not to forget that theirs was a dynasty made-in-parliament and that they needed that high court's collaboration to rule with success.

By 1485, then, this series of five depositions had contributed greatly to Englishmen's understanding of the authorities by which they were ruled. Under normal circumstances their governance was primarily regal, but in the process of removing unsatisfactory kings, men of politics and law gradually set limits beyond which a reigning monarch could not safely go. Otherwise, he risked losing the support of those men whose approbation, as expressed through their aid, counsel, and consent, he needed to rule. These limits took shape during the course of the first three depositions—in 1327, 1399, and 1461—at each of which the right to depose the king was predicated on definitions of the Bad King (one who had violated England's law, custom, and morality) and/or the Useless King (the incompetent executive and inept politician who had not mastered the art of handling men). And even though Edward V's principal defect in 1483, his non-age, did not fit this emerging pattern, two years later Henry VII demonstrated that these definitions had won a permanent place in public law. For the first Tudor king was not content to leave the judgment to God alone, as rendered at Bosworth, but was

⁴⁴ *RP*, 6: 270; cf. *Statutes of the Realm*, 2: 499; S. B. Chrimes, *Henry VII* (London and Berkeley, 1972), 60, n. 1, states that Richard III's coronation oath was "presumably the same as that taken by Henry VII."

quick to bring a bill of attainder proclaiming "the unnatural, mischievous and great perjuries, treasons, homicides and murders, in shedding of infants' blood, with many other wrongs, odious offenses and abominations against God and man, and in especial our said sovereign lord, committed and done by Richard, late duke of Gloucester, calling and naming himself by usurpation King Richard the Third."⁴⁵ So began the legend of bunch-backed Richard the monster, but there was in the beginning of this myth also a summing up: one in which, through five depositions, bad history had made good law.

Furthermore, these depositions led Englishmen better to understand the circumstances under which a king could lose his right to rule. They served also to underscore a gradual, though nonetheless remarkable, change in men's thinking about the authority needed to sanction a new regime. At all five depositions the new king and his advisers made every effort to justify his title and his right to rule. Many of their claims were traditional—the standard appeal to a true inheritance from a once-reigning sovereign; the equally universal sanction derived from God's approval as indicated by military victory (at Towton and Bosworth) and by His sacraments of anointing and crowning. Conquest and force, though often the practical reality among these descendants of William the Bastard, were scarcely mentioned in their apologies. In politics, might may have made right, but in law, few men were eager to assert that brute strength provided their ultimate justification.

Instead, men of politics and law, and the new kings they supported, came increasingly to make their final appeal to the authority of the kingdom. In 1327 this authority appeared only in references to the *vox populi* and "the whole community of the kingdom." By 1399 it had grown to the point where "the estates and the people," who constituted the kingdom and implicitly possessed its authority, could authorize the deposition of one king and the election or choice of his successor. By 1460 the legal fiction of the estates of the realm, now three in number, had begun to fuse, albeit tentatively and under pressure of extreme circumstances, with parliament and its institutional authority. This process continued and took near-final form in 1483–84 when Richard III discovered that his own election by the three estates, to have both practical and constitutional validity, could not take place *out of* parliament, only *in* it. This development showed that, despite the continuing need for royal writs and regal ceremonial, parliament's authority now derived from that of the kingdom; and hence that in politics, if not in law, that body had become transcendent. Thus by 1485 five depositions and Henry VII's Act of Succession had somewhat dimmed those "sparkles of the Divinity" with which King James I attempted to adorn his imperial person before parliament in 1605.⁴⁶ In their stead, many an Englishman saw the glittering spangles of nationality that had begun to embellish the king's high court of parliament, now also the kingdom's representative.

⁴⁵ *RP*, 6: 276.

⁴⁶ *The Political Works of James I*, ed. Charles H. McIlwain (Cambridge, Mass., 1918), 281.

Art and Politics in Cold War America

JANE DE HART MATHEWS

PUBLIC PATRONAGE OF THE ARTS, instituted during the depression as a New Deal measure to provide work for the unemployed, was revived in the Cold War years as part of America's struggle against communism. From this fusion of art and politics issued a form of "free world" advertisement—the cultural exchange program. Ironically, however, the program that was intended to demonstrate that the United States was not a nation of materialists ran afoul of people who were themselves devoted opponents of Communist aggression, but who believed that this effort, in the visual arts especially, contained disturbing evidence of domestic subversion. Recognizing the power of art as a medium of influence and mode of communication, ardent anti-Communists in Congress, notably Michigan Congressman George Dondero, supported by local counterparts, inveighed against works reflecting social commentary that they deemed subversive, whether in an overseas embassy or a local post office, with a zeal scarcely matched since the thirties. Lambasting individual artists who allegedly had Communist associations, they demanded that works by such artists, no matter how innocuous the subject matter, be banned from publicly supported arts institutions and most especially from federally sponsored cultural exchanges. Finally, they attacked modern art itself as an instrument of Communist subversion in terms that blended fact with fantasy.

These three forms of expressing political discontent by focusing on art were really three stages of one process. The initial stage, opposition to social commentary in predominantly representational art, involved at first little more than an objection to the particular message conveyed by artists who used their work to communicate what they perceived as social injustice. The second stage, objection to the political affiliations of the artists, involved a more complicated response because it attempted to link ideology and art in the person of the artist irrespective of the content of specific works. The objection to modern art as Communist conspiracy, the final stage, involved a yet more "sophisticated" thought process, for the assumption was that rejection of traditional ways of seeing form and space inherent in vanguard style of painting implied rejection of traditional world views. Iconoclasm in art, in

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other words, was assumed to extend to the broader realms of the cultural and social order, threatening ultimately all established norms and values.

Most Americans, to be sure, probably did not like or understand modern or abstract art; but most of them, too, could simply "take it or leave it." And certainly most informed opponents of the form would never identify it as part of a Communist conspiracy. Indeed, most Americans would not confuse their esthetic judgment or preferences with political commitments; but some people would do so in a public and dramatic way. And they would demand that others follow their lead in making deviation from their own esthetic canon, not a matter of taste, but of deviation from their standard of "Americanism."

Although such a peculiar conflation of politics and esthetics was commonplace in the United States during the late forties and fifties, it would be a mistake to view the assault on modern art solely in political terms, for the inability to tolerate new esthetic values and modes of perception reveals much about the psychological sources of antiradicalism in this country as well as the symbolic significance of public patronage. Since congressional critics were aided by certain representational artists who feared the growing ascendancy of abstract expressionism, their attacks also illuminate the dynamics of the modernist-traditionalist controversy that flared up again in the fifties: politics not only became esthetics but esthetics became politics. To focus on those anti-Communists who made internal subversion a political issue and their alliance with disgruntled traditionalist painters and sculptors, however, is not to suggest a simplistic or invariable correlation between political and esthetic traditionalism—or conversely, political and esthetic radicalism. Rather it affords a unique opportunity to probe some of the pressure behind the impulse to prevent certain modes of art from being styled as "American"—indeed, to label them as "un-American." More specifically, by focusing on this unique esthetic-political continuum, it is possible to show why modern art, which Communists themselves hated and feared no less than their American enemies, should have become emblematic of "un-Americanism"—in short, cultural heresy.

While opposition to works of art never assumed the proportions in this nation that it did in totalitarian countries, where offending artists often suffered imprisonment as well as loss of official patronage, the impulse to censor was undeniably present in the United States in the Cold War years. The simplest and most direct expression of that impulse involved opposition on political rather than esthetic grounds. Critics objected to the message communicated through immediately perceptible images conveying the artist's convictions about past injustices and present wrongs. Predictably, therefore, criticism developed about the social commentary inherent in such works as Ben Shahn's *Hunger* and Robert Gwathmey's *Work Song* when, in 1946, the State Department purchased its first collection of art for exhibit abroad. But the paintings that drew sharpest attack were not part of that ill-fated collection; rather, they were a part of San Francisco's Rincon Post Office Annex murals.

The controversy, like the murals themselves, was reminiscent of the thirties. One of the last and most expensive (\$26,000) of the mural projects initiated by the Treasury Department's Section of Fine Arts, this 240-foot painting depicting the history of San Francisco was the work of Anton Refregier. An established muralist, whose sketches had been chosen from among those submitted by eighty-one competing artists, Refregier, like most socially conscious artists in that depression decade, believed that art must address itself to contemporary issues and that a mural painting in particular must not be "banal, decorative embellishment," but a "meaningful, significant, . . . powerful plastic statement based on the history and lives of the people."¹ His preliminary designs reflected that conviction. Although the jury approving the sketches praised the coherence of the historical element, federal officials were ever mindful that these were public buildings being decorated with public funds.² In such circumstances, the mood was one of caution: artistic freedom had to yield to public accountability. No artist, however distinguished, escaped the "heavy, if well meaning, hand" of federal supervision.³

Refregier was no exception. There was the usual haggling with officials of the Public Buildings Administration about minor details in the murals as work was resumed after the war. A VFW cap had to be painted out since veterans' groups had protested the presence of a man with such a cap in a strike scene—newspaper pictures to the contrary notwithstanding. By the same token alteration had to be made in a scene that depicted a parade celebrating the winning of the eight-hour day. Officials also objected to the inclusion of a huge head of FDR in a panel on the Four Freedoms, despite the artist's protest that a portrait of the late president was not only historically appropriate but a necessary focal point.⁴ But such changes, while not uncommon on mural projects of the thirties, were insufficient to stifle criticism from civic groups, veterans' organizations, and patriotic and fraternal societies in a nation where concern about Communist subversion had been intensified by the Hiss trial and the "fall" of China.⁵

When the completed project was finally unveiled in 1949, the L-shaped

¹ Anton Refregier, *Government Sponsorship of the Arts* (New York, 1961), 7. Quoted in Gladys M. Kunkel, "The Mural Painting of Anton Refregier in the Rincon Annex of the San Francisco Post Office, San Francisco, California" (master's thesis, Arizona State University, 1969), 27.

² Kunkel, "Rincon Annex Murals," 33. Under terms of his contract, Refregier was to revise his preliminary designs until they met the requirements of the Commissioner of Public Buildings, and he was not to proceed with the execution of the murals until the commissioner or his duly authorized representative had given approval.

³ Richard D. McKinzie, *The New Deal for Artists* (Princeton, 1973), 60-61.

⁴ For details of the VFW controversy, see the *San Francisco Chronicle*, May 17, 1948, and the *San Francisco Examiner*, Oct. 27, 1948. The dispute over the eight-hour day celebration is discussed in William Hauptman, "The Suppression of Art in the McCarthy Decade," *Artforum*, 11 (1973): 52. For the Roosevelt portrait controversy, see the *Examiner*, Nov. 14, 1947, and also Refregier's letter to Gladys M. Kunkel quoted in her "Rincon Annex Murals," 166. For additional details see Matthew Josephson, "The Vandals are Here: Art is Not for Burning," *Nation*, 177 (1953): 244-48; and Amy Robertson, "Refregier Paints a Mural," *Art News*, 48 (1949): 32-34.

⁵ Opponents included the California Council of Republican Women, the DAR, the Native Sons of the Golden West, the American Legion, the VFW, the Sailors' Union, and the Society of Western Artists, a traditionalist group made up largely of commercial illustrators.

lobby of the Rincon Annex was panelled with a series of vibrantly colored, generally well-designed and executed paintings that carried San Francisco history from exploration and conquest, past the gold rush, the building of the Union Pacific, the disastrous earthquake and fire, and into the twentieth century and a second World War, culminating in the signing of the UN charter.⁶ Delighted with their artistic quality, the San Francisco Art Association called Refregier's work "among the most distinguished mural paintings executed in this country in the past generation."⁷ But other critics, perhaps accustomed to the noncontroversial character of most of California's New Deal art, found little to admire about the muralist or his murals.⁸ Refregier, a United States citizen since 1930, was naturally suspect because of his Russian background and myriad leftist activities.⁹ As for the murals, all twenty-nine panels were done in a kind of stylized realism that resulted in simple, elongated angular figures disturbing to many viewers—"Frankenstein monsters" was the appellation of a disappointed legionnaire who lamented that such art could never be "pleasing and uplifting."¹⁰

If style was offensive, content was more so. Denouncing the paintings as "artistically offensive," "historically inaccurate," and "subversive," opponents charged that they cast a "derogatory and improper reflection" on the character of the great state of California.¹¹ Specifically critics objected to scenes dealing with the California fire and earthquake, vigilante activity, Chinese coolie labor, the Mooney case, and labor difficulties on the waterfront. Such subjects, they complained, placed disproportionate emphasis on violence, racial hatred, and class struggle. Detail was as subversive as the general theme. Making monks "cadaverous" or "potbellied" was anticlerical; putting the British flag above the American flag in a panel on World War II was unpatriotic; painting a hammer and sickle on the Soviet flag in the scene depicting the signing of the UN charter was subversive, and portraying the U.S. representative with "mule's ears" was worse still. So the list went. With even San Francisco's Young Democrats insisting that the presence of such murals in a public building was "little short of treason," public officials responded accordingly.¹² In a letter to a California veteran, Representative Richard M. Nixon had previously stated that, while he realized that "some very objectionable art of a subversive nature" had been allowed to go into federal buildings in many parts of the country, he believed that there was no remedy under the present administration. As [*sic*] such time as we may have a

⁶ For a fuller description see Kunkel, "Rincon Annex Murals."

⁷ *San Francisco Chronicle*, Mar. 10, 1953.

⁸ With the exception of the Coit Tower murals that included certain radical symbols, most federally subsidized art in California during the thirties was "overwhelmingly bland and noncontroversial," according to Steven Gelber. See his "New Deal Art in California" (paper presented at the Apr. 1976 meeting of the Organization of American Historians at St. Louis).

⁹ *Congressional Record*, 82nd Cong., 2nd Sess. (1952), A3765; *San Francisco Chronicle*, May 21, 1953.

¹⁰ Charles E. Plant to W. E. Reynolds, Aug. 28, 1950, George Dondero Papers, Archives of American Art (Washington, D. C.).

¹¹ *San Francisco Chronicle*, May 2, 1953; *San Francisco Examiner*, May 23, 1953.

¹² Accusations are detailed in Aline B. Louchheim's articles in the *New York Times*, May 2, 10, 21, 1953, and Alfred Frankenstein's in the *San Francisco Chronicle*, Mar. 10-13, Apr. 3, May 2, 1953.

change in the administration and in the majority in Congress," he wrote, "I believe a Committee of Congress should make a thorough investigation of this type of art in Government buildings with a view to obtaining removal of all that is found to be inconsistent with American ideals and principles."¹³ Nixon's fellow California representative, Hubert B. Scudder, agreed.

By 1953, the conditions that Nixon believed necessary obtained: elections the previous fall had swept into office a Republican president and Congress. More important, the Chairman of the House Committee on Public Works was George Dondero. A Michigan Republican who frequently denounced Communism in the arts, Dondero promptly appointed a subcommittee of the Committee on Public Works to consider removal of murals that he considered "an insult to every loyal American."¹⁴ Accordingly, Scudder introduced H.J. Resolution 211, which directed the Administrator of General Services to remove the twenty-nine murals on the grounds that: 1. they were derogatory to and an improper reflection on the character of California's pioneers; and 2. they contained Communist propaganda. In hearings before the House subcommittee, Scudder was supported in his effort by a colleague from the House Un-American Activities Committee who read at length from committee files of the many instances when Refregier had lent his name to publications, resolutions, congresses, and organizations listed by the Attorney General as subversive. The Commissioner of Public Buildings, who was responsible for awarding the contract to the Russian-born artist, explained that he personally had never liked the sketches. Most damning, however, were Scudder's own remarks. Charging that the murals were not only historically inaccurate, but "slandrous" and "subversive," he inserted into the record supporting statements from critics charging that the murals failed to depict "the romance and glory of the West" and tended instead to promote "racial hatred and class warfare." Other witnesses seconded his objections, agreeing that the murals did indeed project Communist propaganda.¹⁵

Opponents of removal had also come prepared. California Congressmen John Shelly, a liberal Democrat, and William Maillard, a moderate Republican, were both outspoken in opposing the resolution of their conservative colleague.¹⁶ Attesting to the accuracy of the murals, Shelly readily admitted that a great deal of California's early history was not to his liking. The Chinese labor riots and the Mooney bombing were historical facts, he said. Moreover, this was not the USSR, where artists were judged on their political views. Representatives of the Bay Citizen's Committee to Protect the Rincon Annex Murals and various allied groups such as the San Francisco Museum

¹³ Richard M. Nixon to Charles E. Plant, July 18, 1949, Hudson Walker Papers, Archives of American Art. Nixon appears to have reversed himself somewhat in 1955. When a lithograph called "Dick McSmear" was removed from the San Francisco Art Festival, Nixon wired that city's Art Commission on behalf of the artist's freedom of expression. See *Art News*, 54 (1955):7.

¹⁴ George Dondero to the Michigan American Legion, Dec. 1, 1956, Dondero Papers.

¹⁵ Hearings before the House Subcommittee on Public Buildings and Grounds of the Committee on Public Works on H. J. Res. 211, *Rincon Annex Murals, San Francisco*, 83rd Cong., 1st Sess. (Washington, 1953), 1-42.

¹⁶ See Congressional ratings, published in the *ADA World*, 1949-53.

and the San Francisco Art Association, were also on hand to rebut Scudder's charges. The Committee, consisting of what the San Francisco *Chronicle* characterized as an "imposing array" of leaders in business, finance, the arts, and society, had been organized in part through the efforts of Grace McCann Morley, Director of the San Francisco Museum of Art. She was convinced of the flimsy basis of the arguments for removal and the alarming precedent that it would afford.¹⁷ Speaking first for the Committee, Attorney Chauncey McKeever submitted a nine-page statement condemning the resolution. Citing testimony of authorities in the California Historical Society, he argued that the murals were historically accurate as well as esthetically sound and politically safe. Though certain minor changes might be desirable in a few panels, such as the UN scene, to pass the resolution, he insisted, would be to yield to the prejudice of uninformed pressure groups. Thomas Carr Howe, Jr., Director of the California Palace of the Legion of Honor, agreed, as did representatives of the American Federation of Art and Artists Equity. From these and other nationally based artists' groups the message was the same: regardless of style, subject matter, or the political beliefs of the artist, to destroy a work of art was to destroy freedom of expression.¹⁸

In the days that followed, supporters of the murals continued to plead their case. In San Francisco, the *Chronicle* not only covered the controversy fully, devoting substantial space to a detailed statement attesting to the historical veracity of the episodes Refregier had chosen to portray, but also editorialized on behalf of preservation, suggesting that the Scudder resolution was reminiscent of events in totalitarian societies.¹⁹ And in Washington, the House subcommittee was inundated with mail—some of it advocating the destruction of murals that functioned as "an instrument of Soviet psychological warfare," but much of it arguing for preservation of important works of art. Written not only by ordinary San Franciscans, but also by museum directors and art experts across the country, the letters attacked removal as "official cultural vandalism." Arguing on behalf of artistic freedom, Lloyd Goodrich of the Whitney pointed out that opposition to the murals had come from only "a few reactionary elements" in California politics and art. Julian Huxley, former Director-General of UNESCO, deplored such "witch-hunting" in the United States. The murals' removal, he said, was an "example of the growing tendency" in this country "to exert political control over freedom of thought and expression." And from Artists Equity came the reminder that passage of

¹⁷ San Francisco *Chronicle*, May 1, 1953; Grace Morley to Jane De Hart Mathews, Aug. 19, 1975. Indicative of the insubstantial character of the charges against the murals was the assertion that the artist had painted mule's ears emanating from the head of the U.S. representative in the UN panel. But when the painting was examined *in situ*, the forms in blue behind the figure of the American representative which might conceivably be construed as mule's ears in a photograph of the panel were quickly recognized as swags of the blue drapery that decorated the stage of the San Francisco Opera House, where the Plenary Sessions of the UN were held. Morley's efforts on behalf of the murals are documented in letters such as that of Morley to J.D. Zellerbach, Apr. 10, 1952, Hudson Walker Papers. Zellerbach, who was president of the Crown-Zellerbach Corporation, was only one of the many corporate leaders whose support she sought. For a complete listing of the supporters of the Citizen's Committee, see the *Chronicle's* May 1 article.

¹⁸ *Rincon Annex Murals*, 43ff.

¹⁹ San Francisco *Chronicle*, May 1, 18, 1953.

the Scudder resolution might deal an irreparable blow to America's cultural standing abroad at precisely the time federal officials were trying to improve that standing through cultural exchanges.²⁰

The issue was clearly complicated, because Refregier, ever willing to use his art as political commentary, had deliberately chosen to depict controversial issues such as the Mooney case and the anti-Chinese riots in the conviction that the public must be reminded of the "unfortunate aspects" of its history if such events were not to be repeated.²¹ In the end, however, the subcommittee ruled that removal was an administrative matter, and since the Commissioner of Public Buildings insisted that the murals could not be taken down without explicit authorization from Congress, they remained intact. Still, efforts at censorship persisted, and as late as 1956 California veterans continued to explore strategies for the removal of the murals.²² That they persisted was a measure of the intensity of feelings aroused when art was used by social realists to express the artist's hatred of injustice. That they ultimately failed was a result, at least in part, of the effectiveness with which prominent San Franciscans mobilized themselves and their allies on behalf of artistic freedom, for without such action, anti-Communist crusades leading to patriotic excesses could fast gain momentum in the deteriorating political climate of the early fifties.

Fed by frustrations with a U.S. foreign policy that failed to "win" diplomatic victories against the Soviet Union, these crusades became increasingly concerned with internal subversion. Predictably, this effort to ferret out the "enemy within" led to persons who had participated in groups, activities, or causes sponsored by "Communist-front" organizations during the thirties. Artists were naturally among these suspected subversives, not only because of the content of specific works, but also because of past personal or professional associations. Thus self-styled anti-Communists, operating at this second level of sophistication, attempted in the course of such attacks to unite ideology and art through the person of the artist. Events in Dallas, Texas provided a case in point. That this burgeoning center of new wealth should have become the scene of controversy is hardly surprising. The headquarters of millionaire H. L. Hunt, the city had been exposed to his national radio and television broadcasts as well as numerous publications such as the *American National*

²⁰ I am especially indebted to the Chairman of the House Committee on Public Works for special arrangements to read at the Committee office all correspondence relating to the Scudder resolution. The material, "Considerations of H. J. Res. 211, 83rd Cong., 1st Sess. (1953): 'To direct the Administration of General Services to remove the mural paintings from the lobby of the Rincon Annex Post Office Building, San Francisco, California,'" although a part of Record Group 121 at the National Archives, is not otherwise available. "The Rincon Annex Murals: A Psychological Warfare Criticism," a ten-page typed manuscript, is contained in the files, as are the other arguments referred to above in letters from: Alfred H. Barr, Jr., Apr. 21, 1953; Lloyd Goodrich, Apr. 29, 1953; Julian Huxley, Apr. 9, 1953; Lincoln Rothschild, Apr. 22, 1953.

²¹ Interview with Anton Refregier, Jan. 2, 1975. I am grateful to Helen A. Harrison who conducted the interview for providing me with a transcript.

²² George Dondero to Charles E. Plant, June 29 and Nov. 19, 1956; Dondero to Arthur E. Summerfield, Nov. 19, 1956; Dondero to the Michigan American Legion, Dec. 1, 1956, Dondero Papers.

Research Report, which "named" Communists and "Communist-front" organizations, describing their activities in detail.²³ Another Dallasite, Colonel Alvin Owsley, had begun compiling his own directory in 1921, which, with names from the annual reports of the House Un-American Activities Committee and congressional speeches, constituted a formidable list of "subversives" against which local citizens could check the names of writers, composers, actors, and artists that appeared in local newspapers and museum catalogs.²⁴ Although certain names had involved the Dallas Museum in a variety of conflicts, the incident that brought the loudest reverberations was the showing of "Sport in Art."²⁵

Assembled by the American Federation of Art for *Sports Illustrated*, the exhibit was to tour the United States during 1956 before going to Australia under USIA auspices for the Olympics. Two months before its arrival in Dallas, however, local anti-Communists demanded the exclusion of four paintings because of the alleged subversive associations of the artists involved. The four included Ben Shahn's drawing of a baseball game entitled "National Pastime," a skating scene by Yasuo Kuniyoshi, another winter scene with ice skaters by Leon Kroll, and a painting of an elderly fisherman by William Zorach.²⁶ Although Shahn's work, like that of other Social Realists, often served as a means of communicating the artist's political and social values, these particular paintings bore no "message." But however innocuous the works themselves, all four artists had been linked to "front" organizations in the thirties. According to the House Un-American Activities Committee, Shahn, for example, had associated with numerous radical groups, publications, and causes, among them the John Reed Club, the American Artists Congress, the Spanish Refugee Relief Committee, *Masses and Mainstream*, and the National Council of American-Soviet Friendship.²⁷ Museum trustees nevertheless issued a statement saying that the museum acquired and exhibited works solely on the basis of merit.²⁸ Supporters also pointed out that the exhibit, which would be cosponsored by Neiman-Marcus, cost local taxpayers

²³ Charlotte Devree, "The U.S. Government Vetoes Living Art," *Art News*, 55 (1956), 35.

²⁴ *Ibid.*

²⁵ *Ibid.* According to Devree, the Public Affairs Luncheon Club, a women's group of some 400 members, had previously taken the Dallas Museum to task for its modernist tendencies and for its exhibitions of work by artists with "known" Communist affiliations. Under the leadership of Stanley Marcus, owner of Neiman-Marcus, the museum's trustees initially resisted the demands of these critics, only to capitulate when Marcus' successor was in charge. The latter called on Congressman Dondero for advice, circulated lists of "subversive" artists among museum trustees who, in turn, issued a statement that it was not the museum's policy knowingly to acquire or exhibit works by persons known to be Communist or to have "front" affiliations. Not surprisingly, two subsequent exhibitions came under attack: "In Memorium" because twelve of the artists, all recently deceased, had "leftist associations"; and "Sculpture in Silver" because of William Zorach's participation. Neither exhibit was federally funded.

²⁶ *Ibid.* The controversy was followed closely by the *Dallas Morning News*, Feb. 1, 3-5, 7, 23, 29, 1956, and the *New York Times*, Feb. 12 and Mar. 4, 1956, sec. 2.

²⁷ Attacks on Shahn's leftist associations, which may be found in Dondero's early speeches, continued through the "Sport in Art" controversy and culminated in an investigation by the House Un-American Activities Committee. See *Congressional Record*, 81st Cong., 1st Sess. (1949), 11586, and Hearings Before the House Un-American Activities Committee, *The National Exhibition, Moscow*, 86th Cong., 1st Sess. (Washington, 1959), 941-50. Cited hereafter as HUAC Hearings.

²⁸ *Dallas Morning News*, Feb. 1, 1956.

nothing.²⁹ But the museum's critics were in no way mollified. Convinced that the trustees were little better than Communist "dupes," and knowing that public funds were funneled through the Park Board to the museum for maintenance, they appealed to the Park Board to ban the exhibit.³⁰ In subsequent hearings both the identity of these self-styled patriots and the nature of their argument became apparent.

Opponents of the museum's policy were led by the Communism in Art Committee of the Dallas Patriotic Society. The society itself consisted of a federation of some sixteen clubs, principally veterans' groups, patriotic associations, and an assortment of local art clubs.³¹ Their presence in the Patriotic Society owed much to the energetic efforts of Reveau Basset, a local resident and traditionalist artist, who frequently spoke to groups of women amateur painters about the dangerous modernist tendencies of the Dallas Museum.³² By failing to remove the works of Communist artists, they charged, the Dallas Museum was aiding communism, inasmuch as exhibitions in a leading museum gave the artist standing that could be translated into monetary terms. And money, of course, could be contributed to Communist causes that in turn would harm America. There was, spokesmen for the Patriotic Council concluded, a "well-organized apparatus operating to exhibit the works of Communists, however inferior, in preference to the works of others, however superior."³³ Unconvinced, the Park Board rejected the request to withdraw "Sport in Art" amidst mutterings about its members' having gone soft on communism.³⁴ The exhibit went on as scheduled, though critics stationed themselves beside the pictures in question to warn the public that these were the works of "Reds."³⁵ But any notion that artistic freedom had triumphed was short-lived.

The clamor of the Patriotic Council in Dallas had repercussions in Washington. Fearing congressional criticism, USIA Director Theodore Streibert reversed the decision to send "Sport in Art" to the Olympics. Moreover, he promptly cancelled an important overseas exhibit of twentieth-century American painting after the American Federation of Art (AFA), which had assembled the exhibit, refused to remove the works of ten artists whom the Agency deemed "social hazards" by virtue of their "front" associations.³⁶ This cancellation, however, was simply the latest in a long line of aborted exhibitions. The State Department had called back an exhibition of American painting as

²⁹ *Ibid.*, Feb. 4, 23, 1956.

³⁰ *Ibid.*, Feb. 7, 1956.

³¹ Included were the DAR, Daughters of 1812, American Legion, VFW, Public Affairs Luncheon Club, Pro-America, 1950 Study Club, Southern Memorial Association, Inwood Lions Club, Matheon Club, Reveau Basset Art Club and Reaugh Art Club.

³² Devree, "The U.S. Government Vetoes Living Art," 35.

³³ *Dallas Morning News*, Feb. 3, 7, 23, 1956. The leading speech at the Park Board hearing was made by Colonel Alvin Owsley. Other speakers included Colonel John Mayo and B. I. F. McCain, also a Legionnaire and a past president of the Dallas Chamber of Commerce. There is evidence to suggest that Owsley's speech may have been written by a Dallas artist.

³⁴ *Dallas Morning News*, Feb. 29, 1956.

³⁵ Devree, "The U.S. Government Vetoes Living Art," 56.

³⁶ *Washington Post and Times Herald*, June 22, 1956.

early as 1947, fired the young man who had put it together, and sold off the paintings at auction amidst charges from Congress and the press that twenty of the forty-five artists represented "various shades of Communism."³⁷ Mindful of this history, the AFA had hoped to avert this latest cancellation by persuading the USIA director to refer the matter to the White House. Eisenhower had issued a declaration on artistic freedom two years earlier as a result of the efforts of Alfred Barr and trustees of the Museum of Modern Art, who had used the Museum's twenty-fifth birthday celebration to elicit a presidential statement.³⁸ The hope was that it might now become more than a paper proclamation. But the AFA was quickly disappointed. Sherman Adams, the president's chief assistant, supported Streibert's decision.³⁹ In discussing his action, the USIA director explained that if he regarded the artists' politics as irrelevant he would surely face congressional criticism and perhaps jeopardize appropriations for the entire agency.⁴⁰ Accordingly, he also cancelled an exhibit put together by the College Art Association because of the inclusion of a Picasso. And in a similar display of caution, the State Department revoked plans for an overseas tour of Toscanini's former NBC Symphony of the Air because 4 of the 101 members of the orchestra were allegedly pro-Communist. Administration "cowardice" was attacked on the Senate floor by Hubert H. Humphrey and on the editorial pages of the *New York Times* and the *Washington Post*.⁴¹ In the context of the political climate of the fifties, however, such caution was perhaps understandable.

Though by 1954 Joseph McCarthy's fellow Senators had voted to censure their colleague, the crusade that bore his name had not yet abated. Fears over national security and internal subversion as well as frustrations over the cost and complexity of the Cold War persisted during the Eisenhower years, prompting the administration to extend the loyalty/security programs instituted in the Truman era. Thus preoccupied with the paraphernalia of those programs—the Attorney General's list, the loyalty oaths—a bureaucratic organization such as the USIA was not prepared to invite the suspicions of those congressmen who were ever ready to use appropriations as a coercive tool with which to enforce their own definition of loyalty.

Many ardent anti-Communists were bothered, however, by more than "Marxist" social commentary in art and "subversive" political associations of

³⁷ See below, footnotes 54 through 57.

³⁸ Lloyd Goodrich interview, Archives of American Art. The American Federation of Arts, which assembled many of the exhibits circulated abroad by the federal government, had issued its statement that same month. See footnote 17.

³⁹ Goodrich interview.

⁴⁰ In one of his many speeches attacking government involvement with "subversive" artists, Dondero praised Dallas citizens for their efforts to prevent the showing of "Sport in Art." He would certainly have taken to task the USIA had the exhibit been sent abroad as planned. See the *Cong. Rec.*, 84th Cong. 2nd Sess. (1956), 10419-25, 13774-849.

⁴¹ Humphrey was one of the first—and few—Senators to speak out against cancellations. See the *Cong. Rec.*, 84th Cong., 2nd Sess. (1956), 10918-20, 12305-6. Senator William Fulbright also inserted into the *Record* editorials criticizing the USIA from the *Times* and the *Post*. (See A5602-3.) With the exception of Jacob Javits, most congressmen were silent throughout the decade, taking what Humphrey called in reference to his Senate colleagues a stand "a little to the left of Grant or McKinley."

artists. Their *bête noire* was modernism, for the rejection of traditional forms and the commitment to abstraction that characterized vanguard art seemed to impart to their highly structured world the quality of chaos and the demonic that they so easily identified with communism. Thus the charges that the promotion of modern art was part of the Communist conspiracy represented the third and most "sophisticated" stage of the argument of those who labeled any deviation from their own esthetic canon, not a matter of taste, but a deviation from their standard of "Americanism."

A number of congressmen were hostile to modern art, but the representative who most consistently and thoroughly emphasized its intrinsic un-American character was Michigan's George Dondero. A slender, erect man with regular features and thinning gray hair, Dondero inveighed for the better part of a decade against everything from State Department exhibits to artists' unions. The real enemy, however, was modern art and those "misguided disciples who bore from within to destroy the high standards and priceless tradition of academic art." With the closely reasoned rhetoric so characteristic of conspiratorial thinking, Dondero argued that modernism had been used against the Czarist government when Trotsky's friend, Wassily Kandinsky, had released on Russians "the black knights of the isms": cubism, futurism, dadaism, expressionism, constructionism, surrealism, and abstractionism. Each was deadly. Cubism, according to Dondero, aimed to destroy "by designed disorder"; futurism, "by the machine myth"; dadaism, "by ridicule"; expressionism, "by aping the criminal and insane"; abstractionism, "by the creation of brainstorm"; surrealism, "by the denial of reason." To be sure, socialist realism ultimately prevailed in Russia itself. But the art of the Revolution had been cleverly retained for subversion abroad. Thus from the "pen-and-brush phalanx of the Communist conspiracy" had come the "front" organizations of the thirties: the John Reed Clubs, the League of American Writers, the American Artists Congress, and their successors, such as Artists Equity.⁴²

Current targets, according to Dondero, were America's cultural centers, which were being infiltrated by modernism with all its "depravity, decadence, and destruction." The Museum of Modern Art was a prime example since its origins went back to the Société Anonyme of which Kandinsky himself had been an officer. Nor had Kandinsky been the only revolutionary apostle of modernism invading America to corrupt its art. A "horde of foreign art manglers" had descended upon this country just before World War II, spreading their pernicious doctrines. Followers of these "international art thugs" now included Americans such as Robert Motherwell, William Bazotes, and Jackson Pollock. Thus as followers increased and as American universities turned out museum directors sympathetic to modernism, the danger from foreign "isms" increased proportionately. Harvard's Fogg Museum, Dondero charged, was one of the worst offenders. The "effeminate

⁴² *Cong. Rec.* 81st Cong., 1st Sess. (1949), 11584.

elect" it sent out to run the nation's museums not only discriminated against traditionalists in juried shows, but also paid inflated prices for modern art that they jammed down the throat of an unwilling public. In sum, a "sinister conspiracy conceived in the black heart of Russia" had become a threat to America's cultural institutions and to those loyal American artists who sought to protect their cultural heritage from the new forms that were the symbols of a foreign ideology. Only if the hard-working, right-thinking patriotic proponents of academic art followed the example of labor unions and ejected Communists, the Michigan Congressman argued, could traditional art be preserved and cultural institutions cleansed.⁴³

Simplistic and uninformed, such an attack can be temptingly dismissed as yet another example of what Richard Hofstadter has called the "paranoid style." However labeled, Dondero's protest and, more important, the tensions and anxieties behind it cannot be discounted, for to do so would be to ignore the concerns and influence of a fearful and militant minority. For at least some Americans Dondero's was an appealing argument. As in most conspiracy theories, there were sufficient elements of truth to elicit belief from those people who seek to escape the complexity of causation by devising simplistic, face-saving formulas wherein fears can be assuaged and anger externalized. Such people may also have resonated to the anti-intellectual and frankly nativistic overtones of Dondero's address: the characterization of Harvard-produced museum directors as an "effeminate elect," the dubbing of this nation's newest emigrés as a "polyglot rabble." Certainly there was little question that Dondero's remarks struck a responsive note in people such as Wheeler Williams, the President of the National Sculpture Society and one of the "real" American artists whom Dondero called to take up arms against this "horde of foreign art manglers." Indeed, Williams publicly lamented the fact that the USIA would send abroad works by a "newly made American" such as Theodore Roszak, while ignoring such established sculptors as the late Daniel Chester French and Augustus Saint-Gaudens.⁴⁴

Undeniably the ethnic and social character of the American art community had changed radically in the early decades of the twentieth century, as first generation Americans, many of them immigrants, flowed into the New York art world. These were the "newly made" Americans—immigrant sons such as the social realist Jack Levine, who discovered art in a settlement house and fame on the WPA. And there were also the emigrés: the "notorious" French

⁴³ The traditionalist organizations that Dondero wanted to take action were the National Academy of Design, the American Artists Professional League, Allied Artists of America, the Illustrators' Society, the American Watercolor Society, and the National Sculpture Society. See *Cong. Rec.*, 81st Cong., 1st Sess. (1949), 6375. For a fuller exposition of his ideas on modern art, see *Cong. Rec.*, 81st Cong., 1st Sess. (1949), 11584-87; 84th Cong., 2nd Sess. (1956), 13774-79. See also his speech to the American Artists Professional League on March 30, 1957, "Communism and Art," Dondero Papers. According to American Federation of Arts representatives who spoke with Dondero, the Michigan congressman believed not only that museum juries were dominated by modernists, but that art journals and art critics of the metropolitan press were "undermining the standards of American culture." See Thomas Parker to [L.C.] Smith, memorandum, Hudson Walker Papers. In fairness, however, one must recognize the extent to which taste and esthetic criteria were and are affected by a few influential museum directors and critics.

⁴⁴ HUAC Hearings, 916.

surrealists, Tanguy, Masson, and Breton; the charismatic John Graham from Russia; Albers and Hofmann from Germany. On the eve of the Nazi invasion, others had followed, among them the great Dutch neoplastic painter and theoretician of abstract art, Piet Mondrian. Thus, by the mid-forties the emigré community virtually encompassed the leaders of every nonrealistic art movement of the twentieth century.⁴⁵ Modernist all, this "polyglot rabble" would help to change the nation's artistic sensibilities far more radically than had a few *avant-garde* paintings in a New York armory some forty years before. But whether social realist or surrealist, such names and the art they represented were understandingly threatening to the eminently respectable academicians who dominated the National Sculpture Society, the American Artists Professional League, and the National Academy of Design. These groups had long enjoyed a monopoly on official art, and they did not choose to relinquish it. Indeed, the American Artists Professional League issued a "War Cry" against "decadent isms," echoing Dondero in their plaint about the "sensationalists" who had "infiltrated *our* large exhibitions, *our* art societies, and *our* museums."⁴⁶ Thus the final perhaps most important appeal was to traditionalists to reassert their hegemony in an art world that was once again caught up in the modernist-traditionalist controversy.

That controversy was, in fact, at the heart of this whole attack on communism in the arts. Raging intensely in the aftermath of the Armory show, then relatively quiescent in the thirties, it flared with renewed vigor against the backdrop of the McCarthy era. While congressional critics of modernism held forth in Washington, Pope Pius XII denounced surrealist and abstract art as "immoral" and "enslaving to the spiritual powers of the soul." In Boston, the Institute for Modern Art changed its name to the less offensive Institute for Contemporary Art, defining neither term precisely, but in the process calling artists to come forward with a "strong, clear affirmation of humanity." In New York, when the Metropolitan Museum included modernist work in a large retrospective of recent sculpture, conservative sculptors took the Museum to task for proving hospitable to a movement that "by destroying an ideal of beauty, endeavor, and discipline in artistic expression" threatened not only art but "the fundamental freedom of our American way of life." At the White House, President Truman received assurances that hundreds of "stifled" realists could be rallied to paint patriotic pictures of America free from the distortion and disharmony inherent in the works of Communist-inspired modernists.

On the defensive once again, the modernists tried to counter the attack. At the New School for Social Research, where a curtain was subsequently placed over an Orozco mural entitled "Revolutionary Violence," Stanley Hayter

⁴⁵ For a fuller assessment of the immigrants' impact, see Jo Ann Lewis, "Immigrant artists: who they were and what they did," *Smithsonian*, 7 (1976): 92-101. Visual evidence of their contribution is contained in the Hirshhorn's exhibition, "The Golden Door: Artist-Immigrants of America, 1876-1976" (May 20-Oct. 20, 1976).

⁴⁶ See "War Cry," a release of the American Artists Professional League. A copy is printed in HUAC Hearings, 906. *Italics mine.*

debated the whole modernist-traditionalist issue with fellow artist and social realist George Biddle. An advocate of socially conscious art and an ardent admirer of FDR, Biddle, like many traditionalists, was not conservative in either an esthetic or political sense. And he would never have equated modernism with communism. But he was adamant in his argument that nonobjective art would prove a short-lived fad. Its appeal, he insisted, was the product of "war neurosis, a dealer-rigged market, and snobbism." In the pages of the *New York Times* critics continued to explore the issue still further. Aline B. Louchheim had previously deplored the willingness of some traditionalists to make abstract moral judgments about work that was non-representational. Her colleague, Howard Devree, focused more precisely on charges of communism, pointing out that socialist realism, the objective style that dominated official art in the USSR, represented not modernism but its very antithesis. Moreover, he noted a dangerous parallel between the practices of totalitarian regimes under Hitler and Stalin and the current attacks on vanguard art as "degenerate" stuff deserving suppression and censorship. Alfred Barr, former director of the Museum of Modern Art, elaborated on this theme in a long article, "Is Modern Art Communist?" pointing out that, on the contrary, abstraction had been damned in Soviet Russia as decadent formalism since the early twenties. He might also have added that modernism had been subsequently anathema to much of the Communist left, which demanded a didactic art the masses could immediately apprehend. But such defenses of modern art had little impact on its more irrational opponents; the *New York Times* continued to receive letters frequently so "violent in their phraseology" that some were unprintable.⁴⁷

Why Dondero, who had no training in art or art criticism, should have become the defender of these unhappy traditionalists is unclear. As a congressman, however, he had proved no friend of change; it was a measure of his legislative career that he would oppose Roosevelt's domestic and foreign policy with a rigor and rhetoric that earned him in 1942, after ten years in the House, a place on *The Nation's* list of "Our Worst Congressmen"—a group distinguished not only by their "bitterness to the New Deal" but by their "essentially undemocratic outlook."⁴⁸ Unscathed by this expression of liberal

⁴⁷ *New York Times*, Sept. 6, 1950; "Boston Goes from 'Modern' to 'Contemporary,'" *College Art Journal*, 7 (1948):230; "Contemporary Documents: American Sculpture 1951," *College Art Journal*, 11 (1952):280-89; Dale Nichols to Harry S. Truman, Dec. 15, 1950, Nichols File, National Collection of Fine Arts (Washington, D.C.); *New York Times*, May 22, 1953; Feb. 26, 1949. For Biddle's thinking on the issue, see his "Modern Art and Muddled Thinking," *Atlantic*, 180 (1947):58-61. Aline B. Louchheim, "'Modern Art': Attack and Defense," *New York Times*, Dec. 26, 1948, sec. 2; Howard Devree, "Modernism Under Fire," *New York Times*, Sept. 11, 1949, sec. 2; Alfred H. Barr, Jr., "Is Modern Art Communist?" *New York Times Magazine* (Dec. 14, 1952), 22-23, 28-30; Howard Devree, "State of the Art World," *New York Times*, Sept. 17, 1950, sec. 2.

⁴⁸ Will Chasan, "Keep Them Out! Our Worst Congressmen," *The Nation*, 155 (1942):438-9. Dondero's connection with Father Charles Coughlin was of interest since both lived in Royal Oak. Chasan pointed out that Dondero had inserted material from *Social Justice* in the *Congressional Record* and had in turn been quoted in Coughlin's paper. More important, however, was the close friendship he developed with J. Edgar Hoover in the mid- to late forties. See Carey McWilliams, "The White House Under Surveillance," *The Nation*, 174 (1952):150-1.

opposition, the Michigan Republican had held fast to his Congressional seat, emerging in the Truman years as an outspoken foe of communism well before he launched his first assault upon abstract artists as agents of the Kremlin. Whatever the element of political opportunism in such attacks, they reflected an intense aversion to nonobjective painting. Although Dondero explained to art critic Emily Genauer that he opposed modern art as "Communist" because it bred "dissatisfaction" by virtue of the fact that it was unintelligible to ordinary Americans whose "beautiful country" it failed to "glorify," he also emphasized its distortion, grotesqueness, and meaninglessness, adding privately, "modern art is a term that is nauseating to me."⁴⁹ This reaction, moreover, was hardly unique if the thousands of letters that eventually poured into his office are an adequate measure of support.⁵⁰

Precisely who Dondero's ideological constituents were is difficult to determine, but they extended far beyond the geographical confines of Michigan's Seventeenth Congressional District and the modest homes of Royal Oak. Dondero himself believed that he had become the spokesman for great numbers of Americans throughout the country who applauded his attacks on modern art as Communist conspiracy. After 1949, few would deny that he had become the single most important congressional "authority" on communism in art.⁵¹ Moreover, he played that role with the full cooperation of those politically conservative traditionalists who secretly plied him with damaging information about fellow artists,⁵² reinforcing in the process the popular identification of modernism with the left.⁵³

The potential strength of this alliance of esthetically and politically con-

⁴⁹ Emily Genauer, "Still Life with Red Herring," *Harper's Magazine*, 199 (1949):89; Dondero to Charles E. Plant, June 29, Nov. 19, 1956; Dondero to Elizabeth Staples, Feb. 27, 1959, Dondero Papers.

⁵⁰ Dondero asserted that he had communications from roughly 2,000 individuals and groups concerning his attack on modern art. See Dondero to Arthur B. McQueen, Jan. 7, 1957. The Washington *Evening Star* (Feb. 16, 1957) was less precise, referring simply to thousands of letters.

⁵¹ His role was accorded ample publicity upon his retirement from Congress when he was honored by such organizations as the International Fine Arts Council. In a ceremony Vice-President Nixon had agreed to attend, the gold Medal of Honor was presented to Dondero by Major General U.S. Grant, 3rd. See Helen Bassel to Dondero, Oct. 7, 1965; also "Dick" (Nixon) to "George" (Dondero), telegram, Feb. 8, 1957, Dondero Papers. A full account of the ceremony is contained in the *Cong. Rec.*, 85th Cong., 1st Sess., A1172-3. Dondero was also honored by the American Artists Professional League and the Painters and Sculptors Club of Los Angeles.

⁵² Dorothy Drew, a New York City artist who painted Dondero's portrait when he became chairman of the House Committee on Public Works, provided him with material for his speeches attacking modern art and leftist artists. See Dondero to Dorothy Drew, June 21, Aug. 6, 1957; Jan. 12, 19, 1960; also Wheeler Williams to Dondero, Aug. 27, 1965, Dondero Papers. Dondero took great care to keep this fact from becoming public lest their opponents work their "bitterness and revenge" on her. See Dondero to Drew, Feb. 5, 1964, Dondero Papers.

⁵³ While self-consciously apolitical in the late forties and the fifties, the abstract expressionists had, like most artists in New York in the thirties, been involved in leftist activities of one kind or another. See Harold Rosenberg, *The Anxious Object: Art Today and its Audience* (New York, 1966), 262. Edward Laning recalls a visit to the 8th Street Club, a favorite haunt of the abstract expressionists, where he discovered his former WPA colleagues complaining about representational painting and noted that "Nelson Rockefeller's Museum of Modern Art took up where the John Reed Club left off." See Edward Laning, "The New Deal Mural Projects" in *The New Deal Art Projects: An Anthology of Memoirs*, edited by Francis V. O'Connor (Washington, D.C., 1972), 112. This association of political and esthetic radicalism is explored by Donald D. Egbert, *Socialism and American Art* (Princeton, 1967), and *Social Radicalism and the Arts: Western Europe* (New York, 1970). For the disastrous consequences of such linkage in Germany, see Barbara Miller Lane, *Architecture and Politics in Germany, 1918-1945* (Cambridge, Mass., 1968).

servative artists and anti-Communist crusaders had been demonstrated as early as 1947, when the State Department purchased a collection of modern art for an exhibition called "Advancing American Art," to be circulated in Europe and Latin America. Although little attempt was made to recognize the struggling abstract expressionists of what would later become the New York School, the collection did include works by such established modernists as Marsden Hartley and Stuart Davis, along with other well-known artists of the period. Recognizing that these seventy-nine paintings had been accumulated within the limitations imposed by an exceedingly modest budget—only \$49,000—most critics agreed with Lloyd Goodrich of the Whitney, who, with few qualifications, called the collection a "remarkably fine one."⁵⁴ But not so the American Artists Professional League, an organization of "modern classicists" who deplored the increasing strength of "revolutionaries" "debauching" all that was "noble in art." In a letter to Secretary of State James F. Byrnes, they complained that this "one-sided collection" was tainted with radical European trends "not indigenous to our soil." Similar charges came from other artist groups such as the National Academy of Design, Allied Artists, the Salmagundi Club, and the Society of Illustrators.

Echoing their objections, the Hearst press was especially vitriolic in its castigation of this latest federal "folly." While the Baltimore *American* complained about the inclusion of works by "left-wing painters who are members of Red Fascist organizations," the New York *Journal American* reserved its animus for the paintings themselves, which were described as "incomprehensible, ugly and absurd," a "lunatic's delight." *Look* magazine joined the fray, as did conservative radio commentator Fulton Lewis, Jr.⁵⁵

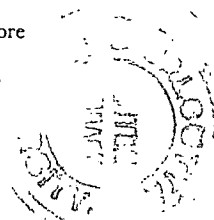
Governmental critics were similarly outraged. Referring to this "sinister" exhibit of modernists, Illinois' Fred Busbey deplored the "weird," "unnatural" depiction of human forms. Georgia's Edward Cox (Democrat) agreed that no sane man could have painted such "crazy" pictures, while Mississippi's John Rankin (Democrat) suggested that they were "Communist caricatures . . . sent out to mislead the rest of the world as to what America is like."⁵⁶ The president agreed, at least on the esthetic merits of the exhibit; of a Yasuo Kuniyoshi painting of a circus scene, Truman remarked that "if that's art, I'm a Hottentot."⁵⁷

⁵⁴ A complete listing of the paintings is contained in the *Cong. Rec.*, 80th Cong., 1st Sess. (1947), 5225. For critical opinions, see John D. Morse to William Benton, Oct. 10, 1946, Hudson Walker Papers; Alfred M. Frankfurter, "American Art Abroad: The State Department's Collection," *Art News*, 45 (1946):21-30ff.; and Goodrich interview. Edward Alden Jewell, of the New York *Times*, predictably dissented. His view of the exhibit is of interest because of the tendency of conservatives to link modern art to the political left. See "Eyes to the Left," New York *Times*, Oct. 6, 1946, sec. 2.

⁵⁵ "League Protests to the Department of State," *Art Digest*, 21 (1946):32-33; Ralph M. Pearson, "Hearst, the A.A.P.L. and *Life*," *Art Digest*, 21 (1946):25; Peyton Boswell, "Humor on the Right," *Art Digest*, 21 (1946):3; "Government Tries Again," *Art Digest*, 22 (1947):32-33.

⁵⁶ Hearings before the Subcommittee of the House Committee on Appropriations, *Department of State Appropriation Bill for 1948*, 80th Cong., 1st Sess. (Washington, 1947), 412 ff., and *Cong. Rec.*, 80th Cong., 1st Sess. (1947), 5187-5292.

⁵⁷ As Robb observed, the Kuniyoshi *Circus Rider* that drew Truman's fire was not one of the more abstract paintings in the collection. See "Art News from Chicago," 39.



Not surprisingly, the State Department, riddled with charges of subversion and under severe scrutiny from congressional appropriation committees, capitulated, cancelling the exhibition, despite the protests of New York artists, museum officials, dealers, and art journal editors, who deplored the incident as a serious threat to artistic freedom.⁵⁸ With the future of the entire Office of International Information and Culture Affairs in doubt, Secretary of State George C. Marshall announced that there would be "no more taxpayers' money for modern art."⁵⁹ The USIA eventually followed suit. Arguing that the agency was interested in art exhibitions only in so far as they provided "a means of interpreting American culture to other peoples," its spokesman insisted that the government "should not sponsor examples of our creative energy which are nonrepresentational." Emphatically, he added: "We are not interested in purely experimental art."⁶⁰ The perception of *avant-garde* art as un-American had now been incorporated into official policy.

Although government officials subsequently modified their stand, hostility to modern art had by no means entirely dissipated when the USIA began plans to send abroad paintings and statuary for the American National Exhibition that Vice-President Richard M. Nixon was to open in Moscow in the summer of 1959. With talk now of detente with Russia, the agency relaxed sufficiently to include in the four-man jury charged with assembling the exhibit Lloyd Goodrich, Director of the Whitney and a persistent proponent of artistic freedom. Charged with the task of selecting works that would reflect the vitality and variety of American art over the previous forty years, Goodrich and his colleagues naturally included examples of the early modernists and those of the New York School along with social realists, American scene painters, and a variety of others.⁶¹ Anticipating objections, they sought to outmaneuver potential critics by withholding any announcement of the works selected until they were packed for shipping, thus presenting opponents with a kind of *fait accompli*.⁶²

⁵⁸ New York Times, May 6, 1947; also editorial comment in *Art News*, 46 (1947):13 and *Art Digest*, 22 (1947):7, as well as the May 1947 issue of the Artists Equity Newsletter. The collection, after being summarily recalled from abroad, was consigned for disposal as war surplus; Lloyd Goodrich arranged for a showing at the Whitney, however, so that prospective buyers from public institutions might see the paintings before purchasing them. Thus they happily escaped the fate of those WPA oils that were sold by the government for the price of the canvas. See Aline B. Louchheim, "The Government and Our Art Abroad," New York Times, May 23, 1948, sec. 2. Even this action was criticized by Representative Busbey. See *Cong. Rec.*, 80th Cong., 2nd Sess. (1948), A4115-6.

⁵⁹ New York Times, May 6, 1947.

⁶⁰ A. H. Berding's statement of USIA policy was enunciated at a meeting of the AFA in October 1953. Although not stated in his original speech, a second form of censorship was made public in an article in the Washington Post and Times Herald on March 6, 1955, quoting agency policy on the political associations of artists. The USIA refused to exhibit "works of avowed Communists, persons convicted of crimes involving a threat to the security of the United States, or persons who publicly refuse to answer questions of congressional committees regarding connection with the Communist Movement." As Alfred Barr ("Letters to the Editor") pointed out, this 1955 statement actually represented a relaxation of censorship as previously practiced by federal agencies that had banned or tried to ban the work of artists obviously less "subversive" than those who were "avowed Communists" or who had taken the Fifth Amendment. See "Artistic Freedom," *College Art Journal*, 15 (1956):184-89.

⁶¹ For the complete listing of works initially included in the exhibit, the rationale behind their selection, and Senator Philip Hart's defense of the exhibit, see *Cong. Rec.*, 86th Cong., 1st Sess. (1959), 9812-14.

⁶² Goodrich interview.

Four days after the list was released the sniping began.⁶³ Naturally, Jackson Pollock's *Cathedral* was critized, as were other "meaningless abstractions."⁶⁴ Wheeler Williams, appearing before the House Un-American Activities Committee, which was investigating several of the artists involved, called the painting a "childish doodle." Fortunately, President Eisenhower declined to act as censor even though he was far more unhappy with Jack Levine's unflattering depiction of a returning general in *Welcome Home*. When asked about the exhibit at a news conference, the president admitted that his favorite was an Andrew Wyeth painting. He went on to say that perhaps future selection committees might include "one or two people that, like most of us here . . . are not too certain exactly what art is, . . . but know what we like and what America likes." "What America likes," he insisted, "is after all some of the things that might be shown."⁶⁵ Although it was subsequently decided to send additional traditional works to Moscow, Jack Levine's *Welcome Home*, with its social commentary, and Jackson Pollock's *Cathedral* remained intact. For champions of artistic freedom, Eisenhower's decision, combined with the extensive criticism they were able to marshal against the investigation of the House Un-American Activities Committee, represented a major breakthrough.⁶⁶

Although there would be future cancellations, the old relish for congressional intervention had clearly begun to wane. The USIA, fearful that more subtle pressure would be exerted through budget cuts, remained exceedingly wary through the early sixties, preferring exhibits that produced little publicity and no controversy. Gathering courage, the agency finally ventured into those international competitions, the biennials, but the news that an American artist had won the prize at the São Paulo Bienal in 1963 and again the following year at the Venice Biennale gave USIA officials an acute case of the jitters. The publicity was considerable, and with the top prize going in 1964 to Robert Rauschenberg for one of his painterly photographic collages, the agency was sure that budgetary repercussions would be forthcoming. Although their predictions were not fulfilled, the fears persisted until 1965, when the International Exhibits were moved out of the USIA and into the Smithsonian.⁶⁷

⁶³ Representative Francis E. Walter's attack on the exhibit and his charges that thirty-four of the sixty-seven artists represented had affiliations with Communist causes are contained in the *Cong. Rec.*, 86th Cong., 1st Sess. (1959), 9746-48. For testimony before Walter and his House Un-American Activities Committee, see HUAC Hearings, 899-963.

⁶⁴ For the discussion of the Pollock painting, see HUAC Hearings, 915.

⁶⁵ A complete transcript of Eisenhower's press conference is contained in the *New York Times*, July 2, 1959. Jacob Javits made a far more eloquent speech on behalf of artistic freedom in the Senate where he argued that whether or not one like modern art it was closely identified with the American scene and should not be removed from the exhibit. "To send . . . officially dictated art," he insisted, "will only be to parrot the performance of their own government." *Cong. Rec.*, 86th Cong., 1st Sess. (1959), 12548-9. Levine, incidentally, specifically excluded Eisenhower and Omar Bradley from attack, professing great respect for both generals, but not for the military establishment generally.

⁶⁶ Interview with Lois Bingham, Feb. 17, 1969. Bingham, Chief of the Office for Exhibitions Abroad at the National Collection of Fine Arts, worked in that area during the fifties when International Exhibits were under USIA auspices.

⁶⁷ *Ibid.*

By that time it was apparent that Dondero and his traditionalist allies had been fighting a rear-guard action in their battle against modernism. The esthetic leadership of these artists long since eroded, they ultimately lost political influence as well. Even their temporary victories proved hollow ones, for, in the final analysis, they had been outmaneuvered by more sophisticated individuals eager to capitalize on the fact that *avant-garde* art and culture exist only in a society that is liberal-democratic (politically) and bourgeois-capitalist (socioeconomically).⁶⁸ Thwarted by repeated interference in USIA and State Department exhibits throughout the fifties, more enlightened anti-Communists, among them Nelson Rockefeller, had naturally turned to "mother's museum" and its newly expanded international program.⁶⁹ Accordingly, the Museum of Modern Art arranged for U.S. participation in a variety of international exhibits, putting together in the process a major show of abstract expressionist art for circulation in Europe in 1958–59. Entitled "New American Painting," the exhibition included a catalog with a comprehensive introduction by Alfred Barr, former Director of the Museum of Modern Art and a long-time advocate of the abstract expressionists. In an essay that subtly reflects the manner in which this self-consciously apolitical vanguard was becoming identified with the cause of human freedom, their work used as a form of "benevolent propaganda for foreign intelligentsia,"⁷⁰ Barr wrote: "Indeed one often hears Existentialist echoes in their words, but their 'anxiety,' their commitment, their 'dreadful freedom' concern their work primarily. They defiantly reject the conventional values of the society which surrounds them, but they are not politically engagés even though their paintings have been praised and condemned as symbolic demonstrations of freedom in a world in which freedom connotes a political attitude."⁷¹

For Dondero and his allies there was irony indeed in that, as New York replaced Paris as the capital of the art world, the painting that earned for this nation unparalleled pre-eminence and prestige was the art of the abstract expressionists. And there was greater irony still in that, through the transmogrifying context of the Cold War, this new generation of New York painters ultimately came to be regarded as the embodiment of the kind of freedom denied their colleagues behind the iron curtain, their works celebrated as quintessentially American.⁷² So rapid and complete was this identification that by the mid-sixties modern art itself had somehow become inextricably linked with the United States as if only in America could the *avant-garde* "spirit" truly flourish. Governmental agencies could at last join private museums and dealers in sending abroad modernist evidence of our creative coming of age—and they did so with little fear of interference.

⁶⁸ Renato Poggioli has thoughtfully explored the political conditions under which *avant-garde* culture can flourish in *The Theory of the Avant-Garde* (Cambridge, Mass., 1968), 95–109.

⁶⁹ For the role of the Rockefellers and the Museum of Modern Art, see Eva Cockcroft, "Abstract Expressionism, Weapon of the Cold War," *Artforum*, 12 (1974):39–41.

⁷⁰ The phrase was used by Max Kozloff in his suggestive "American Painting During the Cold War," *Artforum*, 11 (1973):43–54.

⁷¹ Quoted in Cockcroft, "Abstract Expressionism," 41.

⁷² Kozloff, "American Painting During the Cold War."

That such fears should have persisted as long as they did among intelligent public servants, however, is evidence of the zeal and commitment of people who were determined to keep American art recognizably "American." If they could not control the power of a militant Russia, then they would at least impose an ideological and esthetic conformity associated with their standard of Americanism on art and artists receiving any form of public patronage. A coalition of esthetically and politically conservative legislators and artists, veterans and patriotic groups, they were always a minority. But the events of the McCarthy era demonstrated with frightening force how much political leverage can be gained from the animosities and passions of a small, but articulate and effective minority. Deeply disturbed by the international domestic tensions that beset Cold War America, they sought security from without by searching for the enemy within. Scrutinizing the whole apparatus of government, education, religion, and the communications media, they focused on the "front" affiliations of many of this nation's leading artists. What they saw was evidence not of youthful dedication to Marxist ideology or even a persisting affiliation with the Communist Party, but of contemptible collusion with a conspiratorial power that had reduced half of the world to chains. And when they examined the paintings themselves, they found in the works of socially conscious artists a commentary on society and politics that seemed to suggest not so much a personal protest against injustice as visual evidence of disloyalty. As individuals who sought security in a unified nation, they were unable to tolerate the evidence of social cleavage such paintings reflected. A Marxist concept—class conflict—seemed not only patently un-American but personally threatening.

Convinced that national unity and domestic security were jeopardized by art with "subversive" social commentary and artists with "front" associations, such people possessed little tolerance for new esthetic forms and values. The rejection of modern art and the argument that it was a weapon in the Communist arsenal, however strained the logic, must be carefully scrutinized for what it reveals about the psychological character of antiradicalism as well as the symbolic significance of public patronage.

The identification of modern art with foreign influence, like the identification of political and cultural radicalism, is of long standing. When exhibitors displayed hundreds of postimpressionists, fauve, and cubist works before scandalized Americans at the Armory Show in 1913, some leading art critics had been quick to detect a foreign conspiracy in the invasion of "Ellis Island art," as one commentator termed the new styles.⁷³ Ridiculed by culturally sanctioned artists and critics, a small *avant-garde* community struggled in subsequent years to create what it conceived to be an indigeneous American art that would reflect contemporary life, but such efforts met with scant

⁷³ Barbara Rose, *American Painting Since 1900: A Critical History* (New York, 1967), 13. The persistent charge that modern art was "foreign" is evident in the response to the awarding of the first prize of the Chicago Art Institute to abstract expressionist William Baziotes in 1947. Wrote C. J. Bulliet of the *Daily News*: "These 'isms' grown stale and sterile in the land of their origin, are further enfeebled crossing the Atlantic." Bulliet's comment is quoted in Dore Ashton, *The New York School: A Cultural Reckoning* (New York, 1973), 147.

acceptance from the larger society. It was a measure of public intransigence to the new art that in the 1920s, Max Weber, one of the greatest American cubist painters, was so impoverished that he lacked money for paint and canvas.⁷⁴ A majority of Americans, literal-minded in their taste, continued to assume that American art must be representational.⁷⁵ Never had this identification been more firmly established than in the thirties when many artists, whether rurally oriented regionalists or the more radical urban social realists, sought to detach themselves from "foreign influence" and immerse themselves in the "spirit of the land," capturing the texture of reality in a movement appropriately known as the "American Scene."⁷⁶ Like their counterparts in fiction, theater, and dance, they sought in a new genre of actuality to record and clarify the American experience in explicit, commonly recognizable images that would make possible immediate identification between picture and audience, artist and public.⁷⁷ This effort to capture reality by making art a vehicle for realistically recording places, events, and things, rather than exploring the vast inner landscape of the psyche, coincided, moreover, with a passionate desire of esthetic nationalists for an authentically American art that would rival the great national schools of Europe. It was a significant coincidence and one that would further strengthen the belief that American art was representational art—recognizably American in content, non-European (non-abstract) in style, and democratic in accessibility.⁷⁸ Such had been the case in the thirties and forties, and such would be the case in the fifties.

Traditionalists had a stake in seeing that this formula persisted. The hostility to modernism on the part of many traditionalist artists and sculptors had its origins in many sources, but most noticeably economic rivalry and status anxiety. Academic sculptors in organizations such as the National Sculpture Society had long enjoyed a lucrative monopoly on official art. As suppliers of statuary to public buildings and war memorials, they were responsible for those huge Victorias, Columbias, Giants of Trade and Industry, to say nothing of the muscle-bound horses, that abound in state capitals as well as Washington. Conceived in the classic pattern, it was precisely what was calculated to win approval from Washington's Fine Arts Commission.⁷⁹

Though an attachment to recognizable subject matter persisted through the forties and fifties, abstract expressionist or "action" paintings were exhibited in Peggy Guggenheim's gallery, *The Art of This Century*, as early

⁷⁴ Rudi Blesh, *Modern Art U.S.A.: Men, Rebellion, Conquest, 1900-1956* (New York, 1956), 85-86.

⁷⁵ Lionel Trilling noted a related tendency in American literature wherein liberal intellectuals have followed the Parrington tradition of associating "reality" with democracy. See his "Reality in America" in *The Liberal Imagination: Essays on Literature and Society* (New York, 1957), 1-19.

⁷⁶ Matthew Baigell, *The American Scene: American Paintings of the 1930's* (New York, 1974), 18.

⁷⁷ *Ibid.* See also William Stott, *Documentary Expression and Thirties America* (New York, 1973).

⁷⁸ See my "Art and the People: The New Deal Quest for a Cultural Democracy," *Journal of American History*, 62 (1975): 334 ff; and Baigell, *The American Scene*, 13, *passim*, 45. That so much of the New Deal art was "American Scene" in effect gave it an official imprimatur as Baigell has rightly observed.

⁷⁹ Charlotte Devree, "Is This Statuary Worth More Than a Million of Your Money?" *Art News*, 54 (1955): 34-37ff.

as 1943. By 1945, when Jackson Pollock held his second one-man show, the Wyoming-born artist again received enthusiastic endorsement from both James Johnson Sweeney and Clement Greenberg, the latter critic praising him as the "strongest painter of this generation, perhaps the greatest one to appear since Miró." By the mid- to late-forties, abstract expressionists were lauded not only in *Partisan Review*, *The Nation*, *Magazine of Art*, and *Art News*, but also in popular magazines such as *Life*. Critical acclaim was naturally followed by museum purchases. The Museum of Modern Art, which bought Pollock's *She Wolf* as early as 1944, picked up paintings by Arshile Gorky and Robert Motherwell two years later. In 1947, a William Baziotes canvas received the major prize at the show of the Chicago Art Institute following its exhibit at the Venice Biennale. Moreover, as the fifties progressed, abstract expressionist painting was bought, shown, lectured on, and talked about by an articulate and vocal generation of artists and critics in more and more museums, colleges, art centers, and magazines. Thus, behind the aggrieved cry of dispossessed traditionalists in the conservative art organizations was the impending reality of vanguard triumph. Understandably threatened economically and stylistically, they made common cause with other Americans to whom modernism in any guise was perceived as threatening.⁸⁰

Although economic rivalry and partisan politics underlay much of the hostility to modern art, it would be a mistake to underestimate the intense hatred of abstraction that permeated the attacks of congressmen such as Dondero and the letter writers who provided them unsolicited support. Clearly, these people were threatened at a very basic level. And the reason why lies not only in their world view but also in the intrinsic characteristics of modern art and most especially abstract expressionism.

The abstract expressionists of the New York School were genuine esthetic revolutionaries—but revolutionaries who were steeped in the modern tradition. Having mastered the esthetic basis of modernism with the help of Hofmann, Albers, Mondrian, and the many surrealists who fled Europe during the war, they rejected existing realistic and geometric tendencies while borrowing the biomorphic shapes, mythic symbols, and automatist technique of the surrealists. The result was not merely an eclectic synthesis of European sources but a highly original style that quite literally extended the frontiers of art. Although landscape and figure elements can be discovered in many works, readily identifiable subject matter was rejected. Turning instead to private visions, insights, and most especially the subconscious, the abstract expressionists plumbed the depths of their own experience for metaphors and symbols that would somehow possess universal meaning. Content thus consisted of ambiguous forms with loosely defined, perhaps even subliminal, multivalent associations arrived at not through prior determination but in the

⁸⁰ Irving Sandler, *The Triumph of American Painting: A History of Abstract Expressionism* (New York, 1970), 79, 211–12; Rosenberg, *Anxious Object*, 77. It is significant also that Pollock, de Kooning, and Gorky were included in the American exhibit at the 1950 Venice Biennale.

actual process of painting. Gone were the pleasures of easy recognition and the enjoyment of technical dexterity in the imitation of material form and surface so evident in the painting of Andrew Wyeth. Appreciation of abstract expressionism, critic Harold Rosenberg noted, now required a consciousness of history since the art historical reference, especially in "over-all" abstraction, was often the only content. "The old pleasure of seeing 'life' inside the frame must be augmented, if not replaced," Rosenberg noted, "by the stimulation of recognizing an inspired side glance to, say 'Les Demoiselles d'Avignon.'"⁸¹

Thus for the untutored viewer, the art of the abstract expressionists was not only subjectless in the traditional sense, but it seemed conspicuously foreign in terms of historical antecedents. And since public consciousness had no part in the work, it was perforce elitist.⁸² While this characteristic was widely accepted by abstract expressionists self-consciously alienated from the middle class, it was anathema to frustrated viewers whose very bafflement reminded them that esthetically they had not yet arrived after all—and, indeed, might never make it. Fed the predigested pap of mass culture, they were simply unprepared for the sheer effort required in the process of visual analysis.⁸³ Opposition, as André Malraux had predicted of any mass public confronting *avant-garde* art, derived not only from ignorance but from an obscure sense of "betrayal."⁸⁴ Thus rejected and rejecting, many people were no doubt further enraged by the implications of it all: the notion of a hierarchy of taste—the possibility of cultural classes in a democratic state.⁸⁵ And there were still other barriers. The art of the abstract expressionists was also perceived as functionless when function was a quality that Americans had traditionally demanded in the visual arts. Indeed their very legitimization in this country had been accomplished through the attribution of didactic qualities. And perhaps worse still, these canvases with their static masses of color or tangled surfaces of dripped paint provided no indication of the artist's talent or index to his actual labor. Unlike an Andrew Wyeth painting with its photographic rendering of each blade of grass, a Mark Rothko or a Jackson Pollock communicated no evidence of hard work. And as one art critic has noted, respect for hard work amounts to an "esthetic prejudice" in America.⁸⁶

The elitist character of abstract expressionism, its nonrepresentational

⁸¹ Rose, *American Painting Since 1900*, 61, 66, 70–73, 89; Sandler, *Triumph of American Painting*, 1, 3, 29, 62, 92–93; Rosenberg, *Anxious Object*, 77.

⁸² Gottlieb put the matter succinctly: "The modern artist does not paint in relation to public needs or social needs—he paints only in relation to his own needs." See Adolph Gottlieb, "The Artist and Society," *College Art Journal*, 14 (1955):99.

⁸³ The extent to which mass culture has rendered the general public less able to deal with high culture, especially modern art, is explored in a variety of essays in *Mass Culture: The Popular Arts in America*, eds. Bernard Rosenberg and David Manning White (Glencoe, 1958). See especially, Irving Howe, "Notes on Mass Culture," 499.

⁸⁴ Quoted in Poggioli, *The Theory of the Avant-Garde*, 180.

⁸⁵ The hostile response of the "middlebrow" to the possibility of cultural classes in a democratic state is suggested by Leslie Fiedler in "The Middle Against Both Ends," in *Mass Culture*, eds. Rosenberg and White, 547.

⁸⁶ Rose, *American Painting Since 1900*, 43.

quality, its perceived lack of technical dexterity of function and even of meaning are thus but a few of the factors that would understandably make the art of the New York School inaccessible to many, if not indeed most, Americans in the late forties and fifties. But the vast majority of individuals who found such art unintelligible or unattractive might dismiss it simply as "junk"; they would not respond by calling it decadent, degenerate stuff that was communist inspired. To understand why some Americans would respond in this fashion and do so with an intensity and passion that on occasion rendered their attacks unprintable requires further examination of the art itself and its psychological impact.

These were people, it must be remembered, who were already fearful that traditional American virtues had been undermined by cosmopolitans and intellectuals, the old competitive capitalism gradually eroded by Socialist and Communist schemes, and national security compromised by treasonous plots. To compound matters, they were confronted in abstract expressionism by a form of vanguard art characterized by two dimensionality, fluid space, lack of closed shapes, a deliberately unfinished quality, and an "overall" composition that diffused any notion of focus. Complex, cosmopolitan, and ever-changing, it was intrinsically at odds with the need for certitude and control. Control, moreover, was precisely what was threatened by the strong Freudian elements in abstract expressionism. *Avant-garde* American painters, as has been previously noted, borrowed heavily from the surrealists, emphasizing the unconscious as a source of imagery and automatism as a technique whereby painting became spontaneous action—a gesture of "liberation from value—political, esthetic, moral."⁸⁷ Reality in art now embraced not only the outer, objective world of nature and human activity, but also the inner, subjective, psychological world of self. Sophisticated critics such as Ben Shahn could recognize that this inner reality offered a "rich, almost limitless panorama"—an unparalleled source of images and symbols—while simultaneously deploring the apparent abdication of intention on the part of the artist.⁸⁸

For some of the less sophisticated, the free play of the unconscious could mean only irrationality in its most pejorative sense—the beast within. In a pamphlet significantly entitled *The Animal Stalks*, a realist artist argued that "the insane and sensual animal aspects of 'Modern' art must be as surely repressed as are obscene photographs and narcotics . . . and an art that 'feeds' the mind encouraged [lest] the Western World . . . perish."⁸⁹ Although the author would doubtless have resented the comparison, his were sentiments worthy of that notorious Stalinist censor and suppressor of creative thought,

⁸⁷ Harold Rosenberg, *The Tradition of the New* (New York, 1959), 30.

⁸⁸ Ben Shahn, *The Shape of Content* (Cambridge, Mass., 1957), 43ff. In these Charles Eliot Norton lectures at Harvard, Shahn provides a lucid and perceptive critique of abstract expressionism that belies the notion that such opposition issues only from the political right. His distress at the abdication of intention is shared by Jacques Barzun, who suggests that the progressive elimination of purpose culminating finally in aleatory art has made it necessary to refer to older genres as "intentional art." See Barzun, *The Use and Abuse of Art* (Princeton, 1974), 55.

⁸⁹ Dale Nichols, *The Animal Stalks* (n.p., 1953), 8. The pamphlet is available in the Nichols File, National Collection of Fine Arts.

Andrei Zhdanov, whose own efforts to stifle modernism in the USSR reflect what one scholar has described as an almost pathological tendency toward repression of the sexual element in art.⁹⁰ Like their Soviet counterparts, these American opponents of modernism never underestimated the power of the artist to coerce the senses. Thus, when they lambasted abstract expressionism, they did so out of a compelling, if little understood, need to maintain a rigid view of reality characteristic of what might be called the "cultural fundamentalist"⁹¹—a view wherein the irrational was repressed, causation simplified, change controlled, heterogeneity denied, loyalty affirmed, national unity and personal esteem preserved. Precisely how modernism in art, as in other aspects of life, threatened that reality was never fully and satisfactorily delineated by its opponents, but they correctly perceived that it did. The problem, then, was not just one of ignorance but of innate antipathy or, as Renato Poggioli would say, not of "exegesis but of psychology" for, in the final analysis, the perception of art is essentially a psychophysiological process.⁹²

The intensity of opposition becomes more understandable if it is recognized that public patronage, like Prohibition, was a cultural issue fraught with symbolic significance.⁹³ The fact that the USIA and State Department were sending abroad modern art under official auspices raised the question whether the power and the prestige of the government had been placed now on the side of the modernists. Inasmuch as abstract expressionism symbolized a whole set of cultural values associated with modernism, such patronage served to legitimate those values—to affirm their validity while simultaneously discrediting those associated with cultural fundamentalism. Thus in the contest for social dominance, many traditionalists no longer enjoyed the symbolic satisfaction of being exclusively identified with publicly affirmed values and norms. Confronted with this evidence of declining hegemony and control, some responded with the self-righteous passion of the dispossessed.

That they also responded with charges of communism is hardly surprising

⁹⁰ Maynard Solomon, ed., *Marxism and Art* (New York, 1973), 238.

⁹¹ The term "cultural fundamentalist" has reference to "culture" in the anthropological sense rather than as a synonym for the fine arts and to "fundamentalist" in a nonreligious as well as a religious context. The term is not meant to be perjorative. My conception of the term has been influenced by a number of studies, among them: Theodore W. Adorno et al., *The Authoritarian Personality* (New York, 1950); Richard Christie and Marie Jahoda, eds., *Studies in the Scope and Method of the Authoritarian Personality* (Glencoe, Ill., 1954); M. Brewster Smith, Jerome S. Bruner, and Robert W. White, *Opinions and Personality* (New York, 1956); Neil J. Smelser, *The Theory of Collective Behavior* (New York, 1962); and Eric Hoffer's idiosyncratic but provocative *The True Believer* (New York, 1958).

⁹² Poggioli, *The Theory of the Avant-Garde*, 154. Arnold Hauser has noted in *The Philosophy of Art* that the mass public reacts "not to what is artistically good or bad but to features that have a reassuring or disturbing effect upon their course of life; they are ready to accept what is artistically valuable provided that it supplies vital value for them by portraying their wishes, their fantasies, their day-dreams, provided that it calms their anxieties and increases their sense of security." Quoted in Berel Lang and Forest Williams, eds., *Marxism and Art: Writings in Aesthetics and Criticism* (New York, 1972), 274. For an introduction to the psychology of visual perception, see the writings of Rudolf Arnheim, especially *Visual Thinking* (Berkeley, 1969). There is extensive literature documenting the ways in which perceptual behavior is affected by personality. See, for example, Jerome S. Bruner and Cecile C. Goodman, "Value and Need as Organizing Factors in Perception," *Journal of Abnormal Psychology*, 42 (1947):33-44.

⁹³ The analogy between Prohibition and public patronage was suggested by the reading of Joseph Gusfield's *Symbolic Crusade: Status Politics and the American Temperance Movement* (Urbana, Ill., 1963).



Jack Levine, "Welcome Home" (1946). Courtesy of The Brooklyn Museum, J. B. Woodward Memorial Fund.

when we recognize that communism itself had become for many a symbolic issue that had less to do with a foreign ideology or even the realities of international politics than with the forces of change. For those who responded to the rhetoric of a Joseph R. McCarthy or George A. Dondero, anti-Communism became a call not only for political conservatism but for competitive capitalism, a nativistic nationalism, and religious orthodoxy. Anti-statist, antibureaucratic, antiforeign, and anti-intellectual, it was also anti-modernist. And in its gallery of "enemies," the Museum of Modern Art was as suspect as the National Council of Churches. Indeed, so dominant is this preservatist character that our understanding of an era may well be enhanced if we view the anti-Communist crusade of the fifties and early sixties not just as a political response to legitimate and highly disturbing Cold War problems but also as a revitalization movement designed to eliminate foreign influences and revive traditional values and beliefs in a period of societal stress.⁹⁴ In the process of revitalization, countersubversives focused, as they did in the wake of World War I, on the familiar readily recognizable symbols of Americanism, emphasizing always the concrete, the definite, the unambiguous. And these, of course, were the very qualities that were antithetical to the modern art of the abstract expressionists, with its ambiguous forms, fluid space, open shapes, and multivalent associations. Thus it is hardly surprising that modern art became emblematic of the forces threatening the psychic equilibrium of the anti-Communist right.

⁹⁴ The most effective treatment of an anti-Communist crusade in terms of a revitalization movement is Stanley Coben's "A Study in Nativism: The American Red Scare of 1919-1920," *Political Science Quarterly*, 79 (1964):52-57. Gregory Guroff of Grinnell College has called my attention to a comparable response in the Soviet Union where real international responsibilities acquired in the years following World War II seemed to engender a similar rejection of cosmopolitan culture and a furiously nationalistic campaign complete with renewed denunciations of nonrepresentational art, atonal music, and nonsocialist realist literature.

Consensus or Conflict: The Dilemma of Islamic Historians

A Review Article by ANDREW C. HESS

MARSHALL G. S. HODGSON. *The Venture of Islam: Conscience and History in a World Civilization*. Volume 1, *The Classical Age of Islam*; volume 2, *The Expansion of Islam in the Middle Periods*; volume 3, *The Gunpowder Empires and Modern Times*. Chicago: University of Chicago Press. 1974. Pp. xii, 532; vii, 609; vi, 469. \$20.00 each.

MAURICE LOMBARD. *The Golden Age of Islam*. Translated by JOAN SPENCER. (North-Holland Medieval Translations, volume 2.) Amsterdam: North-Holland Publishing Company; distrib. by American Elsevier Publishing Company, New York. 1975. Pp. x, 259. Cloth \$20.95, paper \$14.75.

V. J. PARRY and M. E. YAPP, editors. *War, Technology and Society in the Middle East*. New York: Oxford University Press. 1975. Pp. viii, 448. \$22.50.

JULIUS WELLHAUSEN. *The Religio-Political Factions in Early Islam*. Translated by R. C. OSTLE and S. M. WALZER. Edited by R. C. OSTLE. (North-Holland Medieval Translations, volume 3.) Amsterdam: North-Holland Publishing Company; distrib. by American Elsevier Publishing Company, New York. 1975. Pp. xi, 183. \$9.95.

FRANZ ROSENTHAL. *The Classical Heritage of Islam*. Translated by EMILE MARMORSTEIN and JENNY MARMORSTEIN. (The Islamic World. Published under the auspices of the Near Eastern Center, University of California, Los Angeles.) Berkeley and Los Angeles: University of California Press. 1975. Pp. xx, 298. \$15.00.

NIKKI R. KEDDIE. *Sayyid Jamāl ad-Dīn "al-Afghānī": A Political Biography*. (Published under the auspices of the Near Eastern Center, University of California, Los Angeles.) Berkeley and Los Angeles: University of California Press. 1972. Pp. xvii, 479. \$20.00.

LENN EVAN GOODMAN, translated with introduction and notes by. *Ibn Tufayl's Hayy Ibn Yaqzān*. (Library of Classical Arabic Literature, volume 1.) New York: Twayne Publishers. 1972. Pp. ix, 246.

PHILIP K. HITT. *Capital Cities of Arab Islam*. Minneapolis: University of Minnesota Press. 1973. Pp. v, 176. \$7.95.

TO PROCLAIM HERE THE EXISTENCE of a crisis in the field of Islamic history might not even carry the reader's interest beyond this topic sentence. This review profited greatly from criticism it received in a seminar on modernization at Temple University, March 26, 1976.

when the historical profession faces more fundamental difficulties than the changing demands of what is sometimes described as an esoteric field. Nonetheless, the writing of Islamic history in the West is at some kind of crossroads; and the news of this event ought to be broadcast if the effort to understand the past on a level that goes beyond the old division of the globe into autonomous cultural areas is to be preserved.

Before proceeding to an analysis, based on some recent publications, of the modern dilemmas of Islamic historians, an explanation ought to be developed for the role the study of this unfamiliar civilization plays in the West. One of the main justifications for expending so much energy on the exotic languages and cultures of the Middle East was that Islamic history represented such a huge chunk of human experience that it was worth-while remembering. It developed, however, that other factors were at work leading Western scholars to the subject of Islamic history. The examination of another's culture turned out to be an effective means of confirming the integrity of one's own tradition; and, in the service of various forms of imperialism, knowledge of Islamic ways was necessary if Muslims were to be convinced of the superiority of Western institutions. Whatever the motivations, the study of Islamic history soon became a dialogue between Western scholars and the culture-bearing elite of the other tradition in a manner that greatly assisted the process of understanding.

Recently, however, the lines of communication between Western and Middle Eastern historians of Islamic civilization have become fouled by the revolutionary processes of modern life. New communities of scholars—nationalist, reformist, Marxist—emerged in the Middle East to ask questions about the past that had either been avoided by former historians or simply not examined. Many of these same Middle Eastern writers further compounded the problem of communication by adopting a hostile and decidedly anti-Western tone in their work. Meanwhile, the response of Western historians to the first signs of a Middle Eastern attempt to modernize Islamic history was to carry on as before, but with a good deal of uneasiness. Some applied the discoveries of modern social science to the field. Yet the Western base upon which most of the theory rested only added to a vague sense of intellectual confusion among Islamicists as, alas, they found themselves confronted by another problem of translation: what does feudalism, or better, class, mean in a region of the world with a history so fundamentally different from that of the West?

While the reasons for the troubles that beset the writing of modern Islamic history are multifold, one of the basic causes of intellectual difficulties arises from the imperviousness of traditional Islamic history to modern criticism. There are, currently, four schools of historical activity in the Middle East. The Marxists and positivists inhabit the distant intellectual horizon. More central, hence far more popular, especially in Turkey and Iran, is national history. But this body of historical work, like much of the reformist thought in the nineteenth century, has grown up with, rather than supplanted, an older

vision of the past.¹ Most widespread, then, especially in Arab lands, are the traditional histories. Faithful to the themes of classical Islamic scholars, these works view history in terms of a divinely guided Muslim community. Not modern, they preserve the historical framework of a bygone era. Thus, if the current effort to describe the past were to be summed up, it would yield, paradoxically, both extraordinary consistency and diversity.²

To penetrate the psychological armor that the traditional vision of history erects against reality, modern authors of Islamic history must contend with three methodological problems: an aversion to applying internal criticism to the reports of responsible witnesses; the effort to emphasize the universal over the particular; and the constant desire to stress the value of consensus rather than conflict. Why these elements of an inherited paradigm are so important demands an explanation based upon Islamic history.

The first characteristic of the classical vision proceeds from the manner in which Islam was revealed to man. The Koran was God's word, not Mohammed's. Man, therefore, did not question the truthfulness of the revelations. When the Koran proved to be insufficient for the formation of a great civilization, Islamic scholars expanded the content of their culture on the basis of the testimony of believers. By extension, critical techniques were employed not to verify the content of what was reported to be Islamic practice, but to check whether or not the reporter was reliable. Even in the case of conflict between the majoritarian and oppositional versions of Islam, Sunnism and Shiism, rational criticism was used only to weaken the position of the opponent and not to change the doctrine of one's own party. In modern times one striking manifestation of the strength of this Islamic method of handling evidence is the failure of Middle Eastern scholars to apply modern literary criticism to the Koran and have the results accepted.³

A second major theme pervading all of Islamic history is the great value placed upon avoiding conflict based upon internal particularisms. Mainly a product of the struggle against an Arab definition of Islam, the drive to integrate as many groups as possible within the Community finds its internal expression in the universal histories written after the Abbasid revolution of 750. In these encyclopedic works the important division was not between Arabs, Persians, and Turks but between the House of Islam and the House of War, between the cultural region ruled by Islam and the outside world.

A third crucial element of the classical methodology comes from the importance Muslim elites attached to the achievement of consensus. On this point the history of how the Community's cultural leaders discovered the normative religious law, which is at the core of Islam, provides the best example of the consensual thrust of this great culture. One of the pillars of the legal theory

¹ Niyazi Berkes, *The Development of Secularism in Turkey* (Montreal, 1964), charts the course of the ideological ambivalence that this phenomenon creates for Turkey.

² Abdallah Laroui, *La crise des intellectuels arabes* (Paris, 1974), 21-44.

³ Charles D. Smith, "The 'Crisis of Orientation': The Shift of Egyptian Intellectuals to Islamic Subjects in the 1930's," *International Journal of Middle Eastern Studies* (hereafter *IJMES*), 4 (1973): 393-99.

upon which the elaboration of the Holy Law rested "was the doctrine of consensus—the widespread agreement of the religious elite on whether a legal matter was Islamic had to be achieved before its result could be incorporated into the religious law."⁴ As the history of Muslim law records, the compulsion on the part of the '*ulamā*', the men of religion, to achieve agreement led not to a constantly changing body of law but to the formation of a closed legal system at the end of the caliphal era. Thereafter men of religion ferociously defended what became a minutely defined ethical system as the cultural reference point for a civilization with a particularly high level of internal social tension.

Centuries of attempting to preserve universalism and to prevent conflict have left an intellectual heritage in the Middle East. Modern authors show a persistent tendency to mix religion with nationalism and to ignore, or to reject, the element of conflict involved in the transformation of Islamic civilization into modern societies.⁵

No less a factor in explaining the power of traditional Islamic history is the unique political role played by the culture-bearing elite. How the rapidity of the Arab conquests prevented Muslims from subjecting caliphal government to the full force of the religious movement that sprang out of Arabia in the seventh century is a favorite theme of Islamic historians. At the end of a complicated internal struggle caused by the Arab conqueror's use of pre-Islamic ruling practices, the men of religion accepted a compromise that left them on the fringe of the political order. So long as rulers defended the Holy Law, the social and moral standards of Islamic society, the '*ulamā*' legitimized a remarkably wide range of governments, making political fluidity one of the chief characteristics of Moslem government. Thus generation after generation of '*ulamā*', all armed with an unchangeable religious law, came to represent what was permanently Islamic and not the offspring of ruling families.

As the Afro-Eurasian land mass became increasingly Islamic, the marginal location of the '*ulamā*' within the political system made it possible for them to absorb revolutionary movements, especially those that proceeded from the less-cultured fringe of the Moslem world. All political upstarts who wished to rule on a wide basis eventually courted the '*ulamā*' for the purpose of obtaining their legitimation. Successful politicians found it easy to form the alliance. An association with the '*ulamā*' involved no deep commitment at the beginning to a political program short of defending the Muslim community. All that had to be accepted was the paramountcy of the men of religion in intellectual and social matters. Education was entrusted to them with few qualms because, as guardians of the Holy Law, they taught what was known rather than what might be. In addition, the '*ulamā*' served both government and society as social managers, resolving conflict on the basis of their knowledge of the Holy Law. This role as peacemakers, as guardians of morality, won public respect

⁴ M. Bernand offers a short definition of "consensus," "*Idjma'*," in *Encyclopedia of Islam* (2nd. ed.; Leiden, 1960), 3: 1023-26.

⁵ Sherif Mardin, *Continuity and Change in the Ideas of the Young Turks* (Ankara, 1969), 24-27.

for an intellectual class that tended to avoid ultimate political responsibility unless government clearly moved in an anti-Islamic direction.

When modern life shredded the fabric of Islamic society in the nineteenth century, the Community turned not to the very small number of Westernized elite for guidance but to the '*ulamā*'. Instinctively this Muslim elite responded to what was a crisis of civilization by reasserting their belief that the right path was known. Change in the modern world could not be absorbed, however, within the narrow framework that the men of religion had inherited from Islamic civilization. A small number of Muslim scholars reacted at the turn of the twentieth century to the anxiety produced by the increasing gap between reality and the vision of what ought to be by a call for reform. But the acceleration of change in the mid-twentieth century left the reform movement stillborn.

Those Western scholars who first struggled with the history of Islamic civilization did not stray far from the paths laid out by the classical Muslim historians. In one way or the other the origin of Islamic history in the West goes back to the reverent study of religious documents. To understand the meaning of obscure passages, scholars—orientalists—placed great stress upon the analysis and acquisition of Eastern languages. When orientalists reached Islamic literature, they applied these same techniques to the study of the timeless documents of Islam. Since little was known in the West about this civilization, it appeared that this cautious exploitation of linguistic abilities was indeed the correct approach. Competent scholars did not attempt to explain the sequence of events that made up the history of Islam; rather they used their prowess with exotic languages to translate Islamic civilization into Western languages.

Taken altogether the translations and commentaries of orientalists fortified the boundaries of traditional Islamic history. First, the narrow literary approach of the orientalists confined historical criticism to areas and subjects determined by a very specific kind of source. Second, their line of approach tended to identify the history of Islamic civilization with the cultural product of a narrow element within Muslim society that had an interest in suppressing diversity. Finally, given the ignorance in the West of the brilliance of Islamic civilization, translations of what the believers wrote, even when accompanied by critical introductions, added to the grandeur of Islamic civilization as it was defined by classical historians.

Two recent publications prove that the stream of translations and commentaries so necessary to the vitality of Islamic history has not dried up. Franz Rosenthal's *Classical Heritage in Islam* is an attempt, through the translation from Arabic of selected passages, to establish the link between classical antiquity and Islam. In texts that run the gamut from works on literary criticism to the occult sciences, the author shows how a creative Islamic civilization made a new synthesis in which the Greek heritage was harmonized with other cultural elements. Lenn Evan Goodman, studying the twelfth century, has translated into English a famous philosophical tale: *Ibn*

Tufayl's Hayy Ibn Yaqzān. Born on, or carried by a current to, an isolated island, the hero of this philosophical romance grew up to discover the highest intellectual and religious truths. Far from being either obscure or irrelevant, the story of Hayy Ibn Yaqzān raised that crucial issue for Islamic civilization: the relation between reason and faith. Taken together, both translations add bright new pieces to an Islamic mosaic whose general shape is already known.

Those in the West who ventured forth into the arena of Islamic history found themselves at work within a discipline whose built-in conservatism was strong. Basic language skills were so demanding that scholars who went beyond the sources were often regarded with suspicion. If orientalism inhibited experimentation, academic convention also isolated Islamic history from the more dynamic intellectual currents loose among Western historians. Housed in oriental centers, Islamic historians found their separateness confirmed by the post-World War II intrusion of government into the field of Middle Eastern studies. What the resulting intensification of support for Islamic subjects did was to strengthen the emphasis on language, on the value of inherited wisdom, and on the training of experts who would manage the relations that had grown up between the stable West and the unstable Middle East. Under such conditions the Islamic historian in the West moved closer to the intellectual attitudes of the '*ulamā*' than he did to the critical viewpoints of the modern historian.

The work of two Islamicists, H. A. R. Gibb and Gustav von Grunebaum, bears eloquent witness to the influence exerted by the men of religion even on the Islamic history written by unbelievers. Both these influential scholars adopted a universal or holistic approach to the civilization they studied. Gibb's magisterial summaries of classical Arabic literature, of Islam as a religious system, and of the attempt by the Ottomans to bring political unity to the Muslim Community are well known. His distinct lack of enthusiasm for the period when Islamic civilization became unraveled, the nineteenth century, is less recognized. It is with the work of von Grunebaum, however, that the parallelism between the way these two Western historians view the history of Islam and the manner in which the '*ulamā*' have recorded the Muslim past stands out. He believed thoroughly, as did Gibb, in the unity of Islamic civilization; he grounded his understanding of Islam in the literature of the culture-bearing elite; he avoided political and economic topics; and he disliked subjects dealing with the theme of conflict. Both von Grunebaum and Gibb felt also the need to communicate with the men of religion and even to advise them on how to cope with modern problems.⁶

This attempt to write a modern Islamic history as one grand unit was elevated to a new level of generalization with the publication of the late Marshall G. S. Hodgson's *Venture of Islam*. A major event for a scholarly field that does not produce much activity, the three volumes and 1610 pages of

⁶ See William R. Polk's contribution on Gibb and Amin Banani's contribution on von Grunebaum in "Islam and the West," *IJMES*, 6 (1975): 131-47; and Hamilton A. R. Gibb, "The Heritage of Islam in the Modern World (III)," *IJMES*, 2 (1971): 138-47.

Hodgson's analytical history of Islamic civilization from the pre-Islamic era to modern times make difficult a brief review. Fortunately, this serious American Quaker often stated his fundamental ideas.⁷ A rebel within the framework of Islamic history and an advocate on the plane of world history for the importance of his subject, Hodgson has leveled meaningful criticism against both Islamicists and other historians who have dealt in one form or another with Islamic civilization. On the plane of world history he appeals for the acceptance of a comparative viewpoint that would give Islamic civilization its rightful position of superiority between the mid-tenth and the thirteenth centuries. To prove that this ought to be, Hodgson wrote a complex and detailed history in the manner of a Persian artisan weaving an intricate tapestry of truly grand proportions.

For the Islamicist, the Hodgsonian synthesis contains a heady mixture of original ideas set within familiar parameters. Those who are concerned with his modifications of the details of Islamic history can consult the lengthy book reviews that are sure to occupy great space in many professional journals.⁸ The relationship between his core ideas, however, and the dilemmas created by the process of modernization for Islam and its history are not so complex that they cannot be treated here.

Like the men of religion, Hodgson wished to liberate Islamic history from "national" particularisms. In this case, however, it was the particularism of Western historians he criticized: the Arabists, who not only overemphasized the contribution of the Arabs to Islamic history, but also created a Mediterranean center of gravity for the entire civilization when its cultural center lay nearer the Persian plateau than it did to the Levant.

Thus, the thrust of Hodgson's work runs counter to the emphasis of Philip Hitti's *Capital Cities of Arab Islam*. Essentially a summary of the author's previous research on the history of Mecca, Medina, Damascus, Baghdad, Cairo, and Cordova, this book highlights the Arab contribution to Islamic civilization and makes no attempt to define that mysterious social institution, the Muslim city.

There would be little disagreement between Hodgson and the 'ulamā' on the definition of the essential social group at the center of the Muslim movement. It was the community of religiously faithful, who could not be defined in local, urban, or aristocratic terms, that made Islamic history. But the wide dispersion of a non-local society, whose boundaries lay where Islam was accepted, demanded a strong integrating mechanism. Here Hodgson acknowledged the unifying role the Holy Law of Islam played in preserving the cohesion of the Community. At the same time, however, he failed to find in the inflexible legalism and the closed thinking that marked the orthodox approach to the problem of unity the factor that explained the catholic appeal

⁷ Marshall G. S. Hodgson, "The Role of Islam in World History," *IJMES*, 2 (1970): 99-123.

⁸ For a short and essentially sympathetic review, see Clifford Geertz, "Mysteries of Islam," *New York Review of Books*, 22, no. 20 (1975): 18-26.

of the religion and its civilization in its Middle Period, 1100–1500.⁹ Hodgson suggests instead, in the most stimulating portion of his work, that an increasing tension between the rigid expression of Islam (Sunnism) and the oppositional (Shiism) and the latitudinarian (Sufism) movements within the Community gave Islam its universal attractiveness.

The object, therefore, of Hodgson's hostile attitude toward the legalistic expression of Islam was not to lay the groundwork for escaping the boundaries of the classical paradigm that dominated Islamic history, but to find another explanation for its inner cohesion. Probing deeper into his argument, we find that the primal force that made this great culture the most dynamic of the cosmopolitan civilizations during the Middle Periods of its history was religious. Sufism, a gentle kind of intellectual and yet popular brand of Islam, swept through the Muslim social order after the collapse of the Abbasid caliphate in the eleventh century. Inherently pluralistic, this religious upheaval could have, given the lack of an ecclesiastical bureaucracy in Islamic civilization, dissolved the very core of Islam. Unity was preserved, however, by the appearance of a new religious mediator, al-Ghazzali, who provided a unique spiritual and intellectual compromise for a revitalized Community.

Although not a particularly new view of what happened on the religious plane in the Middle Periods, Hodgson's emphasis on the creative impulse that mysticism provided does move Islamic history away from a narrow concentration on the literary product of an extremely thin orthodox elite toward a broader conception of what constitutes Islam.¹⁰ Once Hodgson has shown the established religious doctrines to be wanting, he brings into focus the flexible and experimental face of Islamic civilization, ranging over a huge geographical arena to explore politics, commerce, architecture, and a host of other topics.

This latest, and perhaps last, synthesis of Islamic history is not phrased in the language of conflict. That Hodgson accepted the basic exterior framework of Islamic history as it had emerged in the West is certainly an internal reason for the universalistic cast of this profound effort at understanding another civilization. There are also other explanations for his sympathetic view of Muslim civilization. The piety of the Sufi surely bears witness to Hodgson's own Quaker beliefs as seen through the medium of Islamic history. His use of comparative and world historical perspectives aimed not to inflate the scale of Islamic civilization, but to humble the culture-bound arrogance of his own colleagues. Finally, his dialogue with Islamic history was yet another effort to preserve the lines of communication between those in the West who were sensitive to the *Venture of Islam* and those who seemed to be entrusted with the cultural heritage of Islamic civilization: the men of religion.¹¹

⁹ Hodgson employs a complex terminology to draw a distinction between the religion of Islam and the civilization in which Islam was the primary but not the exclusive religious phenomenon. Marshall G. S. Hodgson, *The Venture of Islam* (Chicago, 1974), 1: 3–69.

¹⁰ *Ibid.*, 2: 201–92.

¹¹ Marshall G. S. Hodgson, "Modernity and the Islamic Heritage," *Islamic Studies*, 2 (1962): 116–27.

Hodgson knew well that Islamic civilization, as opposed to Islam, came to an end sometime in the eighteenth century.¹² His untimely death, which left volume three unfinished, was tragically prophetic of the difficulties facing the discipline of Islamic history during the world-wide revolution of the modern era.

Historians of the Muslim world have recognized, in one fashion or another, that the nineteenth-century impact of the West on the Middle East disorganized both Turko-Muslim governments and societies. What was unique about this new relationship between Europe and the Middle East was the multifold internal disruption of society: Islamic institutions at all levels—social, economic, ideological, political—were attacked and destroyed or drastically modified. That this deep involvement of the West in the Middle East would eventually pull the rug from under the way Western historians perceived the Muslim past was, quite naturally, not understood or dealt with in an explicit fashion until recently. It was the study of late nineteenth-century revolutionary movements that precipitated the question; for those that were inspired by Western ideas—Turkish, Arab, and Persian nationalisms—led Islamicists to take up subjects that did not treat the Muslim world as an autonomous cultural area.

Islamic history being what it is, biographies of modern Middle Eastern intellectuals have an understandable attraction. Nikki R. Keddie's *Sayyid Jamāl ad-Dīn "al-Afghānī": A Political Biography*, and Hamid Algar's *Mīrẓā Malkum Khān: A Study in the History of Iranian Modernism*, not only are informative studies of important nineteenth-century political figures, but they also give some idea of what happens to a historical tradition caught in the throes of the modern revolution.

Half modern, half embedded in the civilization of Islam, both these men have unsatisfying, difficult histories. The first was part of the intellectual movement that introduced the vocabulary of nationalism and secularism into Iran where society, consisting mostly of aggregates of groups, had little national self-consciousness.¹³ The second flitted about the Islamic world gathering support for futile Pan-Islamic politics and for a reform of Islam that would have repudiated the consensual achievement of previous centuries. Neither a modern Islam seasoned by Western science nor Persian nationalism was at all convincing to Middle Eastern society by the turn of the twentieth century. In the short run both men were failures.

These two intellectuals escape the boundaries of Islamic history as it has been pursued in the West. Each exploited a conflict, for which there was no parallel in the past, to obtain power, and each denied both the universal and the consensual thrust of classical Islamic civilization. Did they represent the vanguard of a twentieth-century revolution? If so, what is their proper histori-

¹² Hodgson, *The Venture of Islam*, 3: 166–68.

¹³ A better example of Persian nationalism, however, would be Mīrẓā Āqā Khān Kirmānī. See Mangol Bayat Philipp, "The Concepts of Religion and Government in the Thought of Mīrẓā Āqā Khān Kirmānī, a Nineteenth-Century Persian Revolutionary," *IJMES*, 4 (1974): 381–400.

cal framework? Should it be secular nationalism? Malkum Khān might fit; but he was an opportunist. Should it be some form of modernized Islam? Afghānī would surely fall within such a category; but analyzing what he really meant requires the ingenuity of a master detective, something Nikki Keddie provides. The intellectual travail of both "modernizers" reveals that the old methods of Islamic history no longer apply for the nineteenth century and that the intellectual class of the Middle East is transforming itself.¹⁴

The emergence of a modern technical elite in the Middle East is now fairly well documented.¹⁵ Concerned with economic development and educational innovation—the shape of the future in a modern industrial society—these managers seek an intellectual order that is bound to have little relation to the highly structured system of religious thought that emerged in the Islamic world by the end of the Abbasid caliphate, in the thirteenth century.

It is only within recent times, however, that the intellectual interests of the modern element within Middle Eastern societies have spawned a new historiography, a young body of thought that places a great deal of weight upon the element of conflict with the outside, usually conceived of in terms of the colonial struggle with the imperialist West.¹⁶ Increasingly, however, the real historical issue in the Middle East must become an internal one. Not only has a rapid rate of economic change so differentiated the Middle Eastern states that particularism in the social and economic structures has been immeasurably strengthened, but also the need to transform Middle Eastern societies from agricultural regimes to industrial societies is sure to have an explosive effect upon the way the past has been perceived. The stress today's economists place upon noneconomic factors in explaining the lack of development (to say nothing about the need for a new legitimacy to back tough economic decisions) points toward an intellectual war between modern and traditionalist historians in the Middle East.¹⁷

Notwithstanding Hodgson's wish to move the history of the Islamic world from its Arab pole, events will most probably place the radical Arab historian at the forefront of ideological self-criticism. Economic and political acts have loosened the Islamic moorings of the Middle Eastern world. Yet one of the basic cultural reference points for the population of Arab, Turkish, and Persian lands is still a unitary and defensive view of an Islamic golden age. If this vision of the past is to be criticized and reinterpreted in order to encourage ideological development more appropriate to the revolutionary era in which

¹⁴ Nikki R. Keddie, "Is There a Middle East?" *IJMES*, 3 (1973): 255–71. The author of al-Afghānī's biography did not adopt the point of view of the man she studied, but called for a history less entangled in the universalistic themes of Islamic history.

¹⁵ Unfortunately labeled the "new middle class" in Manfred Halpern, *The Politics of Social Change* (Princeton, 1963), 51–78.

¹⁶ Expressed most dramatically for Arab lands by Franz Fanon, *The Wretched of the Earth* (New York, 1963), 35–94, *passim*. For Iran, see Jalal Al-e Ahmad, *The School Principal*, trans. John K. Newton (Minneapolis, 1974), 13–18. The special appeal of Marxism in the Middle East is described by Laroui, *La crise*, 149–52.

¹⁷ *Ibid.*, 189–219, and Z. Y. Hershlag, *The Economic Structure of the Middle East* (Leiden, 1975), 1–9, 13–37, and 254–64.

the Middle East finds itself, then Arab authors are, by history, better placed to challenge the testimony of believers and to break the cake of custom. Should Western historians of the Middle East fail to perceive this change because of loyalty to an outmoded religious and literary tradition, they will surely see a breakdown of the modern dialogue.

If a metaphorical shift from consensus to conflict is to take place within the field of Middle Eastern history, it is probable that the sectarian battles of the Islamic past will take on a new interest. It is appropriate, therefore, that Julius Wellhausen's *Religio-Political Factions in Early Islam* has been translated into English. Despite the turgidity of the text—in both German and English—this old study of the oppositional movements at the dawn of Islam deals with a theme that modern historians of Islam in both the West and the Middle East should find increasingly relevant: conflict.

To invest more energy in the consensual approach to the Muslim past on the basis of wisdom received from the 'ulamā' will lead to diminishing returns. Not only has the emergence of conflict announced the defeat of the old consensus, but also Middle Eastern historians are certain to broaden the scope of their work into social and economic regions that the 'ulamā' ignored.

Publication of two important books proves that Western scholarship can expand its efforts into these two fields with great profit. Maurice Lombard's *Golden Age of Islam* is a translation from the French of his economic history of the Muslim world from the seventh to the eleventh centuries. It offers a concise treatment of a vast subject and should become a standard textbook for courses on Islamic history. *War, Technology and Society in the Middle East*, edited by V. J. Parry and M. E. Yapp, is a study of how war and military technology have changed the society of the Middle East. A timely volume, the collection of uniformly well-written papers traces Middle Eastern military history from the seventh to the twentieth centuries. Approximately a fourth of the book, moreover, shows how the modernization process placed Middle Eastern armies at the vortex of social change.

These recent publications all highlight the dilemmas faced by the Islamic historian. In the Middle East incompatible perceptions of the past reflect the ideological ambivalence of a society passing through a structural transformation of revolutionary proportions. Who the new culture-bearing elite will be and what message they will carry is unclear. Certainly few young Muslim historians will explore pre-Islamic poetry as a means of understanding their former civilization; more likely they will occupy themselves with the study of why capitalism did not appear in the Middle East. In the face of such conditions, Islamic historians in the West need not abandon their work and rush to absorb the intellectual framework of radical Middle Eastern historians. The cultural unity that Islamic civilization represented will continue to give the Middle East a cohesion Islamic historians can neglect at their peril. Yet they must also contend with the revolutionary impact of the modern differences, increasingly economic, between the Middle East and the West. It is possible for Western Islamicists to modify their narrow literary

viewpoint, to explore radical movements, to be more critical of the unity of the classical era, and, at the very least, to deal with the underdeveloped social and economic history of Islamic society from a perspective that does not rule out conflict. There is no reason why the process of modernization should destroy Islamic history in the West as a means of bridging the cultural gap between two now thoroughly interconnected societies.

Herbert Hoover: A Reinterpretation

A Review Article by ROBERT H. ZIEGER

Public Papers of the Presidents of the United States: Herbert Hoover. Containing the Public Messages, Speeches, and Statements of the President. (Published by the National Archives and Records Service.) Washington: Government Printing Office. 1974. Pp. xxxvi, 812. \$13.30.

FRANCIS WILLIAM O'BRIEN, edited and with commentary by. *The Hoover-Wilson Wartime Correspondence, September 24, 1914 to November 11, 1918.* Ames: Iowa State University Press. 1974. Pp. xxvi, 297. \$7.95.

GARY DEAN BEST. *The Politics of American Individualism: Herbert Hoover in Transition, 1918-1921.* Westport, Conn. and London: Greenwood Press. 1975. Pp. vi, 202. \$13.50.

BENJAMIN M. WEISSMAN. *Herbert Hoover and Famine Relief to Soviet Russia, 1921-23.* (Hoover Institution Publications, 134.) Stanford, Calif.: Hoover Institution Press. 1974. Pp. xv, 247. \$7.95.

J. JOSEPH HUTHMACHER and WARREN I. SUSMAN, editors. *Herbert Hoover and the Crisis of American Capitalism.* (The American Forum Series.) Cambridge, Mass.: Schenkman Publishing Company. 1973. Pp. xiii, 138. \$7.50.

MARTIN L. FAUSOLD and GEORGE T. MAZUZAN, editors. *The Hoover Presidency: A Reappraisal.* Albany: State University of New York Press. 1974. Pp. vi, 224. \$12.00.

EDGAR EUGENE ROBINSON and VAUGHN DAVIS BORNET. *Herbert Hoover: President of the United States.* (Hoover Institution Publications, 149.) Stanford, Calif.: Hoover Institution Press. 1975. Pp. xii, 398. \$12.95.

JOAN HOFF WILSON. *Herbert Hoover: Forgotten Progressive.* (Library of American Biography.) Boston, Toronto: Little, Brown and Company, 1975. Pp. viii, 308. \$3.95.

PRESENT CONCERNS IMPINGE upon analyses of the past, particularly the recent past. Events of the last decade and a half have shaken American liberals. The New Deal and social democratic traditions face heavy assault from both right and left. In their contributions to the reassessment of liberalism, historians have refurbished, not only heroes of the radical left, but conservative opponents of New Deal liberalism as well. Much of the literature on Herbert

Hoover illustrates the simultaneous resurrection of the right and repudiation of liberal policies, both domestic and foreign. Indeed, so plausible and balanced does Hoover often appear in retrospect that it requires an effort to remember his political failure and the consistency with which the American people repudiated him and his associates at the polls.

The recent literature is highly respectful of and sympathetic toward Hoover. The only clear exception is Murray Rothbard's essay in the Huthmacher-Susman book, which pillories Hoover's deviations from market economics. At the other extreme, the Robinson-Bornet volume admits of virtually no faults in Hoover, aside from his failure to pander to popular whims. Gary Dean Best and Benjamin M. Weissman seek scrupulous neutrality, but both enhance Hoover's reputation. Joan Hoff Wilson's biography combines criticism of Hoover's rigidity, self-absorption, and defensiveness with sympathy for his views and a tone of presentist special pleading for his postpresidential career. In all, the most successful efforts to attain fresh insights into Hoover's career without overcompensation are those of Ellis Hawley, whose essays grace both the Susman-Huthmacher work and the Fausold-Mazuzan collection.

Hoover's collected papers for 1929 portray the man at the apex of his career. For most of that year he was a popular president with an energetic and positive program. No longer merely an influential advisor and not yet a political has-been, Hoover shows in the papers the optimism and purpose with which he entered the presidency. Press conference transcripts, speeches, letters and statements on particular issues, and presidential proclamations record the confident public face of his administration. Extensive supplements and appendices reprint speeches from the 1928 presidential campaign and his postelection trip to Latin America. A modest spread of photographs reveals the president in his official capacities, not yet ravaged by the Depression.

The public papers contain few surprises. The prose is leaden and difficult, attempts at humor heavy and unconvincing. Energy and seriousness are equally apparent. His messages on agricultural and tariff legislation, his background press briefings, and his special letters and statements reflect thorough preparation. Hoover's statements following the conferences with industrial, financial, and labor leaders held after the break in the stock market ring with a confidence born of his detailed knowledge of the economic system. The presidential papers reveal Hoover's reliance on expert opinion, special commissions and advisory groups, and voluntary action, as well as his repugnance for legislative initiatives. The *Public Papers . . . 1929* volume is a handsome addition to the National Archives' series and begins to fill an important gap in published presidential papers.

It is difficult to understand why the other volume of documents under review was published. Although *The Hoover-Wilson Wartime Correspondence* covers over four years, most of the communications are confined to the period of Hoover's actual service in the Wilson administration—June 1917–November 1918. Many of the communications deal with mundane matters and most would be of interest only to those concerned with the day-to-day activities of

the Food Administration. Although historians have recently done much to link the two men ideologically, little in this collection touches directly on this theme. The letters and memoranda do remind the reader of the enormous complexity of food and agricultural problems during the war and reflect Hoover's decisiveness and vigor as an administrator. It is unclear, however, what broader purposes might be served by the publication of these items, especially since O'Brien's notes are largely limited to identification of references and discussion of immediate contexts. A monograph on Hoover, Wilson, and food and agricultural problems during the war would make sense; this collection of correspondence does not.

Other aspects of Hoover's early public career have already drawn monographic treatment. In *The Politics of American Individualism* Gary Dean Best follows him from his return from Europe to his entry into Harding's cabinet in 1921, while Joseph M. Weissman in *Herbert Hoover and Famine Relief to Soviet Russia, 1921-23* examines his humanitarian work as secretary of commerce. Although Best's book is unattractive in format, shockingly expensive, and narrowly focused, it makes some useful points. Carefully delineating the domestic and international strands of ideology that Hoover developed during and after the war, Best examines closely his service on the Second Industrial Conference, the abortive campaign in his behalf for the presidential nomination, and his decision to enter Harding's cabinet. Best argues that Hoover's concern with the economic consequences of the peace, both international and domestic, guided his attitudes toward the League of Nations, the labor problem, and his own political career. Seeking to promote intelligent debate over these issues and to propound his version of American exceptionalism, Hoover used his postwar fame to bring central questions of postwar political economy to public attention prior to joining the cabinet.

Although the book is solidly researched and intelligently presented, its narrow focus on three years of Hoover's life restricts its significance. Given this narrow focus and the previous appearance of important segments of the book in journal articles, this volume cannot be considered indispensable.

Herbert Hoover and Famine Relief in Soviet Russia, 1921-23 examines another aspect of Hoover's early public career. Weissman's detailed description of the American Relief Administration's enterprise focuses on Hoover's role and on the foreign-policy implications of this private body's activities. Based on archival sources, interviews, and scholarly and eyewitness literature in English and Russian, the book judiciously confronts the various ideologically tinged historical controversies associated with ARA efforts.

Weissman insists on the fundamentally humanitarian nature of the aid program. While the Hoover circle hated the Soviet regime and while some Americans were primarily concerned with trade expansion, neither counter-revolutionary purpose nor economic self-interest dominated ARA activities. Although Hoover and his associates clearly hoped that relief might encourage moderate forces in Soviet Russia, the United States government did not use

the relief mission as a device for political interference. Nor does it appear that American policy-makers sought an early detente through famine relief.

Herbert Hoover and Famine Relief is clear, direct, and cogently argued. Frequent breaks in the narrative, however, and numerous undigested quotations clog Weissman's limpid prose. The book is not, as the publishers say, a philosophic essay, but it is a solid monograph on an important subject.

If these two monographs reflect favorably on Hoover's postwar activities, his tenure as cabinet member and president has generated sharp controversy. *Herbert Hoover and the Crisis of American Capitalism* contains four essays reflecting the mixed reactions of scholars to this phase of Hoover's career. Ellis Hawley, Robert Himmelberg, Murray Rothbard, and Gerald Nash in their respective papers and rejoinders, examine Hoover's ideology and his policy choices, both as cabinet member and as president. The result is a spirited, uneven volume, unlikely to find the wide audience for which the American Forum Series is intended.

The editors provide a weak and careless introduction. Gaucheries of usage and spelling hint of haste and distractedness. The introduction contains irritating assertions that are either meaningless or questionable. Thus, we learn that among "the casualties of the First World War was the Progressive Era," a statement that is either tautological or historiographically dubious. In an apparent effort to make Hoover relevant to today's readers, the editors proclaim that his views of decentralization and voluntarism have gained new adherents, both with the right and with the New Left. This is a useful reminder, but Susman and Huthmacher confuse the issue, lumping these two categories of dissidents together and linking the resulting conglomeration with still another category of "Americans frustrated by the rise of giantism on all sides." To this hopelessly amorphous mélange of troubled people is attributed a reform program, a key ingredient of which is "corporate responsibility," a rubric that either expresses a pious hope for greater business morality or a greater regulatory role for government ("giantism"?). In any event, "corporate responsibility" may be a fond dream of good citizens, but it never sent Mark Rudd to the barricades. Against this left-right-frustrated-coalition the editors pit the "liberals of the 'unreconstructed' New Deal type [who] continue to advocate attempts to solve social problems by piling federal programs and appropriations still higher . . ."—hardly a fair statement of the goals or methods of the liberal-labor-civil rights bloc. Moreover, as Michael Harrington, Bayard Rustin, and others have demonstrated, there has been precious little real commitment of resources devoted to correcting social inequities.

Thus the editors start off with a confused and simplistic perception of the recent debate over public policy. This misleading focus is particularly annoying because this volume was commissioned expressly to stimulate debate and to introduce readers to relevant historical controversy. Left with little coherent direction by the editors, we must read the essays as separate pieces,

gleaning from them what we can. Fortunately, they are frequently informative and provocative.

Robert Himmelberg's essay is informative. It reminds readers that for all of Hoover's associationalism, he often supported antitrust action. If the dominant motif of his public career was an apolitical, nongovernmental New Nationalism, he never entirely discarded the New Freedomite fear of cartelization. During his administration key antitrust suits were initiated and won, and the Federal Trade Commission and the Department of Commerce scrutinized trade associations very closely.

Murray Rothbard's essay is provocative. A doctrinaire free-market economist, he sees Hoover as a fraud and a charlatan who compiled a record of governmental intervention in the economy throughout his tenure in the cabinet. For Rothbard, Hoover's involvement in labor matters, which other observers have seen as cautious, episodic, and limited, constitutes a coherent series of steps toward a form of corporate syndicalism. In the economist's view, America stood on the brink of fascism in 1932–33 when corporate leaders such as Gerard Swope pressed upon Hoover an NRA-like approach to recovery. In his "finest hour," Hoover rejected the Swope plan, although his career up until that point would seem to have been a preparation for it.

Rothbard sees Hoover as a practitioner of corporate liberalism, little different from any other major political figure. If even Hoover's brand of government-business cooperation and his kind of labor policies constitute drastic intervention in the market economy, the untrammelled functioning of which is vital to political liberty, the entire twentieth century—the entire modern world, for that matter—becomes a preface to fascism. If, however, there are degrees of regulation and intervention in political economy, and if governmental activity in the economic sphere is an inescapable aspect of any politics—especially democratic politics in which men and women seek advantage through organized public effort—Rothbard's essay is more dogmatic than convincing. This apocalyptic article is startling, superficially disturbing, and finally unsatisfying because it simply gives another name to actions and programs familiar under other rubrics.

The articles by Gerald Nash and Ellis Hawley are sedate when contrasted with Rothbard's sweeping claims. Nash stresses the role of science and Quakerism in the development of Hoover's ideology and contends that these forces bent him toward a moral absolutism that impeded him politically. Hawley tries to define Hoover's ideology. He stresses links to prewar progressivism and sketches Hoover's efforts to achieve a liberal capitalist order without resort to massive governmental intervention. Hawley adheres to the general view of prerevisionist liberal historians in his assessment of Hoover's performance as depression president, finding him ultimately guilty of an "intellectual rigidity [that] left him isolated from the main currents." But while Hawley's conclusions may be similar to standard liberal interpretations such as those offered by Carl Degler, Arthur M. Schlesinger, jr., and Richard Hofstadter, his immersion in the Hoover papers and his sure grasp of

twentieth-century economic policy enable him to understand Hoover on his own terms and to penetrate the complex substance of Hoover's ideology and programs. The reward of Hawley's essay lies not so much in his conclusions as in following him through the largely unexplored paths by which he arrives at them.

Hawley's depiction of Hoover as a link between power progressivism and the interest-group liberalism of the New Deal is amplified and sharpened in his essay in *The Hoover Presidency*. This volume contains nine papers delivered at a symposium at the State University College at Geneseo in April 1973. Donald McCoy's contribution stresses Hoover's political acumen in the 1928 campaign, while David Burner recalls the high hopes and systematic programs with which Hoover entered the White House. Essays by Frank Freidel and Albert B. Rollins focus on the interregnum of 1932-33. Freidel's account, a distillation of the exhaustive treatment of the subject in his biography of Roosevelt, contains few surprises, but Rollins argues that FDR had long envied and resented Hoover and had built his political career in the 1920s, in part in pointed opposition to his more successful erstwhile colleague of Wilson days. Roosevelt emerges from Rollins' account as an unusually devious and irresponsible public figure, exploiting Hoover's straitened circumstances and his forbidding public presence shamelessly in the 1932 campaign. The evidence is fragmentary at best, however, and there is no reason to abandon Freidel's more familiar version.

The heart of the volume is the section on Hoover's antidepression efforts. Hawley ably sketches both the hopes and limitations of his voluntary associationalism. The shift that occurred in the 1930s, he declares, "was not from laissez-faire to a managed economy, but rather from one attempt at management, that through informal private-public cooperation, to other more formal and coercive yet also limited attempts." Albert U. Romasco usefully surveys the Hoover historiography. He insists that a clear pattern of governmental response to economic downturn, as evidenced in 1907, 1914, and 1921-22, makes Hoover's responses to the Great Depression appear less innovative, thus complementing Hawley's effort to put Hoover's economic policies in context. Finally, in an excellent analysis based on his recent book, Jordan A. Schwartz probes Hoover's political attitudes and finds him arrogant, contemptuous of Congress, and suspicious of the processes of elective government. Schwartz traces these attitudes to Hoover's engineering background and his long and successful career as an expert-administrator whose involvement in the practical realities of business, logistics, and economic problems left him with little aptitude for, or appreciation of, messy but equally practical political realities.

The volume closes with two essays on foreign policy. In one, Selig Adler casts a skeptical eye on the recent conversion of William Appleman Williams and New Left historians to the cause of Herbert Hoover. He suggests that Williams, in his eagerness to challenge the liberal consensus that dominated foreign-policy historiography until the 1960s, may have gilded the lily in

Hoover's case, emphasizing out of context some of his anti-interventionist utterances while ignoring or slighting his economic expansionism, racialism, and strident anticommunism. Adler maintains that, however much Vietnam may have revealed the limitations of interventionism in the 1960s, the menace of fascism and militarism left FDR with little room for maneuver in the 1930s, *pace* Hoover's objections. He calls for vigorous monographic examination of the claims for Hoover's foreign and domestic policies raised by Williams.

In her essay, Joan Hoff Wilson analyzes the economic aspects of Hoover's policies, stressing the link between his efforts in this field as secretary of commerce and those he undertook as president. Wilson convinces a sure grasp of such difficult and complex issues as reparations, tariff policy, and foreign exchange. Not content with rehabilitating Hoover's reputation as a shrewd and highly informed practitioner of economic foreign policy, Wilson also suggests the perspicacity of his overall approach to foreign policy, noting that many of his strictures against overextension have been borne out. Since the essay mentions the Manchurian crisis only in passing, however, and since its substance is confined to the 1921-33 period, the last two or three pages are a gratuitous exercise in political moralism. Wilson is barely able to suggest the outlines of Hoover's response to thirty years of world events, much less to explicate the complexities of foreign policy after he left office.

The Geneseo conference must have been rewarding for its participants if the high quality of the published results is any indication. It appears that neither Edgar A. Robinson nor Vaughn Davis Bornet attended. If they did, none of the subtlety or ambiguity of recent scholarship concerning Hoover influenced their book, *Herbert Hoover: President of the United States*. This is a curious volume, a homage to an old Stanford man by two historians with long associations with that university. While it cites much of the recent literature, and while the authors interviewed hundreds of people associated with Hoover, it is a simplistic defense of Hoover's entire course of action as president. Moreover, its authors' penchant for vapid generalizations and pronouncements is befuddling and irritating. What might have been a sympathetic, tightly argued reassessment of Hoover is a pretentious apology.

The authors perceive Hoover as a man so uniquely capable and so profoundly knowledgeable as to dwarf his contemporaries. They see a public yearning for reform, for freedom from special interests. At the same time, they see the nation's political structure—its parties and caucuses, its constitutional institutions—as hopelessly provincial, self-serving, and corrupt. Little is adduced in support of these claims; they are simply asserted, often in thick, cloudy language and with gratuitous moralisms. Theodore Roosevelt, they declare, “did not accept the doctrines of the ruling elements in the Republican party. In his middle course and his pragmatic approach, he never betrayed the basic meaning of earlier American political experience. Builders of a free society must be free” (p. 7). Again: “Perhaps nothing was so characteristic of the ‘politics’ of the period 1901-1929 as the slogan ‘restore the government to the people.’ This appealed to many of the electorate who felt that

party organizations had become subservient to special interests. . . . The average citizen was quite content to believe that the managers of the great political enterprise they called the United States were the practical men known as 'politicians.' " The book is filled with such judgments that lack references and convey little meaning. A good editor might have corrected this tendency and steered the authors closer to their real purpose, informed rehabilitation of the Hoover presidency.

Such an editor might also have reduced the partisanship of the volume. A reader of *Herbert Hoover: President of the United States* would never realize that knotty, complex issues gripped the country in these years, issues over which honest people could differ and which they still debate. Everything is black and white: Hoover was invariably right, albeit at times guilty of the understandable lapses in patience that afflict a man surrounded by fools and charlatans; his critics and opponents were invariably wrong. The authors use recent research where it suits their purposes but not as a means of elevating the debate to new levels of analysis. A taste of the authors' efforts at analysis is available on the last page. They ask "why the people rejected President Hoover's program for the new scientific era, a program rooted in voluntarism and cooperation rather than federal domination. . . ." Hoover, they aver, "conducted himself as an enemy of war and armaments; as a friend of constitutional government; as a supporter of voluntary methods; as a preserver of a partially regulated capitalist system that could hold its own. . . . President Hoover . . . was, above all, a determined spokesman for the traditional American virtues, hopes, and ideals of individual opportunity, personal freedom, and love of country." The back dust jacket lists encomia for the volume by such luminaries as Thomas A. Bailey, Frank Freidel, Norman Graebner, and Ellis Hawley. But these worthies notwithstanding, the book is neither "succinct" nor "lucid." It may, as Bailey suggests, "help swing the pendulum toward a fairer and more balanced appraisal of the thirty-first President," but only for those who have read nothing on Hoover for the past ten or twelve years. The book is a serious disappointment, given the authors' access to sources and their scholarly reputations.

With all the resurgence of interest in Hoover, there is not yet a comprehensive biography. Until one appears, Joan Hoff Wilson's *Herbert Hoover: Forgotten Progressive* will serve as a brief, competent introduction to his entire career. Based as it is on newly opened archival and manuscript materials and on recent scholarship, it is more sophisticated and analytical than previous biographies. *Herbert Hoover* portrays a man who combined great practical ability and intellectual consistency with a dour and almost reclusive personality. Wilson's discussion of Hoover's ideology, noting both its relationship to other strands of progressivism and its limitations, is fair, although it frequently lacks sharpness and clarity. While her observations on Hoover's ideas and policies on domestic issues in the 1920s parallel closely those of Hawley and other recent students, Wilson's examination of Hoover's foreign policy as president and his views of war and peace in the World War II era are fresh

and particularly stimulating. An excellent bibliographical essay enhances the value of the book and partially compensates for the absence of notes, although at times materials drawn heavily upon in the text are not given attribution in the essay.

Wilson's discussion of Hoover's postpresidential career is lengthy and useful, especially in light of the lack of scholarly attention to this subject. She credits the thirty-first president with remarkable prescience for his warnings against the growth of a centralized state with vast, coercive powers and limitless international commitments. Although she acknowledges Hoover's extreme and often hyperbolic anticommunism, she notes that he never joined the McCarthyites. Her resurrection of his often trenchant and provocative critiques of foreign and military policy in the 1940s and 1950s is a great service, as is her discussion of Hoover's skepticism of bipartisanship in these areas. As Hoover did himself, however, Wilson leaves unanswered central questions about how the United States might hope to achieve the kind of expanding economy Hoover thought necessary for the maintenance of a unique social order without an aggressive foreign policy.

The chief flaw of the last part of the book lies in Wilson's largely implicit effort to enlist Hoover as a social critic. She contends that Vietnam and Watergate "have amply demonstrated his worst fears of a society and economy run from the top down by a coercive system of expertise and by what has recently been called an 'arrogant elite guard of political adolescents' " (p. 278).

This comment reflects the tone of the last part of the book. It also demonstrates some of the limitations of Wilson's prose and analysis. The use of the passive voice—a feature of her writing—obscures sources (there are no footnotes) and, in this case, substitutes a kind of cryptic rhetoric for thoughtful analysis. Hoover's penchant for expertise, his disdain for Congress, his contempt for the hurly-burly of the political process, and his compulsive self-containment hardly make him the model of the open, candid, and accessible chief executive. His faith in the public-spiritedness and ability of corporate executives certainly provided precedents for elitist decision-making. It is hard to see how either Vietnam or Watergate resulted from a "coercive system of expertise." True, Kennedy, Johnson, and Nixon relied heavily on their particular versions of the best and the brightest. But so did Hoover, whose whole public philosophy—as Wilson so correctly notes in an early chapter (pp. 72–3)—denigrated the elected representatives of the people and elevated the engineers, experts, and managers, whether governmental or private.

Moreover, Wilson seems to accept the same kind of facile assumptions shared by Susman and Huthmacher that the New Deal and postwar liberal reform have foisted upon the American people some sort of coercive welfare state without their consent and against their interests. In reality, as Hawley and Otis Graham, Jr. (in his superb essay in Elkins and McKittrick, eds., *The Hofstadter Aegis*) remind us, New Deal reforms were partial, frugally funded, closely in line with prevailing conceptions about the role of government, and extremely sensitive to the rhetoric of decentralization and local control, often

to the disadvantage of minorities, working people, and the poor. Far from creating a centralized welfare state, modern liberalism has fallen far short of its goals—always more modest than those of other Western states—in education, old-age security, health care, and many other areas.

For all of Hoover's rhetoric about grass-roots participation and the values of local decision-making, his reliance was always on elite groups. For all the limitations of post-1933 liberalism, it has been the New Deal and its heirs that have done the most to bring working people, blacks and other minorities, and the poor into decision-making processes. While it may be true that the country's international enterprises over the past generation have threatened domestic liberties, there is no easy linkage between the perniciousness of these activities and the creation of a partial welfare state. Just as the legacy of Hooverism is, as Wilson recognizes in the first two-thirds of the book, mixed, so is the inheritance of the New Deal era. If many contemporary Americans—primarily the liberals, radicals, and hirsute activists, with whom Hoover would have been, to say the least, ill at ease—might today see wisdom and good judgment in his broad strictures on foreign policy, few blacks, urban dwellers, laborites, or poor people would find much of use in his observations on government and public policy in the domestic field.

Wilson's biography accurately summarizes the interim conclusions of this round of Hoover scholarship. The limitations of Democratic liberalism, the agony of Vietnam, and second thoughts about the general efficacy of governmental solutions to social problems are conducive to favorable re-examination of men such as Hoover and Robert A. Taft. In addition, the opening of the Hoover presidential library at West Branch, Iowa, occurred when the graduate schools were swollen with eager Ph.D. candidates, many of whose lives inclined them toward a critical assessment of the previous generation's version of our recent past. At its worst, the current wave of scholarship simply updates and reinforces the self-serving and petulant tone of Hoover's own apologetics and those of his colleagues and associates in their defenses of the Chief. At its best, the current scholarship opens up to re-evaluation not merely Hoover himself, but a whole way of looking at the American social order. Let us hope that Hoover, now presumably rescued from the distortions and slurs of his political and ideological enemies, is not abstracted from his context and made to shoulder a burden of consistent public virtue and intellectual acuity that the record will not support.

Herbert Hoover rose from native stock to a position of wealth and power, first in private life, then as a public figure. Small-town Iowa and Oregon, Stanford University, the engineering profession, and private humanitarian service decisively shaped his preparation for the presidency. This background contributed personal qualities of energy, dedication, and integrity. It encouraged voluntarism, efficiency, and privatism. He never abandoned this background, nor did he transcend it. In certain respects, it would have stood the country in good stead after his fall from power; in other ways, it deserved to be rejected.

"Could it be true," asked Richard Hofstadter in his brilliant assessment of

the 74-year-old former president in 1948, "that the great American tradition was nearing its end because the people had no ear for spokesmen of the old faith?" The rise of militant unionism, agitation over civil rights, the adoption of new regulatory and welfare-state devices, and the general reliance on federal action in a wide range of activities may have resulted from devious politicians, momentary frustrations, and temporary circumstances. But were not such programs and initiatives genuinely popular and even necessary for the preservation of the social order? "Perhaps, after all," Hofstadter wrote, "it was the spirit of the people that was not fundamentally sound." If historians are insensitive to and impressed by the fact that Hoover and his social vision have been frequently and resoundingly repudiated by the American electorate, they may find themselves sharing with Hoover, not only the more attractive aspects of his public thought, but some of the implicit distrust of the people that lurked beneath the surface of his rhetoric.

Reviews of Books

GENERAL

KENNETH WINETROUT. *Arnold Toynbee: The Ecumenical Vision*. (Twayne's World Leaders Series.) Boston: Twayne Publishers. 1975. Pp. 155. \$7.95.

Arnold Toynbee's arena has been the world, or perhaps more grandly, the cosmos. As Kenneth Winetroun notes in this study, "For Toynbee, history becomes a vision of God revealing Himself in action. . . . 'Man's *Oikumene*' is a 'fragment of God's Universe'."

The aim of Winetroun's brief but useful introduction is to demonstrate Toynbee's commitment to this view. Winetroun's analysis principally concerns *The Study of History*, that multivolume, alpha and omega history of mankind that contains the particulars of Toynbee's ecumenical vision of history: "God revealing Himself in action." From this vision comes Toynbee's conclusion in *The Study of History* that religion rather than civilization is the intelligible unit of historical study. *The Study of History* is the history of God more than of the world He created; the thinking behind it is at once historical and philosophical. As understood by Winetroun, *The Study of History* presumes a view of history as pattern, imagination, curiosity, feeling, contemporaneity, myth, theology, and prophecy; history further is comprehensible only in the long view and universally.

Winetroun is coherent in style and explication, and he argues well for Toynbee's place in the world of scholarship, thought, and criticism. One chapter compares Toynbee and Spengler on the future of the West; another treats Toynbee on the future of man. Here is a key area, as Toynbee says: "My study of the world will have been barren and irresponsible if it has not equipped and spurred me to do what I can—infinitesimal though the effect of my action may be—to help mankind cure itself of some of the evil that, in my lifetime, I have seen human beings inflict on each other." Winetroun further discusses how Toynbee extended this presentism to analyze the dynamics of change, and he concludes with an evaluation of that all-important

point, Toynbee's personal relationship with religion.

The book is limited to Toynbee's primary theses and the philosophical-cum-historical foundations upon which they rest. The treatment of philosophy seems sound, as does Winetroun's discussion of Toynbee's historiography. If there is a major flaw it is that Winetroun is too reverent, treating Toynbee with awe while almost curtly dismissing his critics. Nevertheless, the awe is understandable: it is that of one confronted by a giant intellect and a great man of historical thought.

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LEON J. GOLDSTEIN. *Historical Knowing*. Austin: University of Texas Press. 1976. Pp. xxvii, 242. \$10.00.

BERNARD LEWIS. *History: Remembered, Recovered, Invented*. Princeton: Princeton University Press. 1975. Pp. 111. \$6.95.

Until World War II philosophers in this country and Britain seldom wrote on the philosophy of history. Since the war, however, many have, and two dominant schools have emerged. The first has argued that historians make sense of history by drawing on general laws. The second has portrayed history as a kind of narrative discourse whose chief feature is its credibility. Neither school has struck most historians as particularly fruitful.

In *Historical Knowing* an American philosopher, Leon J. Goldstein, shows why neither can be. He deals at greatest length with the general-law theorists. They have held that historians appropriate general laws from natural science or mundane experience to explain the past and have insisted that the past which historians seek to explain has an objective existence of its own. Goldstein denies both assertions. He demonstrates that historians think inferentially, not deductively, and he brilliantly demolishes the notion that there is a real past. He notes, for good measure, that the examples that general-law theorists give us of historical explanation are remarkably vague and un-

persuasive. In his attack on the exponents of the narrationist school, Goldstein is briefer but equally effective. He shows that their definition of history—that it is something marked by credible narrative—leaves history indistinguishable from fiction and naively ignores the way in which historians use evidence.

In all this Goldstein's chief goal is to demonstrate that historians think in a way which recent philosophers have completely failed to understand. Goldstein uses a number of examples to make his point, and some of these may leave the reader squeamish. A. J. P. Taylor is presented as having selected evidence in a way no more arbitrary than any other historian in his *Origins of the Second World War*, and Arthur Schlesinger, Sr. and F. Lee Benson emerge in virtual agreement over the nature of Jacksonian democracy. Goldstein might profitably pay more attention to the nature of historical controversy than he does. Yet this is a small criticism of a book that does a splendid job of clearing away two profitless theories of historical knowledge.

If Goldstein sought to determine what historical thinking is, Bernard Lewis shows us how irresponsible historical thinking can be. Citing Near Eastern examples, he demonstrates how current needs have led men to warp the past virtually beyond recognition. Lewis argues that this began when Near Eastern intellectuals, in their search for a glorious past that would compensate for current weakness, selectively expropriated the work of Western scholars and novelists. Then, with the collapse of the Ottoman Empire and Western colonialism, these intellectuals developed their views of the past to the point where, in highly colored form, they often serve reckless or reprehensible political policies.

In describing the rise of ethnocentric and nationalist historiography, Lewis tackles a question that Goldstein does not consider: the way in which present circumstances can shape not only the questions that men ask of the past, but also the answers they give. Like Goldstein, Lewis believes that the historian should go where his evidence takes him. Unlike Goldstein, Lewis knows that the historian (or lesser mortal) often does not. In framing his picture of how the past has been conveniently resurrected and touched up in the Near East, Lewis strongly suggests that the same has taken place, or is taking place, elsewhere. There seems little question that this is so; the warning that the abuse of the past can be dangerous, although scarcely news to historians, is presented in a fresh and persuasive way.

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LÉON-E. HALKIN. *Initiation à la critique historique*. (Cahiers des *Annales*, 6.) 4th rev. ed.; Paris: Librairie Armand Colin. 1973. Pp. 267. 25 fr.

It is only fitting that such a solid and prolific contributor to European historical scholarship prepare, more than forty years after his splendid study of Cardinal de La Marck appeared, the present study of history. He defines history as "la discipline qui étudie le passé des hommes et présente un tableau raisonné de leurs actions de portée sociale."

As Léon-E. Halkin points out in the *avant-propos*, this work is not intended as either a synthesis or a manual but rather as a presentation of "a selection of appropriate ideas and examples in order to understand better the difficulties [involved in the study] of history and of the historian's mission." He also points out something elementary that is too often forgotten: "The important thing is not so much to learn to write history, as to learn to read it intelligently." With these ideas in mind the author prepared a book divided into two sections: the first he calls "Notions de Critique Historique," and the second, "Applications de la Critique Historique." The first part deals with chapters on the history of history, historical reality and historical truth, methods and rules of historical criticism, hypothesis and synthesis, historical documents, the auxiliary sciences of history, history and philology, biography, and so forth. In the second part Halkin reviews the applications of history to such subjects as Erasmus, Philip II, the myth of Napoleon, and Hitler.

In this work, intended essentially for students, the author presents a clear and well-organized approach to the subject of historiography. He refers to traditional European historians and their contributions, but he ignores more or less completely all non-European historiography. As would be expected of a first-class medievalist, he stresses time and again the necessity of using primary sources, which today is, of course, taken for granted as an axiom, but not always practiced. Halkin continues that it is not always easy to analyze history; in fact for some "it is easier to scorn history than to pass in judgment over it." He contradicts himself only once when he states that "history is to the humanities what mathematics is to the experimental sciences: a guarantee of precision" (p. 49), whereas a few pages later (p. 51) he states that "there is no *one* history" and on the same page adds that "the duty of historians is precisely to make critiques of personal approximations and then to reduce them as much as possible."

On the causative interpretation of history, the author uses the example of the outbreak of World War II and the rise of Hitler to stress that there is no "one cause" of the great events in history but rather "hundreds of factors, known and unknown." Analyzing history in general and its value to us, he says that it "no longer claims to reign over the future or the present, nor even over the

past. However amoralistic this may be, it [history] does maintain an educational value—for those at least who can understand it—in the simple fact that it helps us to understand men and life. It is important that it [history] does not stop corresponding to that tenacious aspiration called 'the memory of mankind.' It is not the function of history to crush man beneath the weight of its past, but rather, to show this past and to remind man, discretely, that we are its heirs."

Halkin's study is a valuable contribution and the summation of a life dedicated to the study of history. If this study says little that is new or unexpected, it will nonetheless be welcomed by peers as well as by the newer generation of scholars.

ALAN SCHAM
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WILLIAM A. GALSTON. *Kant and the Problem of History*. Chicago: University of Chicago Press. 1975. Pp. xiv, 290. \$13.50.

This work is a critical examination of Kant's philosophy of history and its influence on the doctrine of historicism, which William A. Galston defines as "the doctrine that every human situation is circumscribed by a historical fatality that is invisible, unknowable, and arbitrary in the sense that it is not the inevitable or predictable outcome of any preceding situation" (p. 4), and on contemporary political doctrines, such as nihilism and revolutionary idealism, that presuppose this definition of historicism. Galston attempts to show that Kant's views on history, too neglected until recently, not only anticipated the theory and practice of modern liberal democracy, but also helped to precipitate what he calls "the contemporary crisis of liberal democracy" (p. 1). Galston proceeds by analyzing in detail Kant's two most important writings on history—"Conjectural Beginning of Human History" and "Idea for a Universal History from a Cosmopolitan Point of View"—and comparing them with the views of Aristotle, Aquinas, Rousseau, Hegel, and others. Galston concludes that Kant's irresolvable dualism rendered his concept of history "incapable of performing the task for which it was designed: the theoretical mediation between a natural world indifferent to human will and human will impotent to transform that world in accordance with its moral intention" (p. 269).

The author's critique of the essays themselves and of Kant's brilliant but ultimately unsuccessful attempt to reconcile his views on nature, morality, and history is especially good. Less satisfactory are his lengthy, largely irrelevant discussions of Aristotle and Rousseau, which add little to our understanding of Kant's uniqueness or significance. And, for purposes of comparison, why no discussion of Leibniz, Hume, or Herder, who are far

more important for an understanding of Kant than Aristotle or Aquinas? Moreover, the writing is uniformly tedious and sometimes unclear. On balance, however, Galston's first book is a worthwhile contribution to a neglected but significant side of Kant's philosophy.

ROBERT ANCHOR
University of Southern California

RUSSELL MCCORMMACH, editor. *Historical Studies in the Physical Sciences*. Volume 5. Princeton: Princeton University Press 1975. Pp. 185. \$12.50.

The authors of this unique and often fascinating volume on "everything you wanted to know about the cost of academic physics at the turn of the century but were afraid to ask" have provided a very useful collection of information. They have discovered, and at times created, figures for a multinational account of the numbers and growth rates of the population of academic physicists, students, and support staff; the personal income of physicists; laboratory budgets and special research funding, including the costs of major research tools and materials; the cost of new physical plants; and the productivity of physics papers per physicist and per unit of investment. To facilitate comparisons, all monetary figures are given in 1900 German marks—roughly equivalent in purchasing power to American dollars in 1960. The ingenuity displayed in establishing educated guesses for many of the figures is both wonderful and worrisome, and it demands that the reader use substantial caution in using the data presented.

Much of the information gathered here confirms the conventional wisdom, but there are some important surprises. By 1900 the United States led all major nations in total number of physicists and physicists per capita, and it was a close second to Great Britain in total expenditure per physicist. This result is particularly startling because the authors' method of counting physicists probably excludes many astrophysicists who held posts in astronomy. Because this field was both cost intensive and especially large in the United States, American numbers and support levels are probably understated with respect to those of other nations.

In spite of my reservations about the reliability of some figures, if parallel studies approaching this in quality are done for other sciences and for other times, they will greatly enrich our understanding of the institutional settings of the scientific enterprise.

RICHARD OLSON
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Santa Cruz

OWEN HANNAWAY. *The Chemists and the Word: The Didactic Origins of Chemistry*. Baltimore: Johns Hopkins University Press. 1975. Pp. xiii, 165. \$10.00.

Opportunities for formulating and answering basic historical questions are found in the origins of textbooks. The beginning and rise of chemistry as a unique science in the seventeenth century grew out of Andreas Libavius' (1560-1616) commitment to the didactic tradition that "man's arts be reduced to systematic presentation in words" (p. 116). Libavius brought logic, organization, and a concern for correct terminology to bear on the processes of chemistry that included extracting pure juices, mixing and separating substances, and changing and transmuting things. To do this he felt compelled to distinguish chemistry from its applications, such as mining, tanning, and medicine. In facing up to the difficulties in selecting and organizing the variety of processes common to the art of chemistry, he turned to the widely understood art of dialectic, which is "of discovering simple notions and of unveiling them in order for judgment" (p. 124).

The role and contribution of Paracelsus (1493?-1541) to the origin of chemistry is reduced to a foil against which Libavius argued by challenging the Paracelsian philosopher Oswald Croll (1560-1609). Croll's book, *Basilica Chymica* (1609), provides Paracelsian ideas most clearly and is the best example of the Paracelsian chemical pharmacopoeia. The underlying difference between Croll and Libavius was the role of language in the explanation of nature that each espoused. Paracelsus and Croll emphasized the craft of chemistry with all its laboratory complexities, while Libavius stressed the written formulation of chemistry as a discipline that could be taught. From the experience and experiment urged by Paracelsus and the methodization insisted upon by Libavius the first textbook of chemistry was written, Libavius' *Alchemia* published in Frankfurt in 1597.

Owen Hannaway has clearly and cleverly presented the spectrum of intellectual forces (Rationalism, Rosicrucianism, Calvinism, Lutheranism, and so forth) that shaped the earliest chemistry textbook. His text is well presented, succinct, and physically pleasing. Its merits make it a book to be read by historians. It will undoubtedly stimulate a new chapter of discussion and research, if not tradition, in the history of science that will make this specialty topic a notable chapter in the history textbook for this age.

AUDREY B. DAVIS
Smithsonian Institution

STEPHEN KERN. *Anatomy and Destiny: A Cultural History of the Human Body*. Indianapolis: Bobbs-Merrill Company. 1975. Pp. xii, 307. \$10.95.

This is a historical survey of some nineteenth- and early twentieth-century attitudes toward the body

and its functions. Stephen Kern was a research fellow in psychiatry while completing his dissertation in history. The impetus for the book came from his belief that Sigmund Freud's work was only part of a much broader cultural assault on the repressive sexual morality of the nineteenth century. The author attempts to combine psychological insights with the methods of the historian. The book reads well and is amply documented. It also includes considerable information not readily available elsewhere. All of this is to the good. My overall impression of the book, however, is a negative one.

My essential criticism is Kern's lack of historical background for the subjects he discusses. He assumes that many of the things he mentions are unique to the nineteenth century, and this has led him into serious errors. An example of this is Eduard von Hartmann's belief (1895) that if a mother conceived a child with a second man, the child would show the influence of the first husband. Contrary to Kern, this idea was not unique to the nineteenth century, but also circulated widely in the seventeenth century. Kern discusses the new attention paid to the olfactory impressions without realizing the whole public health and sanitation background summarized by George Rosen and others. Kern states, without realizing the conflicting and contradictory trends, that the sexual morality of the early nineteenth century was essentially that of the bourgeois. He does not understand the implications for Victorian sexual morality of the discovery of the third stage of syphilis; he confuses *coitus reservatus* with *coitus interruptus*; he ignores much of the history of bathing; and he misses the social implications of tight lacing. This list could go on.

In defense of Kern, it might well be that the difficulty does not entirely lie with him, but that his research is in a comparatively new field in which there are few guideposts and a general lack of basic monographs as well as overall views for the historian to use as points of reference. Thus each of us tends to create *de novo* rather than building upon the work of our predecessors. Kern is to be commended for his attempt. I am sad to report that he has not been notably successful.

VERN L. BULLOUGH
California State University,
Northridge

STEPHEN GARDINER. *Evolution of the House: An Introduction*. New York: Macmillan Publishing Company. 1974. Pp. xiii, 298. \$10.95.

This series of ten essays, with a conclusion, was written by Stephen Gardiner, a British architect, to show laymen readers the practical ideas behind

house development through the ages. Decidedly a present-day architect's perspective, its principal concern is with the enclosure of space. The book's primary weakness is its failure to show to any extent how ways of living influenced house layout and design, and there is virtually no mention of interior decoration, which can be more revealing than the architecture itself, especially in our own time. It would have been a better book for having done those things; but its merits as it stands largely repair the damage of omission. The author's topics are direct and to the point: "Outlines," "Solids," "The Frame," and "The Garden." All of the essays support his thesis that architecture today has ceased to be humanistic, as it was in the vernacular, and his conviction that a return is possible within the context of modern life. Except for the latter part, the thesis could have burst whole from the English Queen Anne movement of the 1870s. In addition to using photographs, Gardiner has helpfully illustrated his book, in the style of the English, with simple pencil sketches.

Evolution of the House is the best and clearest single volume on the subject available today. Readable and often very witty, it can be appreciated by students and the growing number of house enthusiasts as a guide to the shelves of more detailed monographs, which are more often than not hamstrung by their own jargon.

WILLIAM SEALE
White House Film Project,
Washington, D.C.

JOHN HIX. *The Glass House*. Cambridge, Mass.: MIT Press. 1974. Pp. 208. \$22.50.

Because glass is transparent to light but opaque to heat, glass structures can serve as a solar energy trap. This greenhouse effect, combined with inexpensiveness and beauty, has prompted widespread interest in glass as a building material among architects who, like John Hix, believe the central purpose of architecture is creating artificial environments. Efforts to exploit the unique properties of glass go back a long way, however, as Hix discovered in 1969 when he and his students at Cambridge University built an experimental glass home. Background material gathered by students as part of the exercise aroused Hix's curiosity about antecedents and led him to compile the pictures and chronological notes presented as a history of the glass house.

The book has two parts. The first concerns European attempts to provide artificial climates for exotic plants, mainly during the eighteenth and nineteenth centuries, and it concludes with a survey of the current status of horticulture under

glass. In the second part of the book, Hix turns to the glass house as habitable space. He traces the nineteenth-century development of prefabricated glass and iron structures, stressing the construction and significance of the Crystal Palace and its successors. The closing chapters deal with recent experiments in glass homes and with larger plans for glass and plastic-enclosed towns.

The character of the book is mainly defined by its illustrations, 317 of them accounting for well over half its contents. Each print, plan, and photograph is clearly reproduced, informatively captioned, and carefully documented. The text, rather less fully documented, is perhaps best understood as supplementary to the illustrations. This is no comprehensive history of glass houses, nor was this the author's intention. What Hix offers is a visual essay with commentary on some of the ways glass has been adapted to changing needs and tastes in building over the past two centuries. The result is an attractive and suggestive introduction to an unfamiliar aspect of architectural history.

BARTON C. HACKER
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PIERRE VILAR. *A History of Gold and Money, 1450-1920*. Translated by JUDITH WHITE. Atlantic Highlands, N.J.: Humanities Press. 1976. Pp. 360. \$18.50.

JOHN KENNETH GALBRAITH. *Money: Whence It Came, Where It Went*. Boston: Houghton Mifflin Company. 1975. Pp. 324. \$10.00.

Pierre Vilar's *History* is primarily a work of summary and synthesis, written for a Spanish and French audience. Newly translated into English, it partially fills a gap in our own literature by surveying the evolution of metallic monies over the span of five centuries commencing with Columbus—"partially," because it is too brief a book to wield effectively so broad a brush. But the bulk of the work develops a reinterpretation of the sixteenth century, when a flood of gold and silver streamed from the mines of Peru and the West Indies to the markets of Castile and thence throughout Europe and later into Asia. The standard work in English on that critical episode remains that of Earl Hamilton (*American Treasure and the Price Revolution in Spain, 1501-1650*), who treated the discovery and shipment of the metals as an autonomous force that dramatically altered prices and then the whole scope of economic activity in Spain.

Vilar's contribution is to emphasize the relationship of these discoveries—and monetary change in general—to antecedent conditions. In the second half of the fifteenth century the conflux of demographic expansion, technological change in agriculture and industry, the development of the large

commercial fair, and the spread of nationalism had generated an economic revolution that preceded and stimulated the quest for gold. By 1492 goods were available cheaply in Europe to anyone with gold to buy them. The voyages of Columbus, Vilar argues, were largely motivated by this economic conjecture. He concludes that in "economics as in history, there is no such thing as unilateral causation . . . : economic activity attracted gold and silver, and they in turn were an incentive to further activity" (pp. 255, 262). This conclusion serves not only as an aid to understanding the sixteenth century, but also as a lesson to those economists who claim a central role today for money as a cause of economic change.

One always hopes for the best from a new book by John Kenneth Galbraith. He is the *premier* writer among living economists, the master of the pungent epigram and the amusing anecdote. In *Money* he focuses these formidable talents on the demystification of monetary history, wrenching it from the fists of the *cognoscenti* and presenting it to the general reader. He attempts, as does Vilar, to summarize the post-Columbian development of money. He emphasizes America more than Europe, banking more than metals, and the past two centuries mainly, ending in 1974.

Galbraith, a past president of the American Economic Association, obviously holds impeccable professional credentials and should not be readily dismissed. And yet his writing here sometimes merely simplifies complex issues beyond recognition. He anticipates the criticism at the outset, informing us that "whatever errors . . . this history may contain, there are . . . none that proceed from simplification" (p. 5). But then he writes *inter alia* that "money is nothing more or less than what [the reader] always thought it was" (p. 5), that "the Federal Reserve System . . . enjoys the liturgy but not the reality of independence" (p. 30), and that the 1973-74 rise in oil prices did not contribute to inflation: "In fact, it was deflationary" (p. 299). A treasure of subtlety has been crushed beneath the weight of these assertions.

Galbraith titillates with savage and often unsupported attacks on the famous: Ludwig Erhard was "an accidental man" (p. 162); Richard Nixon "invites and justifies all possible criticism" (p. 285). He concludes his discussion of American policy with a blanket condemnation of those who have managed our money ("the task attracts a very low level of talent" [p. 302]), though his survey has not once mentioned either of the men who have dominated United States monetary policy for the past twenty-five years: William McChesney Martin and Arthur F. Burns. Elsewhere, *Money* is often fair, and it is entertaining throughout. But ulti-

mately the book is a placebo and should appeal only to those who feel they cannot digest a more serious discussion of monetary history.

JAMES BOUGHTON
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Bloomington

RAPHAEL SAMUEL, editor. *Village Life and Labour*. (History Workshop Series.) Boston: Routledge and Kegan Paul. 1975. Pp. xxi, 278. Cloth \$21.75, paper \$10.95.

This product of an experiment in alternatives to the examination system for older, working students offers samples of "people's history," based on a variety of primary source materials. On the basis of this first volume in the series, the experiment must certainly be judged successful.

The first three parts, a general essay by the editor, a study of village harvesters by David H. Morgan, and a study of country work girls by Jennie Kitteringham, are excellent. They explore the diversity of nineteenth-century rural life, the fluidity of occupational boundaries, the economic role of the family unit, the importance of cottage industries, and the seasonal migration of workers in search of jobs. The social upheavals and physical demands imposed by the urgency of harvest periods are not neglected, as is often the case, and it is also refreshing to find women and children quietly taking their place in social history without cant or cavil.

Although it explores similar themes, the last and longest part of the book, a study of a quarry village and an essay on oral history by the editor, fits less smoothly into the overall structure. It focuses on a slightly later period than the preceding essays, extending into the first two decades of the twentieth century. Given the nature of the source materials, this section might have explored some important areas in more detail than was possible in other essays. His examination of income, diet, and family relationships lacks the rigor and vitality of the earlier essays, and the fact that in this village the cottage laundry industry was admittedly as important as the brick works makes his comparative neglect of the economic roles of women and children annoying. Unfortunately, although interesting and often illuminating, Samuel's essay tends to be anecdotal rather than analytical.

Despite the occasional disappointments of the final section, this is a lively, valuable addition to nineteenth-century social studies and an essential work for those interested in rural life and work.

JANET ROEBUCK
University of New Mexico

E. J. HOBBSBAWM. *The Age of Capitalism, 1848-1875*. (History of Civilization.) New York: Charles Scribner's Sons. 1976. Pp. xv, 354. \$17.50.

E. J. Hobsbawm's *Age of Capitalism, 1848-1875* is part of an English series entitled the History of Civilization. This fact is not prominently displayed by the publishers probably for fear that, as part of a series, the book will be classed as a textbook and given but slight consideration. Such things have happened, which is a great pity, since at present some of the best historical writing, by the most competent authorities, appears in this form. This is the day of microscopic research, when most of the synthesis is provided by textbooks. Books such as the present one, reminiscent of Burckhardt's attempts at cross-section analysis and synthesis, are of great importance not only for the student but for the educated layman.

Hobsbawm's book is a superior piece of work, comparable to Robert C. Binkley's *Realism and Nationalism, 1852-1871*, published forty years ago in the Rise of Modern Europe series. As editor of this last-named series, I will be excused if I look at the two books with an eye to the changes that have taken place in historical writing over the past half century.

The most striking difference is that while Binkley dealt only with European developments, Hobsbawm takes the entire world as his province. This is as it should be in light of the rise and fall of imperialism. A second noted difference is that while Binkley dealt first with science, religion, and the arts before turning to economic developments and social problems, Hobsbawm has reversed the order. Here again his approach is probably sounder, for, as he points out, after the flare-ups of 1848-49 "the Industrial Revolution swallowed up the French Revolution." The decade following 1850 saw an unparalleled development of industry, transportation, and commerce. Hobsbawm is a confirmed Marxist, so it is easy for him to elaborate on economic developments and their consequences. I hasten to add, however, that he is not painfully Marxist.

While I admire Hobsbawm's book for the wealth of information and the logic of argument, I cannot help but feel, as a man of an earlier generation, that to really understand a period, a certain amount of systematic, chronological knowledge is desirable, if not essential. Hobsbawm tends to skip to and fro as his argument requires. Traditional events of the period are barely sketched, and the problems and policies of governments and statesmen are mostly omitted. His whole tendency is to personalize the forces of history and to categorize society even to the point where at times it seems

highly artificial. The "working classes" (whoever they may be) appear as the "helpless victims" of ruthless, brutal forces, unregulated and unchecked by governments.

Details on which I might differ from the author seem too insignificant to warrant discussion or even mention. Hobsbawm is far more competent in the field of general history than many economic historians. He never overplays his points and never loses from sight "the global triumph of capitalism" that is indeed "the major theme of history in the decades after 1848." His book is a splendid achievement, certainly the most up-to-date and comprehensive treatment of world history in a period of critical transition.

WILLIAM L. LANGER
Harvard University

THADDEUS J. TRENN, edited with commentary by. *Radioactivity and Atomic Theory*. Presenting facsimile reproduction of the *Annual Progress Reports on Radioactivity, 1904-1920, to the Chemical Society* by FREDERICK SODDY F.R.S. New York: Halsted Press. 1975. Pp. xv, 517. \$35.00.

The Chemical Society of London inaugurated the tradition of the *Annual Reports on the Progress of Chemistry* in 1904, publishing yearly volumes under that title, each of which contains reports of current problems and researches in various fields of chemistry. Radioactivity was then a new and exciting area of research, and among those in England most active in its pursuit was young Frederick Soddy who had worked with Ernest Rutherford in developing the atomic disintegration theory of radioactivity. In 1903-04 Soddy was confirming spectroscopically the production of helium from radium with William Ramsay, a noted chemist and a member of the publication committee for the *Annual Reports*. Soddy agreed to write the first report on radioactivity and continued to contribute these almost yearly until 1920. He received the Nobel Prize in chemistry in 1921 for his work, which included investigations into the origin and nature of isotopes.

The core of *Radioactivity and Atomic Theory* is a facsimile reproduction of the radioactivity reports. Complementing these are Thaddeus Trenn's tables of errata, appendixes, and bibliography, as well as subject and author indexes, the latter compiled and occasionally amended from indexes to the original volumes. This core is preceded by a forty-four-page introductory commentary with sections that direct the reader to those pages of the reports where various topics appear. The introduction indicates problems and theoretical de-

velopments linked with the designated topics. Its references are keyed to the original pagination and volume numbers of the *Annual Reports*, and these appear in addition to the regular pagination used throughout Trenn's entire text.

The major deficiency of the book is its failure to provide a modern historical commentary on radioactivity researches during the years of Soddy's reports. An eight-page "historical overview" in the introductory commentary is a reproduction in facing German and English of a 1923 resumé by F. A. Paneth and György Hevesy. A "biographical account" of Soddy is a short extract from *Les Prix Nobel en 1921-1922* (Stockholm, 1923). Although there are allusions in the introduction to the "interaction of experiment and theory," "theory-building," "experimental anomalies," and so forth, Trenn has clearly intended this volume as a source book and no more. The failure to provide any historical interpretation is in my opinion a shortcoming of the volume, and it is particularly disappointing since Trenn is eminently qualified to write a historical introduction.

MARY JO NYE
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STEPHEN R. WARD, editor. *The War Generation: Veterans of the First World War*. (National University Publications: Series in American Studies.) Port Washington, N.Y.: Kennikat Press. 1975. Pp. 192. \$12.50.

The purpose of this slim volume of five essays is to analyze the activities of World War I veterans in five nations: Great Britain, France, Germany, Italy, and the United States. A particular concern has been to discover whether, as some suggest, military experience creates nostalgia for the "military way," leading veterans to favor rightist ideologies. These essays argue against such a broad generalization. In his introduction Ward points out that in each of the four European countries studied, some veterans supported leftist causes. As for American veterans, Ward might have given attention to an earlier study (Rodney G. Minott, *Peerless Patriots: Organized Veterans and the Spirit of Americanism* [1962]), which maintains that enthusiasm for jingoistic nationalism could well have resulted from the control over organizational policy by "professional veterans" at national headquarters, rather than from Fascist tendencies derived from authoritarian army experience.

The editor finds similarities and differences in each of the countries. One obvious similarity is the importance of persuasive leaders, all of whom "used similar rhetoric, albeit expounding different political views." Where Mosley, Barbusse, and

D'Annunzio failed, Mussolini and Hitler enjoyed spectacular success. (Michael A. Ledeen's chapter leads one to wonder, however, what might have taken place in Italy had D'Annunzio shown more decisiveness in leading the non-Fascist *arditi* veterans, and had he not fallen or been pushed from that window at a crucial moment in the developing national crisis.) Another similarity was the tendency of veterans to take part in disruptive incidents "either initiated on their own behalf or joined by them in support of other causes." James M. Diehl shows how rightist veteran organizations and combat leagues contributed importantly to the collapse of the Weimar Republic; Robert Soucy points out that many veterans were involved in the Paris riots leading to Daladier's resignation; Donald J. Lisio emphasizes the serious political damage that Hoover suffered for his mishandling of the most important instance of American veteran activism, particularly for his blind acceptance of MacArthur's subsequently disproved "communist-threat" excuse for driving out the bonus marchers in defiance of the president's instructions. The tendency among a number of associations to divide according to ideological preferences was another similarity in the three Continental nations. In sum, Ward finds the differences to be more subtle than the similarities. Moreover, he emphasizes the much greater radicalism in Italy and Germany than in Great Britain and the United States.

The five authors, perhaps, should have expanded their essays. Within the limitations of space, however, the results are satisfactory; all five essays are well documented and eminently readable. And one must agree that in its purpose the collection is, as the dust jacket boasts, unique.

MARY R. DEARING
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MARTIN JAY. *The Dialectical Imagination: A History of the Frankfurt School and the Institute of Social Research, 1923-1950*. Boston: Little, Brown. 1973. Pp. xxi, 382. Cloth \$12.50, paper \$3.95.

For Marxists, and perhaps everybody, the central fact of twentieth-century life is the range of strategies by which capitalism has evaded the sentence of doom pronounced upon it by Karl Marx. The administered welfare state and fascism are two of the most ingenious. Many observers would add Soviet socialism. In the process, the revolutionary proletariat has been assimilated by bribes and behavioral engineering into a new mass culture that at best only parodies Marx's hopes for a liberated humanity, and at worst surpasses the hellscapes of Yevgeny Zamyatin and George Orwell.

In *The Dialectical Imagination*, Martin Jay supplies a lucid history of a movement in modern radical social philosophy that has confronted Marx's prognostic shortcomings with an uncommon mixture of sympathy, irony, and openness to both pre- and post-Marxist thought. The Frankfurt School, and the *Institut für Sozialforschung* out of which it grew, developed in its prime a multidisciplinary critique of modern civilization that ranged from the loftiest peaks of speculative philosophy through esthetic theory and *Geistesgeschichte* to inspired works of empirical sociology. Its inner circle, the true dialecticians, consisted preeminently of Max Horkheimer, Theodor Adorno, and Herbert Marcuse. The middle and outer circles included such remarkable scholars as Walter Benjamin, Erich Fromm, Franz Neumann, and Karl August Wittfogel.

Jay's book, based on his Harvard dissertation for H. Stuart Hughes, is one of the few obviously major works in intellectual history by a younger scholar to appear in this decade. His eight well-wrought chapters follow the history of the Frankfurt movement from the early Weimar years to the triumphant return to Germany in 1950. Along the way are detailed analyses of its critical theory, psychosocial research, dissection of mass culture, and philosophy of history.

The greatest single curse of Frankfurt social philosophy was no doubt the denseness of its prose. This becomes a manageable problem in *The Dialectical Imagination*. With little sacrifice of the rich and deliberate ambiguities issuing from the school's dialectical method, Jay translates most of its Hegelian-Marxian-Freudian jargon into good academic English, with a thoroughness that should leave only the most inattentive reader still at sea. The author earns our thanks. It remains unhappily true that in its struggle not to be crude, vulgar, and simplistic, the Frankfurt School too often ended by being obscure, elitist, and labyrinthine.

The difficulty of the school's language, of course, stemmed in part from the difficulty of its concepts. If many scholars, especially outside the German-speaking world, were long unable or unwilling to negotiate the heights of abstraction scaled by Horkheimer and his associates, this was a price that most of them felt obliged to pay. They saw their mission as essentially critical: to break through the crystalline masses of Marxist dogma encrusting the modern radical mind, and with the help of dialectical reason to promote freewheeling modes of thought that would explain the tragic deviations of social democracy from the course plotted a century ago by Marx. The task required avoidance of two equally dangerous snares, that of irrationalist reaction, on the one hand, and of

bourgeois positivism on the other. The effort to stay clear of these familiar traps, and also to breathe new life into an increasingly dogmatic and politically manipulated Marxism, left the Frankfurt School in an isolated forward position out of touch with nearly all segments of the activist intelligentsia. The need to unite theory and *praxis* could not be met, Jay notes, except on the doubtful understanding that theorizing itself might constitute action, "the only form of *praxis* still open to honest men."

In the end, as Jay suggests and as his mentor Hughes concludes in *The Sea Change*, the Frankfurt School leaned perceptibly closer to its sources in both early and late German romanticism than to the ossified Marxism of the parties or to the various schools of Western bourgeois thought. Its recent offshoot, the countercultural utopianism of Marcuse, which falls outside the time frame of Jay's book, was an authentic harking-back to some of the deepest hopes of romantic idealism. In any event, what the Frankfurt School set out to accomplish could not be done without strenuous intellectual and imaginative enterprise, in a sphere necessarily far removed from the exigencies of mass politics, and no less remote from the conventions of modern social science.

It is regrettable that Jay did not carry his history down to 1970, something he could easily have managed in two or three more chapters. But what he has written is superb. Sensitive to nuances of culture and personality, firmly grounded in the sources of modern intellectual history, and fortified by meticulous research that included numerous interviews and exchanges of letters, his work will stand for years. Jay approaches his thinkers with all due respect, but also with the "critical edge" they prized so much themselves. They could scarcely have found a better interpreter.

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ROBERT A. ISAAK. *Individuals and World Politics*. North Scituate, Mass.: Duxbury Press. 1975. Pp. xiv, 322.

This interesting "primer on world politics" finds that most thoughtful approaches to international relations focus either on the nation-state or on global systems. Asserting that both are necessarily abstract and stimulate a belief in man's powerlessness, this account affiliates itself with another major view, "the idealistic humanistic" one (p. 17). Choosing eight individuals who have prominently affected world politics in this century (Gandhi, Lenin, Mao, Woodrow Wilson, Hitler, de

Gaulle, Hammarskjöld, and Kissinger), the author presents for each a political biography of a dozen or more pages, immediately followed by about half a dozen pages on principles of world politics and several more on present implications. Into these various sections are infiltrated photographic portraits and a host of semi-isolated paragraphs that contain related comments, quotations, or definitions.

The author very effectively conveys the essence of foreign policy in the historic and contemporary state systems. He gives high marks to Kissinger as a scholar, as a pragmatic negotiator, and as the United States' greatest secretary of state. We hear of his devotion to the principle of indeterminacy, his weakness in economics, and his adoption of a blend of Metternich's emphases on balance and legitimacy and Bismarck's world view and flexibility. Despite the impressive skills of men like Kissinger, the author sees a future of "recession, depression, terrorism, starvation and war," a time when "the gap between developed and undeveloping countries will widen to ghastly proportions" (p. 257).

There are some disconcerting slips, like a reference to Mao's birth in the "village of Changsha." (Changsha is a city, and Mao was born elsewhere.) Wilson is set in opposition to Hitler: one is presented as a pathological idealist; the other, a pathological realist. Each was presumably characterized by extreme rigidity and dogmatism, characteristics that fit Wilson consistently only when he was left an invalid by his stroke in 1919.

The writing at its best is clear, interesting, and careful in defining terms. At its worst, it runs to jumbled figures of speech: "They knew how to make their political dreams into flesh by seizing the moment that was historically and psychologically ripe" (p. 263).

The test of a textbook lies in its meaningful use by students. If international relations can be taught in the present chaos of American urban high schools, this book will serve well; it is also appropriate for community colleges and for university courses at the freshmen level. It is not really a research effort, but it does move us a few steps toward a new mode of political realism.

EDWARD GULICK
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ANDREW W. CORDIER and WILDER FOOTE, selected and edited with commentary by. *Public Papers of the Secretaries-General of the United Nations. Volume 4. Dag Hammarskjöld, 1958-1960.* New York: Columbia University Press. 1974. Pp. xiv, 659. \$22.50.

ANDREW W. CORDIER and WILDER FOOTE, selected and edited with commentary by. *Public Papers of the*

Secretaries-General of the United Nations. Volume 5. Dag Hammarskjöld, 1960-1961. New York: Columbia University Press. 1975. Pp. xv, 592. \$27.50.

These two volumes, which are in a series intended by the editors to include texts that are "essential or most likely to be valuable to historical research" (vol. 1, p. vi), cover an important part of the period when the United Nations and the office of secretary-general under Dag Hammarskjöld achieved their greatest influence and prestige. High hopes were only to be dashed on the rocks of great-power rivalries.

These two volumes contain important texts not included in the official records of the United Nations. Among these are addresses of the secretary-general to groups outside the United Nations, statements to the press, and press conference transcripts. In the case of Hammarskjöld, the press conference transcripts are particularly valuable for illuminating his conception of the role of the United Nations and of the office he occupied. While these materials are available in various places, it is a great convenience to have them collected in one publication.

There is less value in reproducing texts from the official records because these are more likely concerned with specific UN activities, and the significance of particular reports or interventions by the secretary-general cannot be fully appreciated except in the context of what precedes or follows. Thus, the researcher must go to the official records of the Security Council or to those of the General Assembly if the secretary-general's statement was made in response to a criticism of his conduct or as an explanation of his execution of an Assembly resolution. To some extent the necessary context is provided by the commentary of the editors, but this can be adequate only for general informational purposes.

The commentaries, however, serve another purpose. Since Andrew W. Cordier was intimately involved in the work of the UN as executive assistant to the secretary-general and occasionally was his personal representative, the commentaries are useful for establishing the sequence of events and the motives and purposes of the principal actors. The editors' account of Cordier's closing of the Congolese airports and the radio station at Leopoldville in early September 1960 can be compared with that given by Brian Urquhart in his *Hammarskjöld* (1972). The importance of this action cannot be overemphasized because one consequence was the Soviet attack upon Hammarskjöld that threatened to destroy the usefulness of the office of the secretary-general. Urquhart reports that Hammarskjöld was "taken aback" when he heard of Cordier's action and implies that he would not

have approved such drastic action if he had been consulted in advance, though he subsequently gave full support to Cordier and took responsibility for his action in the Security Council and the General Assembly. The editors' commentary strongly defends Cordier's action as consistent with Hammarskjöld's instructions and fully justified by the situation in the Congo. Unfortunately, Cordier died before he was able to complete and publish a more detailed justification of his action.

The two volumes under review, together with volumes 2 and 3 covering the years 1953-58, make a valuable contribution to our understanding of the role of the United Nations and the part played by an imaginative, resourceful, and diplomatically skilled secretary-general in dealing with important issues of the time. They are probably most valuable for helping us understand Hammarskjöld's conception of the role of the United Nations and the part the secretary-general should play. Since one of the editors was at the same time an important and active participant in the action, the commentary of the editors assumes special importance.

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SAUL FRIEDLÄNDER. *Histoire et psychoanalyse: Essai sur les possibilités et les limites de la psychohistoire*. (L'Univers Historique.) Paris: Editions du Seuil. 1975. Pp. 233.

This work by one of the leading European psychohistorians is particularly welcome because it presents for the first time a coherent summary of the extant literature of psychohistory and particularly the current European contributions to this growing field. Friedländer attempts to assess what psychoanalysis can and cannot do for the historian, and what the psychohistorian should and should not research.

For Friedländer psychoanalysis is the psychological theory of choice for the historian because it is a historical theory of development, enabling history and psychoanalysis to pursue parallel and integrated lines of inquiry. Both disciplines always return to their own mode of validation. Psychoanalysis deals with time processes; it is evolutionary in the same way that history is. Psychoanalysis is also concerned with the complexity of emotions and motivations, while behavioral psychology is not, making psychoanalysis applicable to a wider range of complex historical phenomena than any other theory.

Among the areas where the field of psychohistorical inquiry lies open are the structure of the family, the problem of creativity in biography,

deviant patterns within a society, the formation of temporary groups, the choice of leaders, the personality changes that people undergo in group formation, and the deep symbolic structures of myth, conflict, and language in cultures.

There are also areas that Friedländer would exclude from the psychohistorical domain of study, such as social change, internalization of norms (object relations), and adaptation (ego psychology). This is the least arguable point in Friedländer's prescription for the future of psychohistory. If psychoanalytic theory is of value to historians, then its two current clinical developments, ego psychology and object relations theory, must also be assimilated and utilized by psychohistorians. Since history is in fact concerned with both continuity and change, and one element of both forces is internalization, the psychological how, when, and why of introjections and identifications are of capital importance to historians. It is times of personal and historical crisis that reveal most about underlying structures of tension and conflict. There has never been a time in history where social change was absent. It is a matter of assessing the strength and tempo of the impulses and resistances at work, and for this an understanding of ego energies of growth, defense, and adaptation is essential. While Friedländer's self-limitation upon psychohistory may in other cases be commendable, in the instance of ego psychology and object relations he forfeits current psychoanalytic approaches of great potential value to historical studies.

Friedländer holds that a psychohistorian, in order to work effectively, should be psychoanalyzed himself. The unanalyzed historian risks superficiality and mechanical application of psychoanalysis to history because he has a lower consciousness of his own intellectual processes. But, says Friedländer, this raises a paradoxical difficulty because psychoanalysis is a process that profoundly engages the whole person emotionally. This may mean an inability of the psychohistorian to exercise a critical spirit toward psychoanalysis. About this apparent paradox two things may be said. The first is that all historians have methodological and personal unconscious predispositions that cloud their perceptions in certain areas. The second is that psychoanalysis presumes to work through major emotional resistances to insight in an analysand, so that he will be particularly aware of his propensities to distort or overlook evidence or to become so emotionally involved with a given position that he can see nothing else.

Friedländer's notes are judicious and the bibliography of this rapidly growing field is excellent. The work is a must for historians who wish to keep abreast of methodological developments in psycho-

history and of current historical thought in the world.

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EDGAR WESLEY OWEN. *Trek of the Oil Finders: A History of Exploration for Petroleum*. (Semicentennial Commemorative Volume, memoir 6.) Tulsa: The American Association of Petroleum Geologists. 1975. Pp. xv, 1,647.

This book is one of three volumes authorized in 1963 by the executive committee of the American Association of Petroleum Geologists to commemorate the association's semicentennial. From the point of view of content and historical workmanship, Edgar Owen has produced a monumental book. It is a stout volume with 1,592 pages of text and double columns of printing on every page. The book is dedicated to Everette Lee DeGolyer, one of America's distinguished geologists and oil company executives. The emphasis is upon the evolution of petroleum geology as a science, theories as to where petroleum might be found, the establishment of geology departments by oil companies, the use of geology and geophysics in finding oil fields all over the world, the men who emerged as petroleum geologists and their particular contribution to the new and developing science in the United States and abroad, and the events from ancient times to the present that led to the finding of new oil fields. The author's knowledge of petroleum geology and petroleum geologists and his grasp of the literature is masterful. There are many data on the oil industry apart from geology—the history of different oil fields, leasing, drilling practices, the formation of oil companies that grew into major companies, oil company operations, the rise of international oil industry, conservation, the nationalization of the oil industry abroad, and the corporate structure of the petroleum industry. Except for the introduction written by Wallace E. Pratt, the chapter on Latin America by J. Herbert Sawyer, and brief sections on the different countries in Africa, the author has written the entire book. It is comprehensive in scope; there are twenty-four chapters; sixteen are about petroleum geology and the role of petroleum geologists in the United States; the remaining chapters are about oil fields in foreign countries.

The book is based on an enormous amount of research in oil company histories, articles on oil, mining and geological journals, the writings of petroleum geologists, state and foreign geological surveys, the U.S. Geological Surveys, special reports of petroleum geologists, old and rare books on petroleum, state and federal documents, plus

material that was derived from the author's "own business relationships, committee assignments and informal contacts during 57 years of professional work as a petroleum geologist, landman and participant" in some producing operations. The text is extensively documented, and at the end of each chapter there is a lengthy list of references cited in the text. Scattered throughout the book are many quotations from original sources. There are many maps of oil and gas fields as well as a subject, proper name, and geographical index. The book will serve as an invaluable reference work on the history of exploration for petroleum all over the world, petroleum geology, and petroleum geologists.

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EDWARD SHORTER. *The Making of the Modern Family*. New York: Basic Books. 1975. Pp. 369. \$15.00.

CHARLES E. ROSENBERG, editor. *The Family in History*. (Haney Foundation Series, 17. The First Stephen Allen Kaplan Memorial Symposium in Social History.) Philadelphia: University of Pennsylvania Press. 1975. Pp. 210. \$10.00.

Over a decade ago Phillipe Ariès asserted that the nuclear family became the chief focus of sentiment, at the expense of extended family and community, only in the early modern period. Some historians are still influenced by this hypothesis, but there is growing evidence against it. Diane Hughes, for example, in her excellent contribution to the Rosenberg book, has shown that in medieval Genoa the nuclear family was the sentimental focus for artisans, though not for aristocrats.

Edward Shorter writes with charm about this subject, and many of his suggestions are plausible. He tends, however, to insinuate his case rather than to build it systematically. What we are offered is a provocative proposition in the guise of a serious proposal. I wish that Shorter had taken longer to collect his evidence.

Shorter's main thesis is that there was a great psychological "withdrawal" of the nuclear family from the surrounding community among the middle and lower classes of Europe after 1750. In support of this thesis he presents little direct evidence; what he does present is mixed with evidence to the contrary. He does not point to a particular community and show which families withdrew, to what extent, and when. Without having demonstrated that a significant change occurred, he jumps to an explanation of how it occurred. Shorter attributes the "withdrawal" of the nuclear family to a "revolution in sentiment." This revolution, he contends, was manifest in a more

affectionate relationship between husband and wife, who tended to marry for love, not money and offspring. "Popular marriage in former centuries was usually affectionless," he declares, using anecdotal evidence. An equal number of contrary anecdotes could be cited (for example, in Richard Gough, *History of Myddle* [1700]). Shorter has neither proven that marriage "for love" is more modern than traditional, nor that marriage "for convenience" is more traditional than modern.

In dealing with the modern period, Shorter convincingly describes declining community surveillance over courtship and marriage, permitting increased sexual freedom. Having presented statistics on the increase in illegitimate offspring born to urban working women, he suggests that these women "wished to be free" and were seeking "individual self-fulfillment." Scott and Tilly, in their contribution to the Rosenberg volume, show that such women were not liberated, nor did they wish to be: they just wanted to get married. Shorter, by inferring the motivation for sexual intercourse from one of its consequences, has committed a "phallacy."

Turning from the man-woman relationship to the mother-child relationship, Shorter sees a "revolution in sentiment" in the form of improved mothering. "Good mothering," he declares, "is an invention of modernization." His evidence is mainly that infanticide was widely practiced in early modern, but not industrial, Europe, and that the practice of giving an infant to a wet nurse in the countryside tended to disappear after 1750. No doubt there were shocking cases of child neglect and abuse in early modern Europe, as Shorter points out, but currently one million such cases are reported in the United States each year. No doubt the abandonment of newborn infants was widespread in early modern Europe, but how does the number of abandoned infants then compare with the number of unborn aborted now? No doubt there was a high mortality rate among infants given over to rural wet nurses, but how did this rate compare with that of infants reared in disease-ridden towns and fed with unsterile animal milk? When working women began to care for their own infants, around 1900, it was not because of a revolution in sentiment but in biological science.

Lawrence Stone, in his well-organized contribution to the Rosenberg book, has divided the history of the English family in the early modern period into two phases. He says that in the first, the sixteenth and early seventeenth centuries, the nuclear family became more patriarchal; in the second, the late seventeenth and eighteenth centuries, it became more affectionate. Considering only the first phase, Stone reports that the authority of the father was strengthened by laws, the legal rights of

wives were reduced, and children were subjected to harsh physical discipline. Stone thinks that this rather unattractive kind of nuclear family "rose" while the extended family declined.

His evidence, however, is not quite convincing. The fact that the state took over many clan functions does not tell us anything about how people in the clan felt about each other. Stone has not pointed out a particular clan and shown that nuclear families withdrew from it, to what extent, and when. His method is to cite prescriptive sources, such as laws and domestic advice books, illustrating them with anecdotes from actual family life. Illustration is not proof: for every anecdote about a patriarchal marriage in early modern England, one could find an anecdote about an egalitarian or matriarchal marriage. Stone's article is nevertheless worth reading.

In other contributions to the Rosenberg book, Wolfram Eberhard competently surveys upper-class Chinese family history; David Landes compares two Jewish financial families, but fails to enter into the psychosocial interior of either; and Michael Zuckerman takes Dr. Benjamin Spock to task for neglecting to build character and conscience in children.

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RUDOLF A. MAKKEEL. *Dilthey: Philosopher of the Human Studies*. Princeton: Princeton University Press. 1975. Pp. xiv, 456. Cloth \$20.00, paper \$9.50.

Dilthey's importance in the theory and history of literature has been widely acknowledged, but his contributions to the social sciences and psychology have been insufficiently explored. This situation is caused in part by his image as a humanist who was not interested in historical explanation, but also because of the dominant position of Max Weber as the theorist of a humanistic sociology. As a result, hardly any of Dilthey's writings have been translated into English. In this study Rudolf A. Makkeel points out some interesting aspects of Dilthey's approach to history. His focus is on Dilthey's psychology and esthetics, which are intimately linked with and which influenced his conception of the study of history. Arguing that, in contrast to the prevalent view, no break exists between Dilthey's earlier psychological and his later hermeneutic works, the author demonstrates the continuity in Dilthey's thought on the basis of his writings on esthetics. Literature was, in fact, Dilthey's test discipline for the human sciences. The link between esthetics and history is Dilthey's conception of the imagination: the poet and the

historian both attempted to bring out the typical in their treatments of reality. Their imagination was not arbitrary or fantastic but reality-bound.

The author characterizes Dilthey's goal in devising the human sciences as an attempt to develop an "aesthetic of history" that would fulfill the same function for the study of history as Kant's transcendental esthetic had for that of nature. In supporting his thesis, Makkreel utilizes Dilthey's writings on psychology, esthetics, and history, and he gives an insight into Dilthey's thought as a whole. In his reinterpretation Makkreel dispels some common misunderstandings, such as the notion that Dilthey's treatment of history is psychological or that empathy was the dominant conception in his approach to the past. Moreover, Makkreel does not consider Dilthey as a precursor or follower of other thinkers (Husserl, Heidegger, Kant, Hegel), but as a philosopher in his own right whose esthetics of history is relevant for the present. (In the last chapters the author gives some indications of his own ideas about an esthetics of history.)

Makkreel treats Dilthey's philosophy from a purely internalistic point of view: no reference is made to his position in society, his political opinions, or the ideological function his works might have had. Pointing out similarities and differences of Dilthey's thought in relation to other philosophers, and, to a lesser extent, psychologists, Makkreel does not include social scientists in his reflections.

The author presents his views by examining Dilthey's writings chronologically rather than thematically. By analyzing Dilthey's shifting views of such topics as psychology, esthetics, and history, which are evident in the various stages of his thought, Makkreel has made it somewhat difficult for the reader to follow his main line of argument. He compares Dilthey's thought with that of other thinkers of the period and with contemporaries' comments on topics Dilthey touched upon. These observations, however illuminating in themselves, further complicate the book.

Makkreel's study is nevertheless an important contribution to our understanding of Dilthey and the critical philosophy of history.

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FREDERICK M. SALLAGAR. *The Road to Total War*. New York: Van Nostrand Reinhold. 1975. Pp. xi, 197. \$11.95.

This book, originally a study written under the sponsorship of the RAND Corporation for the

United States Department of the Air Force, sets out to solve a theoretical problem in a real way, or a real problem theoretically. One can reasonably start at either end, for the thesis running through it is that total or all-out war is both by definition and fact of World War II origin. The attempt to give total war a dictionary definition and then to correlate warfare to it is itself a theoretical undertaking without any empirical value. It is, realistically, of no effect, except to condemn or condone real warfare situations.

Quite apart from that, this book confines itself almost exclusively to air warfare, fitting total or all-out war to that branch, and more particularly to its bomber subarm as it was employed in World War II. This is indeed a very serious weakness from the historical point of view, for, lacking precise definition, all-out or total war has not been unknown before. Not even the 1975-76 civil war in Lebanon, which did not have a single strategic or area bomber taking part, can be written off as being totally without an all-out or total war property. This book also fails to convince students of the art, science, and sociology of war and warfare that all-out or total war is synonymous with "indiscriminate" and "unrestrained," words the author uses repeatedly and perhaps even a bit indiscriminately.

To say, as the author does (p. 136), that the British wartime prime minister and Germany's chancellor had much in common in principle on the purpose and quality of war in general and the specific causes and purposes of World War II in particular seems not only unwarranted, but unnecessary and polemically distasteful as well. The apposition is placed this way: "Churchill and Hitler, although they held unprecedented power in their respective countries were also the victims . . . of events they could not control." As to what accounts for the author's, and the RAND Corporation's, neglect of President Roosevelt, I cannot say. The road to total or all-out war (which does not have its origin in the late war or in strategic area bombing as such)—that is, the worldwide scale of that war and the profound depth of the issues involved—can be historically as honorably linked to the president of the United States as to Churchill.

As near as I can judge from the bibliography, all twenty-eight titles listed are secondary works. The truly significant ones, both for the history of the war in all its facets, stages, and decisions and for interpretation or, better still, understanding, are the official histories of both the British and American governments. What is, therefore, wrong with this book originates in inadequate sources from which weak inferences are drawn.

In sum, deducing consequences from view-

points, despite a reliance on some select events and statesmen's statements, is bound to lead to meager results.

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ANCIENT

SARAH B. POMEROY. *Goddesses, Whores, Wives, and Slaves: Women in Classical Antiquity*. New York: Schocken Books. 1975. Pp. xiii, 265. \$8.95.

Despite the title, a marketing gimmick of the publisher, there is nothing low-brow about this work. Using evidence from legal transactions, inscriptions, papyri, pottery, literature, and historical records, as well as insights drawn from sociological, psychoanalytical, and quantitative approaches to history, Sarah Pomeroy has fulfilled the need for an up-to-date, carefully documented reference work and textbook on women in antiquity.

The author is sympathetic to women in a variety of societal situations. While the book begins with Greek goddesses and ends with the cult of Isis of Rome, it focuses on the private and public lives of Greek and Roman women. In fact, the goddesses themselves are viewed as married and single female members of the godly social structure. While Pomeroy cautiously avoids making an overall thesis, certain generalizations pervade the book. The independence of women during Sparta's wars, the Peloponnesian War, and the Second Punic War supports an assertion often made for modern history that women benefit during war. Likewise, unmarried goddesses, vestal virgins, and Roman widows indicate that "the most liberated females are those who are not bound to males in a permanent relationship" (p. 213). However, permanent relationships also receive recognition: of ancient Athenians, "we know of some courtesans who attempted to live as respectable wives, while we know of no citizen wives who wished to be courtesans" (p. 92). The relative happiness of the various groups she considers beyond a historian's knowledge, and the author reserves her feminist stand for the most striking of issues, the practice of female infanticide.

Pomeroy distrusts the use of literature to proclaim a prehistorical matriarchy or to provide evidence for powerful, unsecluded women in Hellenic Athens. Growth of upper-class, female economic and political power she sees as a feature of Hellenistic and of later Roman society. Her concern for understanding the lives of lower-class women is fulfilled best in her study of Roman slaves and workers. She testifies to the desire of freed men and women to purchase children born in bondage and

to the inequality of the Roman charity system—the much-praised dole was given only to men.

The greatest strength of the book is the clear presentation of the extant data and the Socratic avowal of areas of collective historical ignorance.

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JOSEPH VOGT. *Ancient Slavery and the Ideal of Man*. Translated by THOMAS WIEDEMANN. Cambridge, Mass.: Harvard University Press. 1975. Pp. x, 227. \$12.50.

Joseph Vogt has been interested in ancient slavery for many years. Under the auspices of the Mainz Academy he has done valuable work organizing studies in this subject. This volume presents an English translation of his collected essays, published previously in Germany under the title *Sklaverei und Humanität* (1965).

The edifying title of the book may ensure its sale in this country, but the quality of the essays is disappointing. Vogt seems to take for granted that Greeks and Romans became less tolerant of slavery as time went on and that philosophic and religious teaching must have affected their general attitude toward slavery. This is surprising, for he is well aware that neither Stoicism nor early Christianity sought to abolish it. In fact, Greek and Roman literature shows that the institution of slavery was accepted as a matter of course. When Vogt says that Aristotle justified slavery, he is misrepresenting what Aristotle said, and anyone who has read and understood the first book of *Politics* would recognize the misrepresentation. He is also misrepresenting Euripides when he uses passages from his plays as evidence that people in the fifth century were becoming increasingly sensitive to the evils of slavery. There are similar passages in Homer. Vogt does not seem to realize that while Athenians in the theater might have been thinking of their own kindred who suffered enslavement when they fell into enemy hands, they might not have been inclined to extend their sympathies toward non-Greek slaves in their own service. Many an Athenian, indeed, might have thought that Thracians or Phrygians should have been grateful for the privilege of living as a slave in Athens instead of in the "underworld" of their own country where true freedom was unknown.

Vogt has written his first essay, "Slavery and the Ideal of Man in Classical Greece," without taking into account the information found in the Attic orators, and this essay, like that which follows ("Slavery in Greek Utopias"), shows a curious lack of imagination. The Greek attitude toward slavery was conditioned by the exclusive character

of the Greek city state, and Greek philosophers were much more concerned with the good of citizens of their ideal states than with applying principles of justice to their slaves. Instead of offering essays on slavery in Hellenistic times, Vogt moves on to discuss the slave wars of the late Roman Republic. Here, too, when he asks why serious rebellions occurred only in certain places and at certain times, he seems more concerned with grievances than with the conditions that made united action by slaves possible. His belief that the appeal of Eunus was partly national, that he tried to present himself as a kind of Seleucid king, will not convince many readers. Other essays dealing with Roman slavery are well written and present some of the abundant evidence that Romans were often kind and considerate to their slaves, though it is surprising to find so little attention paid to Cicero's letters.

The student of ancient history will find interesting material in this book, but he should be warned against uncritical acceptance of its conclusions.

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JEREMY L. TOBEY. *The History of Ideas: A Bibliographical Introduction*. Volume 1, *Classical Antiquity*. Santa Barbara: Clio Press. 1975. Pp. xii, 211. \$17.25.

The present volume on classical antiquity is to be followed by three others, bringing the bibliographical introduction down to the present. The ideas to be considered are defined as those "used by educated people in each era to comprehend the universe and rationalize their social institutions, arts, and religion." Such ideas are dealt with under four main headings, to each of which a chapter is assigned: philosophy, science, esthetics, and religious thought in antiquity. These are preceded by a twenty-page introduction to the work in general and to this volume in particular; they are followed by a postscript, summarizing conclusions and pointing ahead to the next volume. There is also an author index to the modern literature and a table of contents sufficiently detailed for ready reference to the topics treated. The book is well arranged and clearly presented, and it should be useful to the intelligent layman for whom it is intended.

The author makes no pretense of neutrality: he is an adherent of the views of Arthur Lovejoy. He writes that "the works to be discussed are of two kinds: those that are recommended and others, well-known, that have a faulty approach to the history of ideas." But the discussion is perfunctory. A basic assumption is that the Greeks

were the intellectual force in antiquity, eclipsing the Babylonians and Egyptians who preceded and the Romans who followed them. This explains the lack of attention to Rome, especially republican Rome, and the emphasis on science and philosophy, while the humanitarian influence of Roman law is ignored.

There are serious omissions even within Tobey's framework. For example, a writer like Xenophon is simply ignored, yet he was universally read; while not an original thinker he is perhaps the best representative of the varied intellectual interests of the age in which he lived. Another strange omission concerns the field of geography. From an interest in geography came the debates over the relationship between man and his environment as well as more specifically oriented questions such as the rise of the Nile or the theory about the zones. The treatment of mathematics is excellent, however, and Tobey deals admirably with medicine. Space prevents further discussion here of religion and esthetics.

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Los Angeles

HANS DELBRÜCK. *History of the Art of War within the Framework of Political History*. Volume 1, *Antiquity*. Translated from the German by WALTER J. RENFROE, JR. (Contributions in Military History, number 9.) Westport, Conn.: Greenwood Press. 1975. Pp. 604. \$25.00.

This English version of the first volume, published in 1900, of Hans Delbrück's grandiose effort to trace the strand of war through the web of universal history is based on the third revised edition of 1920. It is a conscientious, intelligent rendering that sticks closely to the German, but at the price of reading like a translation. The ease, elegant precision, and irony that distinguish the original are almost entirely missing. More serious is the absence of an introduction providing the historiographical context for Delbrück's criticism of classical authors and for his efforts to reconstruct the battles and military institutions of antiquity on the basis of objective criteria—*Sachkritik*. More than five decades have passed since Delbrück's last revisions; in the intervening years our understanding of Greek and Roman history has deepened, many of Delbrück's hypotheses have gained acceptance, and others have been disproved. To pass in silence over the state of classical studies that stimulated Delbrück's work, and to say nothing about the sizable literature that followed its appearance, is to do a disservice to the modern reader. If a new introduction was not considered necessary, the publishers might at least have in-

cluded a translation of Karl Christ's excellent reappraisal that precedes the text of the fourth German edition of 1964. The remaining volumes of Delbrück's *History* should be treated differently.

The continuing importance of this work lies less in its numerous reinterpretations than in the unusual, composite approach that led to them. From conditions of his own time, and from episodes of the early and later modern periods for which exhaustive documentation exists, Delbrück drew criteria—march speeds, logistic requirements, technical factors, demographic relationships—that permitted him to sort out the possible from the impossible in the classical tradition, and from which he interpreted the development of military institutions and strategic thought in antiquity. The dangers of schematic, rationalist explanations inherent in his extrapolations, comparisons, and quantification were usually avoided by the historicist outlook he brought to his work. Delbrück claimed scientific character for his methodology, and in some respects rightly so, but it was the humanistic scholarship and esthetic standards through which he filtered the findings of *Sachkritik* that gave his writings a value which justifies translation even at this late date.

PETER PARET
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SIGNE ISAGER and MOGENS HERMAN HANSEN. *Aspects of Athenian Society in the Fourth Century B.C.: A Historical Introduction to and Commentary on the Paragraphe Speeches and the Speech Against Dionysodorus in the Corpus Demosthenicum (XXXII-XXXVIII and LVI)*. Translated by JUDITH HSIANG ROSENMEIER. (Odense University Classical Studies, volume 5.) Odense: Odense University Press. 1975. Pp. 270.

The aspects of Athenian society spoken of in the title consist of treatments of Athenian population, imports and exports, maritime trade and loans, banking, mining, and the judicial process and procedure in private cases, with a detailed consideration of *paragraphe* and *diarmartyria*, formal objections to bringing a case to trial (pp. 11-137). The authors include a commentary and suggested dating for eight Demosthenic speeches. They were chosen because the special plea, *paragraphe*, plays a part in them and because they concern the foreign trade of Athens that was a large (but relatively unnoticed) part of Athenian life (pp. 138-213).

The two parts are treated almost independently; there are few cross references. However, the subjects generally covered in the first part give the reader the setting for the situations involving the legal issues of the second.

The authors present completely and concisely,

with documentation, the pertinent factual data for the areas discussed. The interpretations of previous scholars are clearly reported and evaluated in a soundly critical manner. The whole could admirably serve as a syllabus for a student who wished to gain a full understanding of what is known on the subject, while the scholar will find the criticism, opinions, and conclusions of the authors not only stimulating but valuable in arriving at a more accurate interpretation of facts. For example, the observation that calculations of the population of Athens based on the production and import of grain (interpreted as wheat) are based on the incorrect assumption that Athenians did not eat barley.

The commentaries reconstruct as accurately as it is possible to do from the speeches what took place in each instance the part played by each character, the legal issues involved, and the use of *paragraphe* proceedings in the tactics of delaying a decision. Appendix 4 makes a good case for a distinction in meaning between *aphesis* and *appalage* against those who consider the terms to be a "synonym cluster" to denote a legal settlement by action of the plaintiff and/or the defendant. The three other appendixes present a chronology of Pasion's bank; maps of Athenian trade routes based on literary and epigraphical evidence, finds of Attic red-figured vases, coin hoards, and grave steles; and a summary of the evidence on *paragraphe* proceedings. A bibliography by subjects, a general index, and indexes of sources, geography, and names make the book eminently usable.

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R. J. A. TALBERT. *Timoleon and the Revival of Greek Sicily, 344-317 B.C.* (Cambridge Classical Studies.) New York: Cambridge University Press. 1975. Pp. xii, 234. \$15.00.

This is a comprehensive study of Timoleon's eight-year career in Sicily, his work of political liberation, constitutional reform, and economic revival that, as recent archeological excavations show, continued long after his political influence ended in 317 B.C. Talbert begins with two chapters on the ancient literary sources and then, in five subsequent chapters, examines the various chronological, military, political, and constitutional problems of Timoleon's career. The final three chapters deal with the relevant archeological and numismatic evidence. The special archeological bibliography, as well as the lists and charts in the numismatic chapters, will be particularly useful to the specialist.

Talbert's work is generally marked by caution, common sense, and a determination not to go beyond the evidence. He is willing to leave problems unresolved if the evidence proves inconclusive. For instance, on the events preceding Timoleon's capture of all Syracuse, Talbert concludes that no choice between the conflicting accounts of Plutarch and Diodorus is possible, having argued that Diodorus' version is not implausible and Plutarch's not without weaknesses. Similarly, he leaves open the possibility that Timoleon's treaty with Carthage guaranteed the independence of Greek cities in the Punic as well as the Greek zone of Sicily. On the constitutional reforms at Syracuse, Talbert believes that the pro-oligarchic Timoleon created a council with a limited membership, but insists that Diodorus' references to the Six Hundred neither support nor refute this opinion. He also rejects the idea that Plato influenced Timoleon in any way.

Talbert offers several other interesting suggestions and observations. He argues that Diodorus based his account of Timoleon chiefly on one unknown historian, but drew on Theopompus and possibly others for some details. The military studies point out Timoleon's remarkable abilities as a mercenary commander and the likelihood that his original force initially received little or no payment. In a later chapter he connects the fourth-century changes in Corinthian silver coinage and the almost simultaneous appearance of the new Corinthian Pegasi in Sicily with Timoleon's success in reviving agriculture and trade.

In sum, this is an excellent book, undoubtedly the best general treatment of Timoleon's career yet published.

MARY GOLDSBERRY
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FRANK ADCOCK and D. J. MOSLEY. *Diplomacy in Ancient Greece*. (Aspects of Greek and Roman Life.) New York: St. Martin's Press. 1975. Pp. 287. \$17.95.

The Greeks, from their experience, did not have a name for diplomacy, nor did they develop a specialized vocabulary for its practice. The city states had no continuing institutions, such as a State Department, charged with the formulation and conduct of a definite foreign policy. Thus, although originating in a desire to maintain peaceful coexistence, international relations tended to depend on the ability of individuals attempting to implement policies formed hastily and under pressure. The governmental structures of the states also hampered policy making. In Sparta authority was divided between kings and ephors; in Athens a volatile

council and assembly made final decisions in open meeting.

The late Frank Adcock and D. J. Mosley use records of negotiations, meetings, and agreements of various types for their study. They present it in two parts. Before his death in 1968 Adcock composed a short narrative of Greek history to serve as a framework for the analysis of the forms and institutions of diplomacy contained in the second part. D. J. Mosley, known for his monograph, *Envoys and Diplomacy in Ancient Greece* (1973), prepared Adcock's work for publication and wrote the second, more lengthy, section. There is also an appendix containing a few selected documents in translation, a short bibliography, and notes on references to source material. There is virtually no discussion of problems of interpretation. The result is essentially a reference handbook with succinctly presented material and occasional penetrating comment, but with an overall treatment of Greek diplomacy that is disappointing.

Tersely and clearly written, Adcock's account establishes the framework of interstate relations for classical Greece from the formation of the Peloponnesian League through Philip of Macedon's League of Corinth, "a typical exercise of generous statesmanship and shrewd self-interest." But the author takes little more than token notice of the diplomatic activity of the Persian War period, which resulted in a successful war coalition, and of Hellenistic Greece. His interpretation is conventional and in some cases outdated. After a short survey of the evidence, Mosley discusses the aims of diplomacy, which was mainly concerned with the regulation of relations among the states and "in some respects a negative and defensive reaction rather than constructive." There is a good chapter on the methods of diplomacy, particularly in policy making, but the section on types of treaties is laborious. The concluding chapter, "The Instruments of Diplomacy," sketches the organization and working of Greek leagues, from the alliances of classical Greece through the federal systems of the Achaean and Aetolian Leagues of the Hellenistic period.

In short, Adcock's narrative is too brief to supply the historical context needed to vivify the analysis; Mosley's analysis is valuable for its collection and organization of material, but the general reader will find his section hard going because of the mass of detail.

CARL ROEBUCK
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IOAN I. RUSSU, editor. *Introducere istorică și epigrafică, diplomele militare, tablitele cerate*. Preface by ȘTEFAN PASCU. (Inscripțiile Antice din Dacia și

Scythia Minor. First Series: Inscriptiile Daciei Romane, volume 1.) Bucharest: Academiei Republicii Socialiste România. 1975. Pp. 285.

Not since the late nineteenth-century publication of volume 3 (Dacia) of the *Corpus Inscriptionum Latinarum* (CIL), that monument of scholarly enterprise directed by Theodore Mommsen, has anyone assembled a new collection of inscriptions of the Roman province of Dacia (A.D. 106–271). Because of the numerous epigraphical discoveries made since then, especially over the last thirty years, and published in specialized and often obscure journals, an up-to-date *Corpus* would be an important contribution to further research and study of the history of Dacia. The task of making such a compilation, however, requires extraordinary linguistic skills and a vast technical knowledge. Widely recognized in Europe for his numerous books and articles dealing with Dacian inscriptions, Ioan I. Russu is uniquely qualified to undertake this assignment. The first volume is a superb epigraphical achievement worthy of its place as the successor of Mommsen's work.

Russu's new edition brings together all of the Dacian inscriptions known to date, including those published in the CIL, which in many cases are here revised and corrected. Besides a Romanian translation and pertinent bibliography, a photograph or drawing (often both) accompanies each inscription, thus offering an exact copy of the original text—a vast improvement over the CIL. In addition, Russu has assembled military diplomas and waxed tablets, leaving the remaining types of inscriptions (including *instrumenta*) for the second volume, as yet unpublished. The arrangement of documents in the first volume is chronological; in the second it will be geographical, from southwest to north, following Trajan's path of conquest. Either is a far more practical grouping than that found in the CIL, where the inscriptions are grouped generally from north to southwest, following the direction of Mommsen's travels in 1857 over the territory of old Dacia.

The first volume contains a brief but thorough and accurate account of the Roman conquest, organization, and development of the province (ch. 1). An explanation of the importance of inscriptions to the study of Dacian history and a survey of Dacian epigraphy follow (ch. 2). Of special note here is Russu's estimate of the valuable epigraphical contribution of the nineteenth-century scholar, Ștefan Moldovanu, whose work was omitted in Mommsen's study.

Scholars know of thirty military diplomas from Dacia, ranging in date from A.D. 86 to 164. Of these, twenty-three mention Roman auxiliary units stationed in Dacia, while seven mention units sta-

tioned in other provinces. Several diplomas are extremely fragmentary and are here published for the first time. Of particular value to Roman military history, these documents contain a wealth of information for Roman prosopography, as well as for political and social history.

Twenty-five waxed tablet inscriptions dating from A.D. 131 to 167, discovered in the eighteenth and nineteenth centuries in the gold mines of the Apusian Mountains, comprise the final chapter. Russu's thorough treatment of these important economic documents completely replaces the discussion of them in the CIL.

Roman historians will long remain indebted to Russu for the excellence and usefulness of this publication. His promised second volume excites eager anticipation.

DONALD W. WADE
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PAUL MACKENDRICK. *The Dacian Stones Speak*. Chapel Hill: University of North Carolina Press. 1975. Pp. xxi, 248. \$12.95.

Paul MacKendrick's latest exercise in petrological ventriloquism describes the archeology of Romania from prehistoric to early Byzantine times. The subject is fascinating. Romanian archeology has provided important evidence on such diverse subjects as the spread of the Neolithic, interaction between Greek colonies and local tribes, the development of Dacian kingdoms, and the rapidity and depth of acculturation in the latest of Rome's provinces. The history of Romania, from its position as the chief Romance language state in Central Europe to its present position as a Marxist nation, has shaped archeology and made the study of the discipline an interesting aspect of intellectual history.

MacKendrick's book follows a familiar format started in 1960 with *The Mute Stones Speak*. The author concentrates on the classical period, but he attempts to place it against preclassical developments. The narrative is very site-oriented, with descriptions of many excavations illustrating different periods. These accounts are generally clear, although sometimes dry, and are accompanied by good plans and maps and passable photographs. Descriptions of sites are combined with some information on the excavators, although this is less abundant than in previous volumes. The anecdotes are amusing, but superficial, and add little to an appreciation of the development of Romanian archeology.

The book has several positive qualities. No other book in English covers prehistoric Greek, Dacian, and Roman material. The account is accurate and updated. The bibliography, maps, and chronological charts are useful. The most serious limitation is

the conceptual framework. MacKendrick conveys a trite vision of the ancient world, centering on the sublimity of Greek culture, the brutality of Roman imperial and class oppression, and the foibles of Roman emperors. The material itself often calls for more sophisticated interpretation. The frame of reference is largely limited to books by MacKendrick himself. In discussing the Neolithic, for example, he refers only to Greek archeology without any attempt to place the Romanian material in the larger context of northeast Mediterranean prehistory that has developed so rapidly in recent years. He conveys no sense that modern archeology can be used as a sophisticated social science tool. No real consideration is given to the various forces, from nationalism through fascism to Marxism, that have shaped Romanian archeology.

The scholar will be grateful to MacKendrick for this introductory work. Its limitations, however, should be kept in mind and interested people urged to explore more deeply the complex world of Romanian archeology.

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ANTHONY BIRLEY. *Septimius Severus: The African Emperor*. Garden City, N.Y.: Doubleday and Company. 1972. Pp. xiv, 398. \$8.95.

P. A. L. GREENHALGH. *The Year of the Four Emperors*. New York: Barnes and Noble. 1975. Pp. xv, 271. \$17.00.

ROBIN SEAGER. *Tiberius*. Berkeley and Los Angeles: University of California Press. 1972. Pp. xviii, 300. \$12.95.

Plutarch says that he wrote biography, not history. In his *Lives*, history is only a background against which he depicts the noble and base deeds of his heroes. But modern historians are different. Ronald Syme once remarked that "biography offers the easy approach to history." When a contemporary ancient historian professes to be writing biography he certainly will wind up writing his kind of history; when he says history, the bet is that what he really has in mind is prosopography. This is in keeping with another "Symean law" according to which "in all ages, whatever the form and name of government, be it monarchy, republic or democracy, an oligarchy lurks behind the façade; and Roman history, Republican or Imperial, is the history of the governing class" (Ronald Syme, *The Roman Revolution* [1939], p. 7). But the strange peculiarity of an authoritarian regime is that there also is a despot, cruel or benign. To find him one need not look behind the façade: he lurks everywhere.

The three books under review exemplify all the

virtues and shortcomings of the maxims "through biography to history" and "oligarchy first, despot next." P. A. L. Greenhalgh is a specialist in the history of early Greek warfare. *The Year of the Four Emperors*, his first book dealing with Roman history, is a popular and straightforward account of events and personalities of the fateful year 68-69, when after the death of Nero four emperors occupied the throne in succession. It is informative and readable, and probably nothing more needs to be said of it. The books by Seager and Birley are important scholarly monographs; they have footnotes, bibliographies, and, in the case of Birley's book, a detailed and very valuable prosopographical appendix. In matters of scholarly erudition both books are beyond reproach, as was to be expected of their authors. Robin Seager has made a name as a historian of the Roman Republic, and Anthony Birley has published extensively in the field of imperial history.

Seager's Tiberius is a rationalistically inclined gentleman—Seager would have none of his idiosyncrasies and follies because they are not of particular interest to him. Obviously they cannot be of any importance to a "sober" historian, who, like Seager, seriously maintains that "Tiberius' principal virtue was moderation" and that "the servility of senate and people gave him ample opportunity to exercise it" (p. 142). But "to preserve moderation in matters of honours was not easy" (p. 143), nor was it easy for Tiberius to maintain the appearance of republican freedom in his dealings with individual senators" (p. 132). Nor is it easy to believe in Tiberius' propaganda of *moderatio*, but Seager is somehow convinced. The principate was forced upon Tiberius against his will by Augustus (and the Senate). He tried to refuse it, and "to deny Tiberius' sincerity is perverse" (p. 36). Seager quotes Béranger's brilliant study on the ceremony of the refusal of power, but he has not learned too much from it. It is a misunderstanding to speak of sincerity or dissimulation. This ceremony was not a "polite comedy" but a religious, magical, and political ritual. In an authoritarian regime everything tends to be ritualized, and nothing is a farce.

Following in the footsteps of Augustus, Tiberius was, of course, also interested in raising the "moral" standards of society, sexual and otherwise. The old trick of despots and other benighted minds obviously works: Seager swallows the bait. Nor can Birley refrain from condemning the "levity" of women, stage, and arena (p. 101). Seager, following Suetonius, credits Tiberius also with sumptuary measures—"limiting the activities of food-shops and restaurants, and forbidding altogether the sale of pastries" (p. 141). Tiberius the republican loved freedom, but even he could not do too much about it. For security reasons some

people had to be "removed," but both authors agree that "counting of heads misses the point" (Seager, p. 239), and that "it is no good counting heads" (Birley, p. 279). Seager admits, however, that Tiberius' rule degenerated into a reign of terror (p. 239); Birley on the other hand defends Septimius Severus against the *obtretractores* (ancient and modern) who called him "Punic Sulla." He was neither Punic (his accent was provincial) nor a cruel tyrant. His behavior was "much more normal than historians of Rome like to remember" (p. 281). He rates rather low in comparison with Vespasian (who executed few senators), but rather high if compared with Commodus or Domitian.

The trouble with Western historians of the Roman Empire is that they know too well the history of the republic and that they themselves are republicans who are more or less converted to the "necessity" of autocracy. If they knew better the history of China or Russia they would be much less perplexed. In an autocracy, purges, servility, and adulation are not vices but virtues, signs not of decadence but of political skill. For a historian of Imperial Rome Machiavelli and Wittfogel are as important as Tacitus and Syme.

JERZY LINDERSKI
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J. H. W. G. LIEBESCHUETZ. *Antioch: City and Imperial Administration in the Later Roman Empire*. New York: Oxford University Press. 1972. Pp. xiv, 302. \$17.00.

The Syrian city of Antioch in the fourth century A.D. has received its due, which is considerable, from ancient and modern authors. The voluminous writings of the Sophist Libanius have provided the foundation for many an edifice of modern scholarship. One thinks of Seec's masterly study of the chronology of Libanius' letters, Petit's *Libanius et la vie municipale à Antioche au IV^e siècle après J.-C.*, Festugière's *Antioche païenne et chrétienne*, and the middle part of Downey's *History of Antioch in Syria*. Liebeschuetz adds to this with a social, economic, and administrative survey of the fourth-century city along the lines laid out by his teacher, A. H. M. Jones. The phrase "later Roman Empire" in the subtitle is misleading, but within its limits the book is useful for its abundant citations from the ancient evidence.

Liebeschuetz is strongest in his exploitation of literary sources—it is indicative that he chose to begin with a review of the life and works of Libanius—yet Petit's books and Norman's work on individual speeches still remain basic. Somehow in writing what is a work of synthesis Liebeschuetz never penetrates to the life of that teeming and turbulent city. A passing reference on page 134 to

the magnificent private residences at Antioch is insufficient for a topic to which the Princeton mosaics can contribute handsomely. Six pages under the forbidding rubric "Routine Entertainment" are all that we have of dancing, acting, chariot racing, and (a little oddly) bathing establishments. Liebeschuetz's treatment of Antioch's relations with the Syrian countryside is competent but again rather drab. A rich variety of items on Syrian hermits is assembled on page 234, but without any inspiration from Peter Brown's perceptive article on the rise and function of the holy man (*Journal of Roman Studies*, 61 [1971]: 80 ff.).

A study of Antioch must not depend only upon classical literary sources. It requires close attention to the geography of Syria, the topography of the city, and the results of the Antioch excavations. The absence of a plan of the ancient city and the inadequacy of the description of the site are symptomatic of a general weakness in Liebeschuetz's *Antioch*. Occasionally he makes good use of Tchalenko's pioneering *Villages antiques de la Syrie du nord*, as on pages 71–72 in refutation of Libanius, but one misses an overall awareness of the physical contours and remains. Unfamiliarity with archaeological evidence has also infected a map of the Syrian defense system: recent excavation has made it certain, for example, that Qasr el Heir, north-east of Palmyra, is not Adada as shown.

Finally, some trouble must be noted in the area of nonclassical literary sources. Bardaisan would have been surprised to learn that Christian Syriac literature at Edessa came into existence in the fourth century (p. 62).

G. W. BOWERSOCK
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WALTER GOFFART. *Caput and Colonate: Towards a History of Late Roman Taxation*. (Phoenix: Journal of the Classical Association of Canada. Supplementary volume 12.) [Toronto:] University of Toronto Press. 1974. Pp. 165. \$10.00.

The peoples of the Roman Empire were also certain about death and taxes, but we have had better understanding of how they suffered the former than of how they exacted the latter. This excellent book now remedies the situation. Goffart shows that *tributum* was a contribution, not a direct tax based on census returns, though the census possibly determined the lump sum levied on provinces. The management, collection, and even apportionment of municipal taxes were in the hands of local aristocrats who not only paid the major share but also performed the *munera*, an essential part of imperial taxation and vital to the local communities. Liability was apportioned on the basis of

personal *professiones*, but how responsibility was shared by the lower classes remains a mystery.

After the currency collapse of the third century, Diocletian collected taxes in kind, the *annona*, from all parts (and more people) of the empire, but he continued to rely on municipal initiative, personal *professiones*, and local tax registers for its collection. Diocletian checked abuses by introducing abstract units of account, *iugum* and *caput*, into which all resources of the empire were reduced by means of a schematic survey. Both *iugum* and *caput* were shares of assessment, not land and head taxes, and tax liability was converted into shares of the assessment by means of uniform conversion schedules for lands and mortals. In a postscript Goffart argues that the rank and file of the military system served as Diocletian's model for this innovation.

Through a chronological analysis of pertinent laws, Goffart defines the changing meanings of tax terms and clearly demonstrates how personal *professiones* and local tax registers came to be used by the imperial government, almost like a census, until gradually *iugum* and *caput* became the basis for taxing lands and souls late in the fourth century. Finally, when land alone came to bear the tax burden, the *colonus* was bound to the soil to guarantee its productivity. While I cannot agree with all Goffart's political, social, and economic conclusions drawn from the methods of taxation outlined, I have reserved disagreement in order to focus on the book's very real merit. Ancient historians should pay close attention to the author's complicated but carefully reasoned arguments.

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MEDIEVAL

WILLIAM R. BEER, translator. *Slavery and Serfdom in the Middle Ages: Selected Essays by Marc Bloch*. (Publications of the Center for Medieval and Renaissance Studies, 8.) Berkeley and Los Angeles: University of California Press. 1975. Pp. ix, 276. \$17.50.

PETER CLEMOES, editor. *Anglo-Saxon England 4*. New York: Cambridge University Press. 1975. Pp. ix, 262. \$24.00.

A comprehensive and systematic history of medieval slavery and serfdom has yet to be written. Charles Verlinden's *L'Esclavage dans l'Europe Médiévale*, which appeared more than twenty years ago as the first of a projected four-volume study, is a monumental achievement. It contains a collection of documents essential for a history of slavery but deals with slavery only in the late Roman Empire in the West, the Iberian Peninsula, and

southern France. More recently, the chapter on the Middle Ages in David Brion Davis' ambitious work, *The Problem of Slavery in Western Culture*, is concerned with theories of slavery rather than with actual practice. Davis views Western culture from the limited vantage point of North America, and he was innocent of most of Marc Bloch's writings on slavery.

The volume under consideration contains six studies on medieval slavery and serfdom, all previously published but brought together here from often inaccessible journals and translated into English for the first time. In the classic essay, "How Ancient Slavery Came to an End," Bloch shows how the breakup of the Roman latifundia contributed to the decline of slavery in the empire and how trade and war perpetuated it. The attitudes of the Germanic laws, the dilemma of the Church, the varying meanings of the terms slave and serf, the methods and meanings of manumission, together with the economic necessities of the times are treated with respect to the growth of serfdom in the Carolingian period. The second article, "Personal Liberty and Servitude," continues the discussion and sketches in broad outline the evolution from slavery to serfdom in Western Europe down to about 1300. The other four are specialized studies, emphasizing the variety and complexity of serfdom where local custom determined particular conditions and obligations.

Beer has obviously struggled with Bloch's sometimes complicated syntax, and the resulting translation is generally smooth and lucid. The notes at the end of the book, apart from illustrating once again Bloch's proverbial learning, invite attention to the wealth of primary and secondary material that must be examined before a thorough treatment of the subject can be written. The recent researches of such scholars as Theodore Evergates and John Freed require modifications of Bloch's interpretations of feudal society, but until the study of medieval slavery and serfdom is finally produced, these writings of Marc Bloch remain the starting point.

Anglo-Saxon England 4 is the fourth volume in an annual series devoted completely to Anglo-Saxon studies by using "less commonly considered forms of evidence" and a cross-disciplinary approach. This important book both contributes to the realization of the editor's goals and sustains the high level of scholarship of its predecessors. Of the thirteen articles here, six deal with language and literature, two with art and crafts, two with history, and one each with liturgy and numismatics. The final paper is a list of the forms, symbols, and abbreviations used in the *Dictionary of Old English* and in *Old English Syntax* (both now in preparation); these forms and symbols should be used in

papers submitted to *Anglo-Saxon England*. One of the history papers is a valuable review article discussing all the literature on Scandinavian settlements in England published since the appearance of Stenton's *Anglo-Saxon England* in 1943. The bibliography listing publications for 1974 will be indispensable for students of many aspects of early English culture.

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HARRY W. HAZARD, editor. *A History of the Crusades*. Volume 3, *The Fourteenth and Fifteenth Centuries*. Madison: University of Wisconsin Press. 1975. Pp. xxi, 813. \$25.00.

This is the third large volume in the monumental, multiauthor history of the Crusades projected many years ago by A. C. Krey, Duncalf, and La-Monte and finally, but very slowly, seeing the light of day under the direction of Kenneth M. Setton and his collaborators. The editor of this volume, Harry W. Hazard, remarks that the book has been delayed for twenty years, so that some of the chapters were originally prepared in the 1950s and 1960s. Four of the authors have died. The long postponement has been unfortunate, but the volume is at last very welcome.

As in the previous two volumes, each author is a distinguished authority on his subject. Aziz S. Atiya leads off with a skillful survey of the Crusades of the fourteenth century. Deno Geanakoplos offers two chapters on Byzantium and the Crusades, focusing on the unsuccessful efforts of Eastern and Western Christendom to resolve their religious differences in the interest of presenting a united front in the defense of Byzantium against the infidel. Peter Topping presents two chapters on the struggles for power among dynastic factions in the Morea that are as learned as one would expect from him but rather dense with dynastic detail (for example, "the dower of Louis's consort Joanna, a granddaughter of John and Gravina and daughter of the late Marie of Anjou, sister of Queen Joanna"). Setton himself gives us a couple of chapters on the Catalan adventurers and their rivals in Greece. Anthony Luttrell tells of the futile crusading efforts of the Hospitalers, who were faced with insufficient resources, lack of powerful allies, the rivalries of Venetians and Genoese, Greeks and Latins, the ineffectiveness of papal crusading policy, and the general indifference of Latin Christendom. Ettore Rossi continues the tale of the Hospitalers after 1421, centering upon the defense and the ultimate loss of Rhodes. Sir Harry Luke makes an interesting narrative of the efforts of the Lusignans of Cyprus to carry on the Crusades, their

vassalage to the sultan after 1426, and the acquisition and loss of the island by Venice. Charles Julian Bishko recounts the *reconquista* from the late eleventh century, concentrating on the twelfth and thirteenth centuries and continuing through the fall of Granada. Hazard tells of the decline of Muslim North Africa. Another of the more readable chapters is that by Mustafa M. Ziada on the troubled successions to power in Mamluk Egypt and the unruliness of the Mamluk soldiery. Denis Sinor writes of Latin contacts with the Mongols and the shifting alliances of Byzantines, Latins, the Golden Horde, and the Ilkhans. Edgar N. Johnson recounts the expansion of the more advanced Germans in the lands on the southern and eastern shores of the Baltic at the expense of the more primitive Slavs. Frederick G. Heymann gives a very concentrated account of the futile efforts of empire and papacy to repress the Hussites, 1419-36, to the compromise at Basel, which enabled the movement to survive until its absorption in the sixteenth-century Reformation. Finally, we have another survey chapter by Atiya on the fifteenth-century Crusades.

Each chapter, except for Atiya's surveys, is provided with an initial bibliographical note, and there are a number of maps, a very helpful gazetteer, a chronology, and a good index.

The volume as a whole shares many characteristics with those other great collaborative works, the Cambridge histories. The authors are outstanding scholars, the subjects are treated relatively fully and authoritatively, and it is packed with one fact after another, largely concerning dynastic history. It will be invaluable to specialists and for reference use, but even professional medieval historians will find it difficult to read some of this generally unfamiliar material for general knowledge and pleasure. This is not meant to be highly critical, but merely descriptive. For the right uses, it will be a superb tool.

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EILEEN POWER. *Medieval Women*. Edited by M. M. POSTAN. New York: Cambridge University Press. 1976. Pp. 112. Cloth \$12.95, paper \$4.45.

This delightful little book brings together a number of essays about medieval women prepared by Eileen Power before her premature death in the 1940s. These studies have been selected and arranged by Power's husband and colleague, M. M. Postan, who also added the footnotes and brief bibliography.

Although not couched in a scholarly guise, since

most of these papers were originally prepared as popular lectures, these studies nonetheless offer a wide ranging and reliable discussion about the role of women in their various social classes, with examples drawn from England, France, and Germany. Since these papers were written before the opening of the feminist crusade in its current phase, they avoid the sometimes contentious note of recent work on the place of women in European society. In this work, women emerge as an essential component of the medieval social order; their significance was often much greater and more independent than their formal legal position may have implied.

From an opening chapter on medieval ideas about women—shown to be delightfully contradictory—through chapters on the lady, the working woman in town and country, the education of women, and nunneries, Power discusses medieval women in a charming and witty style.

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FREDERICK H. RUSSELL. *The Just War in the Middle Ages*. (Cambridge Studies in Medieval Life and Thought. Third Series, volume 8.) New York: Cambridge University Press. 1975. Pp. xi, 332. \$32.50.

The Just War in the Middle Ages is a distinguished book that is a worthy and valuable addition to the third series of Cambridge Studies in Medieval Life and Thought. Frederick Russell has utilized all the primary and secondary material available to him, including manuscripts and early printed books, to present an exhaustive study of medieval just war theories. He writes clearly and logically, and his book is certainly a definitive account that will be useful to scholars in many different fields.

Aristotle first used the term just war, and Russell sets the stage for his discussion of the medieval theories by a brief account of classical traditions, Old and New Testament attitudes, and Roman law precedents. St. Augustine, however, was the most seminal figure in the early Church in this area as in so many others, and Russell treats at length Augustine's view that war was "both a consequence of sin and a remedy for it." It was Augustine who legitimized warfare and made it necessary rather than inherently sinful by grounding it in evangelical precepts, especially the notion of charity—war, if carried on without malice, was an act of love, since it punished evildoers and prevented them from doing further wrong.

Russell next focuses on Gratian's *Decretum*, pointing out that it was Gratian who extended

divine authority for waging war to ecclesiastical authority and gave the Church a moral mandate to wage war in its own behalf. Gratian also advanced a legal definition of the just war and suggested conditions under which it should be carried out, thus laying the basis for the introduction of the concept into modern international jurisprudence.

The Decretists explained, consolidated, systemized, and updated the analysis of war in the *Decretum*, elucidating the purpose of warfare and restricting the locus of authority to wage war to "princes, popes, and prelates." By Huguccio's time, the use of violence was considered the monopoly of legitimate authority and justified whenever necessary to protect the patria or the Church from their various enemies. The Decretalists then accommodated previous ideas to the political conflicts of the thirteenth century, but with an emphasis less on the just cause than on questions of authority and jurisdiction.

A concluding chapter focuses on St. Thomas Aquinas and his circle, and Russell portrays Aquinas as a most traditional yet innovative thinker whose major contributions were his application of Aristotelian political convictions to the analysis of war and his attempt (although not in any systematic way) to synthesize Augustine's idea of war as punishment for sin with Aristotle's notions of the common good and *raison d'état*.

Russell's approach is basically textual, and he admits to tunnel vision in analyzing the materials. He does, however, make frequent reference to the context in which the various theories were being worked out. The Crusades, a specifically medieval development, receive no separate or extensive treatment in this book, although Russell makes many interesting observations about them and devotes several pages to the way the Decretalists, especially Innocent IV and Hostiensis, handled the matter. In general, the canonists and theologians hesitated to assimilate the Crusades into their just war theories, a reticence attributed to their reluctance to involve the Church too directly in bloodshed. As the author says, however, "The restrained justification of crusades and other ecclesiastical wars of course constituted no barrier to unrestrained prosecution of these wars" (p. 296).

In his conclusion, Russell calls attention to the time-bound nature of medieval just war theories and other deficiencies that preclude their general application in modern times. Yet his book, an important and scholarly contribution to medieval studies, is quite timely and raises provocative questions for our own age.

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JONATHAN SUMPTION. *Pilgrimage: An Image of Mediaeval Religion*. Totowa, N.J.: Rowman and Littlefield. 1976. Pp. 391. \$15.00.

This industrious compilation of instructive, entertaining episodes and information from pilgrimage history disappoints at several levels. It is not the much-needed survey of pilgrimage history, nor really a probing of medieval popular religion under the aspect or microcosm of pilgrimage, but rather Jonathan Sumption's gloomy impressions of that religion as illustrated by a selection of episodes jumbled together from ten centuries and several countries.

Chapter topics fill roughly thirty-five pages on the cult of saints and relics, twenty on miracles and charlatanry, sixteen on cures and medicine, nine on origins and ideals, sixteen on pilgrimage as penance or penalty, twenty-two on the Great Age (from 1000), eight on the pilgrim's vow and indulgences, twenty on cult (canonization, publicity, offerings), forty-two on "The Journey" (which is fascinating, and the best part of the book), six on the shrine, forty on Rome, ten on pilgrimage as tourism, twenty on later medieval cult, and fourteen by way of summary.

In content, *Pilgrimage* views the thousand years of medieval popular religion as a pathology (its practitioners were a brutalized, gullible, death-and-devil obsessed *massa damnata*), with pilgrimage as an aberration of ritual magic to assuage claustrophobic guilt and terror. In methodology it is old-fashioned G. G. Coulton: juxtaposition of sources widely separated in time, place, and character, bereft of context or examination, each taken at face value. Even the pathology of popular religion is a valuable field, but it would demand wide erudition about the religion (its varied *Sitz im Leben*), a disciplined focus in time and place, and the newer methodologies instead of mere sifting for stories. Emmanuel Le Roy Ladurie has recently demonstrated what can be done here in his celebrated *Peasants of Languedoc*.

Platoons of doctoral students in the new medievalism, armed with the weapons of the Bloch-Feuvre *Annales* school, should be deployed against the challenge of pilgrimage as image, as microcosm, as media and tourism, as pathology, and as deep human drive or tropism. Meanwhile the knowledgeable medievalist can extract much incidental profit by excising and transplanting data the author has laboriously gleaned.

ROBERT I. BURNS, S.J.
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CHRISTOPHER N. L. BROOKE, assisted by GILLIAN KEIR. *London, 800-1216: The Shaping of a City*. His-

tory of London.) Berkeley and Los Angeles: University of California Press. 1975. Pp. xxi, 424. \$21.00.

It is trite to say that this is a big book worthy of its subject. In both conception and execution this is a major study, which will be used and cited for many years. The work reflects and justifies the years of effort the authors devoted to planning, writing, and traveling. It is one of those happy products that stand midway between monographic and synthetic work, and it faces, with value and dignity, in both directions.

The book begins with the London of Offa of Mercia and of Alfred the Great and runs through the death of John. We have by then the recognizable medieval map of the city, almost a full complement of the intramural regular houses and churches, a municipal charter, and a mayor. Since the main story only begins with the Norman Conquest, the choice of the earlier date is largely to illustrate the basis on which Norman London rested. By the time we take our farewell in 1216 we can look back upon FitzStephen's famous description of Plantagenet London, upon the turbulent movement for a commune, and into some identification and analysis of the new oligarchy that was to dominate the future. My only criticism is that, in an already long and complex book, there is a tendency to include items of little relevance. The offending portions are mainly those that deal with the city's religious houses. These sections are set in the context of the intellectual life of the twelfth century and only by inference in that of urban growth. Those parts covering Italian towns and the general problems of urban development, however, are very good, and full credit goes to the authors for their inclusion.

The development of a great city is a drama of social change and evolution. The dramatic tension in this version of one particular case study comes from the way several major themes are juxtaposed: self-government or royal control (the city's mayors versus the king's sheriffs), a city of Englishmen or of foreigners (Vikings, Normans, Flemings, or Frenchmen), and an insular city without its own archbishop or a European port and metropolis. The wonder is that there are no simple answers, no winners. The growth of wards and parishes, of the mayor's office, of suburbs, and of banks and the mint, are all separate but related incidents in the continuous drama. The urban revolution of the twelfth century was a great milestone in the emergence of modern European civilization. London had a reasonable role in this, as well as a major one in the growth of the English kingdom and people. Both aspects are well illuminated here.

The book, even by today's outrageous prices, represents good value for the money.

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ROBERT FOLZ. *The Coronation of Charlemagne, 25 December 800*. Translated by J. E. ANDERSON. Boston: Routledge and Kegan Paul. 1975. Pp. xii, 266. \$18.00.

First published in French in 1964, this small masterpiece is at long last available in English translation. Robert Folz is well known for his monumental studies of the legend and cult of Charlemagne in the Middle Ages; for this reason he is better qualified than most historians to convey a deep understanding of the efforts and significance of Charlemagne for France and Europe. Above all, even in his own lifetime Charlemagne towered, somewhat charismatically, in the imagination of his contemporaries. Folz's special sensitivity to the religious and spiritual nuances of Charlemagne's impact enables him to provide a thoughtful, scholarly, and humane portrait of the events leading up to the coronation in A.D. 800.

It is unfortunate that the publishers should have chosen for the dust jacket a late medieval, idealized portrait of Charlemagne, which misdirects the reader's attention. Folz certainly does not. He concentrates on the growth of Frankish power, starting with Clovis and leading to Charlemagne. As the topic requires, the focus is on political and religious developments that explain the imperial coronation, rather than on social and economic history. The finest chapter of the book, "The Empire in Theory and Practice," surveys the immediate consequences of the coronation—the new outlook and sense of responsibility it promoted. Folz presents all this with the brevity and lucidity that prolonged occupation with the subject makes possible; the elegance of style reflects his easy familiarity with the source material.

I hesitate to criticize a good book, but when I put it down I felt the author had not touched upon those nagging doubts that keep disturbing my understanding of the reign of Charlemagne. The emperor dominates the preceding and succeeding centuries—or so the sources make us believe. But we know that the technology available and the social and geographical impediments to administration must have made it impossible for Charlemagne to translate into practice the politico-religious conceptions that inspired his laws and letters. What are we to make of this contradiction? Folz does not address himself to this question, and given the book's subject, there is no reason why he

should. Still, one hopes that one day he will tell us whether he thinks Charlemagne caused the Carolingian Renaissance or the Carolingian Renaissance drew our present-day picture of Charlemagne.

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RICHARD VAUGHAN. *Valois Burgundy*. Hamden, Conn.: Archon Books. 1975. Pp. vii, 254. \$12.50.

The author of the recently completed four-volume history of the Valois dukes of Burgundy (1363–1477) has undertaken here "to gather some of the material and all the more important arguments and conclusions of the larger work." His book is much more intelligently written and argued than Joseph Calmette's *The Golden Age of Burgundy* (New York, 1962), the work in English that most resembles it in format.

Valois Burgundy is intended for students and the general reader. Those familiar with the earlier volumes will find little new here save a discussion of the sources. After an introductory chapter explaining the political, institutional, economic, and cultural milieu of his subject, Vaughan reiterates his conclusion that Philip the Bold, the first Valois duke, consciously laid the essential territorial and institutional foundations of a separate Burgundian state while remaining in essence a French royal prince: nearly half of his total revenue in his last years came from the treasury of his nephew, the mad King Charles VI. John the Fearless is seen as the best general of the four rulers, but as an enigmatic figure who left little mark on Burgundian institutions. Philip the Good was a generally able man who eventually outlived his usefulness. In reversing the French orientation of his father and grandfather and looking instead toward the Netherlands and the Rhine, Philip instituted a change of policy that was to lead to the sudden decline of Burgundian power in the next reign. Vaughan finds that Charles the Bold deserves more credit than he has generally been accorded as an administrator, but less as a general.

The student will find here a clear and concise description of the men and institutions that governed a large and extremely diverse territory in the late Middle Ages. Valois Burgundy suffered by always remaining distinct northern and southern blocs that were separated for most of this period by the duchies of Bar and Lorraine. The first three dukes were reluctant to create central institutions that might have overcome the regional particularism of the various principalities of their composite state. Still, Vaughan regards the fall of Burgundy

as less the result of institutional failure than of the military disasters and imperial involvement of Charles the Bold. Charles' rivalry with Louis XI of France was a picturesque sidelight to the larger thrust of his policy.

The organization of *Valois Burgundy* leads to some repetition, but this is history in the grand manner. Vaughan is at his best with vivid descriptive passages dealing with the dukes and their court, administration, and military engagements. His stated goal, "to describe a political organization, not to write the history of territories, regions, areas," has surely been realized.

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JAN READ. *The Moors in Spain and Portugal*. Totowa, N.J.: Rowman and Littlefield. 1975. Pp. 268. \$12.00.

An inveterate traveler in the Iberian Peninsula and an *aficionado* of things Moorish, Jan Read has here digressed from his accustomed specialties in drama and film to the field of history. The result of his labor of love is what the book jacket cautiously labels as "perhaps the first" popular survey of the Moors in Spain and Portugal for the English reader. Inclusion of one brief chapter on Portugal, together with a chapter tracing developments from 1492 to 1614, are the main structural elements distinguishing Read's treatment from that of W. Montgomery Watt (*A History of Islamic Spain* [1967]).

One does not look to a work on this level for originality of interpretation. Américo Castro's views on the pre-eminence of the individual and the common man for the evolution of Spanish history serve as a thesis of sorts for the survey. Lévi-Provençal's comprehensive studies have been judiciously mined for material in chapters one through eleven covering developments up to and including the fall of the Cordoban caliphate. The lack of any comparable secondary work for events beyond 1031 is reflected in the emphases and lacunae characterizing the last half of the book. Thus, Read's approach to Islamic culture, supplemented by Menéndez Pidal's monumental works, has guaranteed consideration of literary, philosophical, and religious facets of Moorish society to the near exclusion of economic aspects. Political analysis frequently halts at the level of details derived from dynastic-military chronicles. The Mudéjar element, imperfectly researched as yet, is understandably neglected, as are contemporary debates and publications pertaining to the crucial theme of cultural interpenetration in medieval Spain. The Almohade period remains as ill-under-

stood as ever, its establishment being noted as having been "sealed" by the victory of Alarcos in 1195 only to be decisively undermined by defeat at Las Navas de Tolosas in 1212. Valencia, as the scene of distinctive socioeconomic and artistic developments, is almost totally ignored; even the slight reference made to that region merits its inclusion in an otherwise adequate index.

It is regrettable that some disproven hypotheses and partisan interpretations are perpetuated. To cite a few examples, the theory of a causative relationship between Islamic frontier institutions and Christian military orders can and ought to be discarded in view of present research by O'Callaghan and others. To revere Islamic martyrs as genuine victims of their faith while speculating that persecuted Christians were "deliberately trailing their coats" reflects the aprioristic generalizations of an earlier generation of Arabist historians and ignorance of monographic literature such as that by Edward Colbert (*The Martyrs of Cordoba, 850-859*).

If Read has produced a volume calculated to disappoint the scholar, he has nevertheless striven rather successfully to please and meet the needs of the general reader. Colorful vignettes of significant personages, lively episodic digressions garnished with apt quotations from Arab and Hispanic chronicles and Moorish literature, occasional plates and maps, and a comparative chronological chart all make for pleasurable reading. Allusions within the text itself and a select bibliography compensate for lack of a conventional critical apparatus and point the interested reader toward standard secondary works pertaining to the subject. Those who pick up this book expecting an independent scholarly interpretation will be disappointed. Those who ask no more than an introduction to the Iberian phase of Islamic history in an entertaining and generally reliable fashion will be gratified.

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ROBERT IGNATIUS BURNS. *Medieval Colonialism: Post-crusade Exploitation of Islamic Valencia*. Princeton: Princeton University Press. 1976. Pp. xxiv, 394. \$21.50.

The present work constitutes the third volume of Robert Burns' magisterial analysis of Valencian society in the thirteenth and fourteenth centuries. *The Crusader Kingdom of Valencia* dealt with the role of the Church as a frontier governing institution. *Islam under the Crusaders* expounded the economy, class structure, law, and institutions of the conquered Muslim society. The present volume concentrates on economic life as reconstructed from

Christian tax records: that economic life whose revenues were decisive for the rise of Aragon-Catalonia as a dominant Mediterranean commercial and naval power.

Conquered Valencia was far more prosperous than either Aragon or Catalonia. The Christian kings respected the agricultural efficiency and the artisan and commercial skills of their new subjects. While imposing themselves as landlords, political and military sovereigns, they did their best to protect Valencian prosperity by retaining as far as possible the kinds of taxes and economic codes that had developed under Almohade rule.

Burns' sources are mainly Christian, rather than Muslim, the former having kept many more written records than did the latter. The reader will learn much about bakeries, baths, and butcheries, water distribution and salt supplies, transport and hostleries. The book is a veritable encyclopedia of trade descriptions, monetary values, tax definitions, and "state of the question" discussions of the primary sources, the regional and chronological variations, and the past scholarly interpretations of the individual topics being treated. In the copious footnotes the author is constantly at pains to explain the basis of his reasoning, the nature of his hypotheses when filling in gaps, and his estimate of the reliability of his documents.

Despite the technical nature of his discussion, Burns manages to create a coherent and pleasing narrative. The reader is abundantly aware of a complex market economy, of agriculture as "big business," and of all classes being engaged in artisan and seagoing commerce. Even small towns have public baths, the mark of widely diffused prosperity. There are almost no Mozarabs, and Jews are not distinctively important to the economy because of the high level of Mudéjar economic development. For some decades following the conquest, Mudéjar society is perhaps better off economically than in the last period of Almohade rule because tax rates are comparable and life is less violent. But the loss of independence slowly produces psychological depression and lowered economic vitality.

This is a unique work of scholarship, extracting the maximum amount of information from scattered tax records and blending with this information the political, psychological, and communal relations between the subject Muslims and the minority Christian rulers.

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PETER PARTNER. *The Lands of St. Peter: The Papal State in the Middle Ages and the Early Renaissance*. Berkeley and Los Angeles: University of California Press. 1972. Pp. xvii, 471. \$17.50.

Peter Partner, a recognized authority on the Papal States in the Late Middle Ages and the Renaissance, here offers a continuous history of the patrimony of St. Peter from the beginnings until the mid-fifteenth century. A subsequent volume on the Papal States in the Renaissance has already been announced by the University of California Press. The present volume is an unabashedly old-fashioned political and military history, a type of work the author concedes is not in favor at the moment. Its detailed account of wars and treaties is scarcely interrupted even by descriptions of the institutions of government, not to speak of the economic and social background. Still, accepting it for what it professes to be and allowing the author to write his book, not the one I might have preferred, it is thorough, judicious, and reflects great learning. Although there is no bibliography and only rather superficial notes on the historical literature and the sources, the footnotes are replete with references to the best and latest scholarship.

The foreground of the work is occupied by local wars and politics, but Partner properly views his subject as a subtopic of the history of the universal Church. The Papal States were not just another episcopal patrimony, but a very partial realization of the Donation of Constantine. In the High and Late Middle Ages the concern of the popes for the patrimony was steadily growing, and idealists like Dante and Langland criticized the papacy for its secularity. The protesters were a minority, however, for the temporal power of the popes was generally accepted in the Middle Ages. If the popes possessed and ruled their lands, furthermore, wars naturally followed. The bloody hands of the popes, Partner believes, did not especially offend medieval society or erode the religious authority of the papacy.

Readers may wish to consider a few more of Partner's revisionist positions with a view toward modernizing their class notes. He gives a sober, nonromantic account of the dominance of the house of Theophylact, rejecting the lurid tales of Otto I's adherent, Bishop Luitprand of Cremona. (Learned historians continue to ruin the spicy anecdotes that used to enliven, if not enlighten, our classes. No doubt it is for the best.) In discussing Cola di Rienzo, he points out that the tribune, much admired by historians for his modernity, claimed a vision of the "holy" Boniface VIII, the great theocrat abominated by the same historians, whereupon Cola set out to avenge the outrage of Anagni. This aspect of the reformer's views has escaped the attention of most writers. Partner also proposes a new evaluation of the achievements of Cardinal Albornoz, who did "very little to secure the pacification of Italy, which was deemed a necessary prerequisite for the return of the Roman bishop to his own see." As a final example of

Partner's revisionism, he criticizes the interpretation in national terms of the outbreak of the Great Schism.

The book is somewhat carelessly written and edited, so that there are occasional obscurities.

DONALD E. QUELLER
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DOUKAS. *Decline and Fall of Byzantium to the Ottoman Turks*. An annotated translation of *Historia Turco-Byzantina* by HARRY J. MAGOULIAS. Detroit: Wayne State University Press. 1975. Pp. 346. \$18.50.

The last days of the Byzantine state and the fall of Constantinople are described by a number of historians and eyewitnesses, at least four of them Greek (Sphrantzes, Chalkokondyles, Kritovoulos, and Doukas). They represent different views, especially as regards the sizing up of the adversary forces and their leaders and the analysis of the causes for the demise of the *Historia Turco-Byzantina*, printed in no fewer than four editions (the first being that by I. Bullialdus in 1649) before the appearance of the fifth edition by Vasile Grecu (Bucharest, 1958), based on a manuscript newly discovered in the Bibliothèque Nationale. Grecu corrected errors in the previous editions and added a Romanian translation. H. J. Magoulias has now translated into English and annotated Grecu's edition.

In a lengthy introduction Magoulias writes of Doukas' life and the value of his work. The entire body of knowledge about Doukas, which does not include his baptismal name, comes from what he himself revealed. He lived in New Phokaia, where he was a secretary of the podesta; later he moved to Lesbos to enter the service of the Genoese Gattilusio family. He was on a mission to Adrianople in 1451 when Mehmet, the future conqueror, first entered what was then the Ottoman capital.

In his *Historia* Doukas describes the preparation of alum, which was very important in dyemaking, and the technique of covering cannon with oil-soaked felt to prevent it from shattering from the heat of discharge; he also reports on the large-scale conversions of Christians in Anatolia and the Balkans. In what Magoulias recognizes as a vivid and exciting journalistic style Doukas supplies portraits of key historical figures, data on the ethnic backgrounds of some of the Ottoman leaders, and information on various Ottoman practices and customs. Doukas praises Murad II as a virtuous and gentle person, kind to Turks and Christians alike; he condemns Mehmet the Conqueror in a one-sided characterization; and he commends Constantine XI for his courage while refusing to consider him a legitimate ruler because he was crowned not by the patriarch, but by a metropolitan in Mistra.

Doukas recognized the duplicity of the Westerners and condemned the two Hungarians for aiding the cause of the Turks, but he was a Unionist aristocrat and believed that some accommodation with the West was necessary for Byzantine survival. A friend of the Genoese and a pro-Latin, Doukas resented the anti-Unionist commoners of Constantinople. In fact, he looked upon the members of the Orthodox national party as "schismatics."

In sum, Magoulias has turned into English a very important document, which is enriched by his insights and additions.

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MODERN EUROPE

WILLIAM H. MCNEILL. *The Shape of European History*. New York: Oxford University Press. 1974. Pp. vi, 181. \$7.50.

The author is very modest in describing his work as a "little" essay. In 120 pages or so he traces the history of Europe from antiquity through the Middle Ages and on to 1973. Locating the "main drive wheel of historical change" in human interaction—whereby strangers meet and, as a result of the encounter, modify "familiar ways of behaving"—McNeill sets out to map the shape of European history. Those who have read his earlier book, *The Rise of the West*, will not be surprised at his attractive design. One of the many virtues of McNeill's work is that he writes about the West with a generous appreciation for the contributions of other civilizations. Like Lopéz, Cipolla, and others, the author is altogether mindful of the way in which Europe appropriated technology, business organization, metaphysics, and even God. His essay examines the extent and velocity of cultural and technological change. The effort should be applauded, for the author includes many fields, from science and technology to art and literature. The impulse behind this synthesis is also admirable: the first two chapters describe some of the leading features of traditional European historiography and pose some methodological questions. McNeill suggests that the "inherited shape" of European history is no longer intellectually negotiable. The scaffolding fails to support a platform of plausible synthesis. Formerly, leading historians of Europe saw the history of that continent as the progressive unfolding of liberty, and surely such a view does not inspire confidence. McNeill discusses other traditional models, contending that the basic crisis of present-day historical writing is one shared by Marxists and liberals alike who see

history "as unilinear, climaxing in a more perfect liberty." He connects this crisis with the loss of historical intellectual authority of which two symptoms are dwindling course enrollments and a scarcity of academic posts. This essay, in which he searches out patterns of behavior by generalizing from a "limited body of information" and by making intuitive leaps, is in fact his response to the historian's dilemma.

McNeill is right. The feeling that problems of historical understanding have epistemological difficulties, not to be overcome by naive manuals counseling objectivity, seems to be pervasive. Perhaps Theodor Adorno's diagnosis is apt: "Consciousness has lost the power to think the absolute and to bear the conditional." One response to the dilemma is to follow Braudel, the *Annales* school, and our own delight in technology while retreating from any consideration of the possibility of political, religious, or moral intervention by humans in history. We can view past worlds in their relationship to nature and apply reductionist psychological constructs so that the individual can do little to shape history, but is condemned to respond to such external circumstances as climate, food supply, population pressures, and so forth. Such a view is dolefully attractive if we believe that we are soon to be overtaken by the triumph of totalitarianism in these, the last days of capitalism. Despising politics, we can turn to a study of material existence, seeing our civilization, like so many others, as a helpless victim of secular trends. We are then of course powerless to intervene politically. Dubious of the values of civilization and contemptuous of high culture, we can be fashionable à la Lévi-Strauss and exalt the instinctive, primitive, and spontaneous in an elegant ballet of revenge against the constraints of modernity. McNeill encourages us to do more: one can honor the impulse toward synthesis, and the battle cry need not be "Give Us Nineteenth-Century Liberty or Give Us Braudel!"

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BARRY GORDON. *Economic Analysis before Adam Smith: Hesiod to Lessius*. New York: Barnes and Noble. 1975. Pp. xiii, 282. \$27.50.

In this scholarly study, Barry Gordon has made a concise and unique contribution by describing and analyzing early economic thought in the Western world. He introduces the ideas, personalities, and events of ancient, medieval, and Renaissance periods to illuminate the content and scope of modern economics.

Historians, particularly those seeking background for the trend of Christian economic

thought, will find the depth of research and analysis especially useful. Gordon assumes, however, that the reader has knowledge of the wars, population shifts, intellectual changes, and new economic patterns. The economic ideas of many lesser-known writers are skillfully woven into the major pattern of each period.

In the first three chapters covering the Greek period many new writers and their philosophies are presented. The emphasis is on the conflict between the expansionist city-state approach of the Sophists and the opposing Socratic philosophy. In the latter, a careful analysis is given of Aristotle's method, since it dominated later medieval thinking. The early Biblical, Christian, and Jewish influences, particularly the Mishna and the Christian Fathers, are skillfully blended to produce a significant combination with Roman legalism, which resulted in the canon law of the Middle Ages. A high point of this scholastic thought is epitomized in the writings of St. Thomas Aquinas.

The last chapters are devoted largely to the gradual shifts in Scholasticism brought about by changes in the economy. The Venerable Leonard Lessius represents the most effective efforts to try to adjust to the modern era.

Do not expect to find the writings of early mercantilists, natural theorists, or physiocrats in this study. The mercantilists are relegated to a revival of the nation-state building of the Sophists and the physiocrats to a reactionary form of a just and prosperous society of medieval times.

The lack of a bibliography is somewhat compensated for by the excellent footnotes and an adequate index.

FRANK E. DYKEMA
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JOHN DEE. *The Mathematical Praeface to the Elements of Geometrie of Euclid of Megara (1570)*. With an introduction by ALLEN G. DEBUS. (Primary Sources from the Scientific Revolution.) New York: Science History Publications. 1975. Pp. 33, reprint unnumbered. \$8.95.

MICHAEL SEAN MAHONEY. *The Mathematical Career of Pierre de Fermat (1601-1665)*. Princeton: Princeton University Press. 1973. Pp. xviii, 419. \$20.00.

John Dee and Pierre de Fermat are not well known to historians. Yet both these savants played important parts in the Scientific Revolution; both, in contrasting ways, were fascinating characters and are at present undergoing reassessment. The differences between the two "mathematicians"—I use quotation marks to suggest the wider scope the discipline had in the sixteenth and seventeenth centuries than it has today—were in part environ-

mental. John Dee, the extroverted English Protestant, moved in court circles and was heeded by monarchs, whereas Pierre de Fermat, the introverted French Catholic, was a district magistrate who seldom met his peers and was unknown to the establishment. What they had in common was the strong sense of the importance of mathematics per se as a framework for the rational world and as a marvelous tool. (Dee was also a geographer and navigational expert who once held the exploration rights to all North America above the fiftieth parallel! Fermat, moreover, did some fine work in physical optics.)

Dee's reputation has always been tinged by his involvement in alchemy, magic, and divination (Queen Elizabeth I's coronation date was chosen on his auguries); consequently his solid work gets overlooked. Perhaps no single piece can better highlight that work than his "fruitfull Mathematicall Praeface" to the Dee-Billingsley English translation of Euclid's *Elements* in 1570. This facsimile, with an illuminating introductory essay by Allen Debus, is admirable.

No definition of mathematical genius could exclude Fermat, who, unlike Dee, was very creative. Michael Mahoney's thorough survey of Fermat's life and achievements is unique. Most of us know that the salients in that achievement were in the areas of probability, of what might be termed the metacalculus, and of number theory. Fermat's Last Theorem, whose provenance I have long averred is suspiciously obscure and ill-documented, has baffled generations of amateur and professional mathematicians. A hobbyhorse cannot be mounted here, however, so I shall merely predict that Mahoney's Fermat will not easily be superseded as a definitive text.

N. T. GRIDGEMAN

National Research Council of Canada

ELISABETH HANSOT. *Perfection and Progress: Two Modes of Utopian Thought*. Cambridge, Mass.: MIT Press. 1974. Pp. ix, 219. \$17.50.

JOHN MCKELVIE WHITWORTH. *God's Blueprints: A Sociological Study of Three Utopian Sects*. Foreword by DAVID MARTIN. Boston: Routledge and Kegan Paul. 1975. Pp. xiii, 258. \$23.00.

Perfection and Progress is an ambitious attempt to explain in their own terms the utopias of Plato, Thomas More, Johann Valentin Andreae, Edward Bellamy, H. G. Wells, and William Dean Howells. In one of several conflicting statements of her intent, the author maintains that her aim is not to survey utopian thought or to analyze all of its salient features, but rather to "describe certain changes" in the way utopias were constructed.

Despite her disclaimer, Hansot has indeed attempted to survey utopian "thought experiments," and she does engage in considerable analysis.

Her thesis is basically a gloss on a shorter work of Judith Shklar, who argues that with Rousseau a critical change in utopian thought occurred, from models for argument to programs for action. Hansot shows that the classical-to-modern shift occurred much earlier—in the seventeenth century—and deepens our understanding of the fundamental differences between the "two distinct utopian traditions." Authors shifted from using utopias as standards of judgment to writing action-minded, reformist critiques of their societies; from a concern with the nature and values of man to the institutional mechanisms and arrangements of society; from inability to act to the modern "recognition of [man's] ability to initiate social change and to use it for ends of his own devising." The contrasts are real, and the earlier dating of the change is convincing.

The thesis is persuasive despite Hansot's method—a nonhistorical, relentless series of *explanations de texte*, each author analyzed according to rigid categories and none placed in historical context. Moreover, her eccentric sample omits Campanella and all the influential French utopians. Above all, her reduction of twentieth-century utopian thought to H. G. Wells and W. D. Howells is a caricature. The inclusion of Huxley and Orwell might have forced her to confront a possible second shift in utopian thought, one that would have marred the neat symmetry of "two traditions."

Lacking in historical sense, narrow in method, and unjustified in its omissions, this book is a near failure. Joyce Hertzler's half-century-old narrative study still remains the best introduction to utopian thought.

The Whitworth book is less a failure than a cliché: another rearrangement of familiar historical materials to illustrate a behavioral theory. It often works, as in Erik Erikson's psychoanalytic interpretation of Luther. Like Hansot, Whitworth uses the respectable old device of a representative sample. But his three choices, unlike hers, are well suited to his purpose: the celibate Shakers, the less than celibate Perfectionists of Oneida, and the later but no less successful Bruderhof—the last serving as a kind of control group. A study in the sociology of religion, the bulk of the book consists of three extended historical summaries, based on printed sources, of each of the three sects. Tacked on are thirty-one pages of conclusions making some unexceptional comments on visions, leadership, social origins, sexual and family relations, and so on in the light of charisma, revitalization theory, repression-sublimation, and social control.

One welcomes the sociological analysis of com-

munitarian sects, but the book should have been written with a wider grasp of primary and secondary sources and a better melding of behavioral concepts and raw data.

MARIO S. DE PILLIS
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Amherst*

J. M. WINTER, editor. *War and Economic Development: Essays in Memory of David Joslin*. New York: Cambridge University Press. 1975. Pp. viii, 297. \$22.50.

This book undoubtedly achieves its contributors' main purpose: to honor the memory of M. M. Postan's successor in the Economic History Chair at Cambridge. David Joslin was a talented historian, a wise and helpful teacher, a valuable colleague, and above all a good man, which is well said in H. J. Habbakuk's short prefatory memoir. Joslin would be proud of his friends and pupils: their essays vary in length and originality, but some are very good indeed.

Edward Miller investigates with care and caution the incidence and effects of war taxation on English economic and constitutional development between 1294 and the 1340s, and he concludes that they were considerable. G. R. Elton describes how the English parliament during the sixteenth century became accustomed to demands for extraordinary taxation not only during and directly for war but for more general reasons of national security and prestige as well. Geoffrey Parker offers a persuasive balance sheet of the economic costs of the Dutch Revolt to the South Netherlands (that is, Belgium), the Netherlands "proper," and Spain; he concludes that the costs were very great. Peter Mathias points out how many of the problems and practices of the nineteenth-century public health movement were anticipated by the eighteenth-century army and navy medical departments.

Phyllis Deane concludes her statistical essay with the judgment that "in the last analysis . . . it would appear that the war of 1793-1815 . . . does not seem to have caused more than superficial fluctuations in the pace and content of the British industrial revolution." Simon Schama, in the most stylish of the essays, magisterially reviews the fiscal policies of the governments of the Netherlands during the eighteenth century and through the shocks of French occupation and dominance, and he argues that no lesser experiences could have sufficed to make the system of public finance adequate to the state's demands. Clive Trebilcock's powerfully original and striking essay insists that the failures of munitions procurement and supply during the first year of the Great War were all the less explicable and excusable for having been ex-

actly paralleled during the Boer War! Joe Lee's shorter chapter tells a similar tale of official failure: that of the German bureaucracy to anticipate, before the Great War, the food supply difficulties which war and blockade would bring with them. Roy and Kay MacLeod go very thoroughly into the Great War's exhilarating impact on the British manufacture of optical glass and its promotion of combined operations by government, science, and industry in that field. D. C. Coleman also applies himself to the 1914-18 government-science-industry nexus, telling the story of the manufacture of aeroplane "dope," a story differing from that of optical glass in its richly scandalous aspects and ambitions. Jose Harris does a cool job of demythologizing on Beveridge and his wartime report. The editor's contributions are a short, thoughtful essay, "The Economic and Social History of War," and a long bibliography of "the literature which relates to Western European warfare."

There is no doubt that there are gaps in this book, but, in the present state of war and society studies, it is extremely useful.

GEOFFREY BEST
University of Sussex

MARCELLO MAESTRO. *Cesare Beccaria and the Origins of Penal Reform*. Foreword by NORVAL MORRIS. Philadelphia: Temple University Press. 1973. Pp. xii, 179. \$7.50.

A solid and readable biography in English of Cesare Beccaria has long been needed, and Marcello Maestro's book is especially timely. The European eighteenth century—in a somewhat wider context than that of the overly-written-about French Enlightenment—is at last beginning to speak to the twentieth-century western world. Among those with a great deal to say to us is Beccaria. His modern sounding *On Crimes and Punishments* was published in 1764 and has influenced penology ever since. The most controversial stance assumed by Beccaria in that work was his call for the abolition of the death penalty; and, then as now, he was opposed by liberals and conservatives alike. His ideas for today are as sharp and relevant as when he first wrote that "the purpose of punishments is neither to torture a man nor to undo a crime already committed . . . the object of punishment is simply to prevent the criminal from injuring anew his fellow citizens and to deter others from committing similar injuries. Therefore, those punishments and that method of inflicting them should be chosen which, in due proportion to the offence, will produce the strongest and most lasting impression on the minds of men, and inflict the least torment on the body of the criminal. . . . In order that a punishment may attain its object it is

enough if the harm of the punishment exceeds the advantage of the crime, and in this excess of harm the certainty of punishment and the loss of the possible advantage from the crime should be included; all beyond this is superfluous and consequently tyrannical." While expressing enthusiasm for the spirit of Beccaria's work, Voltaire's supposed liberalism was unable to accept the Italian's abandonment of the use of torture and the death penalty. Kant, too, attacked Beccaria for his stance on capital punishment. But Beccaria did and still does influence penal reform, and one such influence was the Pennsylvania state law of 1794 which provided that "no crime whatsoever hereafter committed, except murder of the first degree, shall be punished with death in the state of Pennsylvania." One must remember that men living at that time in most English-speaking areas, as well as others, who stole a horse or even a bolt of cloth might well find themselves at the noose-end of a rope—legally.

Maestro's book has filled a gap in English historiography on the eighteenth century. It is a slight book, unencumbered by scholarly apparatus. To the general reader this is an advantage, but to the academician it is a disappointment. Beccaria's involvement in economic, political, and even educational reform is suggestively touched upon, but there is room for fuller treatment, and one connecting in greater detail with the whole European eighteenth-century scene.

LAWRENCE F. BARMANN
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CLARKE GARRETT. *Respectable Folly: Millenarians and the French Revolution in France and England*. Baltimore: Johns Hopkins University Press. 1975. Pp. x, 237. \$10.00.

Millenarianism, as defined by Clark Garrett in *Respectable Folly*, describes a range of beliefs rather than a clearly enunciated ideology. His heterogeneous group all shared traditional Christian beliefs in Christ's imminent return, His earthly reign, and the final judgment (thus excluding from consideration eighteenth-century Frankists and secular utopians). These individuals alternated between the hope that the millennium would soon come, the assumption that men working in harmony with God's plan could hasten its arrival, and the fear that either the millennium would be postponed indefinitely or that it would be preceded by the apocalypse.

Regarding comparative history as more revealing than a study of one culture, the author investigates the impact of the French Revolution on the hopes and aspirations of both English and French millenarians. This investigation reveals

some interesting patterns. Although from different social strata, all the millenarians described were alienated from their environment. All were "seekers" before the Revolution, and most (perhaps responding to the rapid changes of mid to late eighteenth century) had adopted millenarian beliefs before 1789. But Garrett rejects earlier interpretations of millenarianism as a precapitalist expression of social oppression, or collective insanity, or social alienation. For him, millenarianism was a means whereby individuals who remained within Christianity could assimilate and express in traditional forms the hopes and fears created by an era of widespread social upheaval.

Garrett's characters lived on the fringes of society and exerted little contemporary influence. The author, perhaps in order to demonstrate the significance of his subject, strays beyond his self-imposed bounds to discuss the use of millenarian terminology by more influential men who no longer accepted Christian assumptions. His conclusion that men can only express their hopes and fears in the contemporary idiom diminished any significance that might be seen in this common vocabulary. Although orthodox Roman Catholics and Anglicans denied the worldly interpretation of the Book of Revelations common among millenarians, a generalized belief in the possibility of an earthly paradise was widespread during the eighteenth century. Thus Garrett's book describes a small group who expressed common sentiments in somewhat more bizarre terms than their contemporaries. He fails to show why these individuals remained Christian and why, as he insists, this study adds significantly to our understanding of eighteenth- and nineteenth-century history.

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GEORGE DENNIS O'BRIEN. *Hegel on Reason and History: A Contemporary Interpretation*. Chicago: University of Chicago Press. 1975. Pp. 188. \$8.50.

A considerable number of works have recently appeared in English on Hegel. George O'Brien nevertheless sees a justification for his essay since no one has undertaken a careful textual analysis of Hegel's *Reason in History*, which he believes provides a key to Hegel's philosophy of history. The author wants to correct the interpretation, popularized by American and British analytical philosophers, of Hegel as a "metaphysical, speculative, antiempirical, a priorist" thinker (p. 2).

O'Brien wishes to demonstrate that Hegel was a historical thinker who stressed the role of individuality in history, not a philosophic monist as the modern analysts have claimed. Hegel would have

rejected the analysts' assimilation of thought with logical deduction, and therefore he should not be considered a speculative thinker. Hegel sharply distinguishes between explanation in history and explanation in nature, stressing that while nature is marked by regularity and repetition, history is characterized by innovation and takes into account the natural, man made, progressive character of historical reality.

If Hegel sees reason as the absolutely final purpose of history, he nevertheless understands the philosophic concept of history as nothing more than "actual history, properly understood" (p. 45). Hegel's doctrine of reason points to individuality rather than universality. His concept of causation, O'Brien believes, was deeply influenced by Aristotle. There is an inevitability in the progress of freedom from potentiality to actuality. But this inevitability does not exclude moral freedom. World history is thus not reducible to a predetermined schema. It is predetermined only in the sense that "as long as the underlying belief of society is that all men are truly free, then the course of history must be toward a form of social structure which adequately recognizes and expresses that belief" (p. 149). Hegel offers an understanding of history that is "at once empirical and a priori, individualized and yet leading to some achieved value" (p. 175).

This is not an entirely new interpretation. One may ask whether the very succinct, abstract presentation in *Reason in History*, edited from lecture notes after Hegel's death, can properly be understood without taking into account the body of Hegel's work that provides a more concrete expression of his view of history. O'Brien only occasionally considers passages from *Philosophy of History* and *Phenomenology of the Mind*, specifically the master-slave discussion. One may also ask whether the author has not underplayed the role of the "cunning of reason" in Hegel's philosophy of history, which subordinates the actions of individuals to the universal purpose of history. Nevertheless, the study serves as a useful and intelligent companion volume to *Reason in History* and a valuable corrective to the popular oversimplification of Hegel's philosophy of history.

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WILLIAM WEBER. *Music and the Middle Class: The Social Structure of Concert Life in London, Paris and Vienna*. New York: Holmes and Meier Publishers. 1976. Pp. 172. \$17.50.

The social history of music is a daunting field. Its ultimate goal must be to relate changes in music to

changes in society. But music is an essentially abstract art, with esoteric features. Sociologists rarely possess deep critical understanding of it, while musicologists tend to treat it as a world of its own.

William Weber disclaims any ambition to discover how "society affected the creative process itself." Instead, he has undertaken a minute examination of concert life in London, Paris, and Vienna during the critical decades of the 1830s and '40s. From a wide range of sources he has gathered an unprecedented volume of information, which confirms in fascinating detail several known phenomena of the period: the rivalry between the two elites formed by the old aristocracy and the new upper middle class; the widening gulf, cutting across class differences, between "classical" and "popular" music supporters; the growing independence, in matters of musical taste, of the lower middle class. The most surprising conclusion is that musical life in the three capitals, though differing in detail, was in outline much the same. It will no longer be possible seriously to describe Britain in this period as "das Land ohne Musik."

However, certain weaknesses illustrate the danger of writing about music without adequate understanding of musical styles. The author's sharp distinctions between segments of society may be justified, but no amount of scrutiny of newspapers and subscription lists can validate his equally sharp distinction between classical and popular music. Nowhere does he show awareness of the conflict between the aristocratic preference for older Italian music and the middle-class preference for modern German instrumental music. Yet this was still an issue in the period; it is particularly well documented for London. Weber has missed this point because he has categorized both types of music as "classical."

He can be faulted on methodological grounds also. His conclusion that the increase in musical activity "amounted to a real cultural explosion" is entirely based on a count of concerts mentioned in newspapers and journals, summarized in tables. But part of this "visible" increase is undoubtedly due to a rise in the advertising and reporting of concerts, which, of course, had its own related social causes. It would be wise to disentangle these contributing variables before talking of explosions.

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PETER N. STEARNS. *1848: The Revolutionary Tide in Europe*. (Revolutions in the Modern World.) New York: W. W. Norton and Company. 1974. Pp. 278. Cloth \$8.95, paper \$2.75.

At the 1975 meeting of the Society for French Historical Studies in Madison, Wisconsin, a session devoted to the historiography of the French Revolution drew an overflow audience of two hundred persons; a seminar to discuss work on 1848 in France drew six persons, of whom one was a student who thought he preferred diplomatic affairs to revolutions. No one would suggest that 1848 should have precedence over 1789 in interest or in research, but the revolutions of 1848 involved most of Europe and were not just riots. Why the neglect? Peter Stearns' book does not answer the question, but it provides the kind of review of the literature in the field that one needs as a starting point.

The fact that the Princeton University Press is still printing Priscilla Robertson's social history of the 1848 revolutions, which came out in 1948 in time for their centennial, suggests the paucity of good books covering more than one country. Stearns' book has the advantage of being, like the Langer paperback, shorter and more up to date. If the Stearns book makes no pretence of originality or research, and if the style is a bit pedestrian, it is a very satisfactory book. I particularly like the first chapters where he writes some sensible things about artisans and proletarians and middle-class liberals. His book was finished before some of the articles and monographs by young scholars who are working in the field of popular culture appeared, and one suspects that views on nineteenth-century workers will be altered substantially by the research of Joan Scott, Robert Bezucha, John Merriman, and others. My preference would be for more emphasis on the older chestnuts of liberalism and nationalism that seem to get less recognition in Stearns' book than is their due and often are ignored by the present generation of doctoral candidates. For Germany one thinks of Jacques Droz following in the footsteps of Veit Valentin—I know of no studies of the French, Italian, or Austrian revolutions of comparable scope—but one wonders who will be their successors and who, building on the work of William L. Langer, will write a major, in-depth, truly comparative study of all the revolutions of 1848. Peter Stearns cannot be faulted for doing what the editor of his series requested, but the challenge is still there for some mature historian to pick up.

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Continuing the elaboration of themes already well outlined in his *Il Socialismo prima di Marx* (1966), Gian Mario Bravo has collected in this volume a series of essays written and published separately between 1964 and 1969. Notwithstanding their self-contained character, however, the whole hangs together and provides a fairly extensive critical account of available works concerning a phase in the history of socialism that has remained relatively unexplored. Unlike the Second and Third Internationals, which have been studied in depth primarily because of their political consequences, the ambiguities of the First International have been largely ignored.

Through detailed and penetrating examinations of some of the best historical research in the field to appear during the past ten years, Bravo surveys some of the fundamental issues of the period and the most interesting figures. Thus, extensive reviews of Werner Kowalski's *Vorgeschichte und Entstehung des Bundes der Gerechten*, Edmund Silberer's *Moses Hess: Geschichte seines Lebens*, Renée Lambert's *Mouvements ouvriers et socialistes (Chronologie et bibliographie): L'Espagne*, and Arthur Lehning's three volumes of the *Archives Bakounine*, supplement the core of the book, an eighty-page bibliographical essay dealing with research carried out during the century following the foundation of the First International in 1864. For Bravo, "the First International represented in the last century a movement which, beyond the prejudiced judgments of frightened contemporaries, went beyond the very organizational schemes that it has posed . . . to assume a broader meaning: i.e., the insertion of the lowest social strata at the highest level of the class struggle" (pp. 170-71). Following Marx, he considers that phase of the history of socialism to have closed with the blood bath of the Paris Commune.

Questions concerning the relation of the various figures with the French Revolution, the connection with the birth of Zionism (in the case of Moses Hess), and the legacy that was bequeathed to the Second International have not yet been fully dealt with, and, most of all, a complete history of the International Workers' Association remains to be written. In undertaking any of these tasks, future historians will find Bravo's book an excellent bibliographical map to the literature on the subject.

PAUL PICCONE
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GIAN MARIO BRAVO. *Il socialismo: Da Moses Hess alla Prima Internazionale nella recente storiografia*. (Pubblicazioni dell'Istituto di Scienze Politiche dell'Università di Torino, number 23.) Turin: Edizioni Giappichelli. 1971. Pp. 285. L. 3,500.

NORMAN LEVINE. *The Tragic Deception: Marx Contra Engels*. (Twentieth Century Series.) Santa Barbara: Clio Books. 1975. Pp. xviii, 259. Cloth \$15.00, paper \$4.95.

The question that lies behind Norman Levine's *The Tragic Deception* is a poignant one: why has the anthropological vision of Marx's early communism not been realized in the goals and results of modern communist movements? The answer is that Engels misunderstood Marx's message and set his would-be followers on a wrong path. In reducing humanist Marxism to a mechanistic and unilinear theory of technological determinism Engels opened the way for the positivistic dogmatics of the Stalinists.

That Marx and Engels had different perspectives is not a new idea; yet probably no one before Levine has painted the contrast so sharply. It stemmed from "a void in Engels' intellectual equipment." Whereas Marx saw history as the developing locus of human praxis, Engels could only conceive a metaphysical subordination of men's purposeful creativity to outside powers. His mature equation of history with technological change matched his early "right Hegelian" identification of the active force as the Absolute Idea. Only Engels was a historical materialist: Marx was a "dialectical naturalist."

There is a kernel of truth in this. Marx and Engels came to communism in different ways and remained different people. Engels had little of Marx's philosophical imagination. The Paris manuscripts and the *Grundrisse*, both of which Marx kept to himself, might have surprised Engels. But the two friends did not live in different mental universes. Levine identifies Marx too closely with Feuerbachian humanism and separates Engels too strictly from it. He fails to take Marx's critique of Feuerbach seriously, or to consider the positivistic tendencies visible in his writings. The 1859 preface to *A Contribution to the Critique of Political Economy* is simply ignored, and the famous statement about windmills, steam-mills, feudal lords, and industrial capitalists from *The Poverty of Philosophy* is not even explained. The Feuerbachian elements in *The Condition of the Working Class in England* are not mentioned, and evidence which might justify the assertion that Engels "did not understand what Feuerbach meant by 'species being'" is not presented (what could it be?). The claim that Engels in 1844 "did not reject British classical economics" is simply untrue, and it hides Marx's original debt to Engels'—Feuerbachian—critique of political economy.

The level of discourse in this book is not high. Two pages separate the avowal that "Marx was attempting to surmount the Hegelian and idealist reduction of life to pure consciousness" from the (false) assertion that, for Marx, "only consciousness could have a history." One is equally puzzled to be told that "in Marx's lexicon, power had nothing to do with imperium, but dealt rather

with full development," and to learn that "Marx envisioned himself as the successor of Robespierre and Babeuf." *Hic Rhodus, hic salta* does not mean "Here is the rose, here dance," nor was that what Marx thought revolutionary conditions "cry out."

Levine asks the wrong question. Even if Engels had made of Marxism something Marx did not intend, it would not follow that he is responsible for what history has made of it. As Marx understood, the causes for such transformations lie deeper.

JERROLD SEIGEL
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FELICIAN PRILL. *Ireland, Britain and Germany, 1871-1914: Problems of Nationalism and Religion in Nineteenth-Century Europe*. New York: Barnes and Noble. 1975. Pp. 196. \$22.50.

One would not hear today, as I heard ten years ago from an American member of the Conference on British Studies, that one could teach modern British history and completely ignore Ireland. Gladstone's remarks in a letter from Germany in 1845 are applicable for the last 130 years: "Ireland, Ireland! That cloud in the west, that coming storm, the minister of God's retribution upon cruel and inveterate and but half-atoned injustice!" Not only Irish-Americans but dissident national minorities all over the world were influenced by and interested in Irish nationalism, including the many nationalities in Austria-Hungary, the founders of the Congress party in India, the Breton nationalists in France, and members of the Flemish national revival in Belgium, to name a few. Studies of the Irish Question in this dimension could be extremely valuable.

Felician Prill, former ambassador from the Federal German Republic to the Irish Republic, has published a well-written essay on the German response to the Irish Question. It is unfortunate that his effort is at best suggestive and in general very superficial. One wonders, for example, how the Bavarian view compares and contrasts with the Prussian. There is little analysis. One is only tantalized with a few quips in editorials and the marginal scrawls of the Kaiser on diplomatic dispatches. One learns that an address from the Orange Order during the Kulturkampf was viewed as unwelcome support by Bismarck and William I. The German papers often had marvelous insights into Anglo-Irish relations, such as an editorial on Asquith's role during the Ulster crises of 1914: "The Prime Minister is such a perfect *cunctator* that if he were asked officially what time it is, he would give an evasive reply either by postponing it to the forthcoming week or by requesting a question in writing for the next ses-

sion." While it is interesting to read that Bismarck compared the Parnellites to "our Fenians" in Alsace-Lorraine and Polish Prussia, it is regrettable that the contrasting attitudes throughout Germany are not explored in depth.

JOSEPH M. HERNON, JR.
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GRACE P. HAYES. *World War I: A Compact History*. Introduction by R. ERNEST DUPUY. New York: Hawthorn Books. 1972. Pp. xii, 338. \$9.95.

CORRELLI BARNETT. *The Swordbearers: Supreme Command in the First World War*. Reprint. Bloomington: Indiana University Press. 1975. Pp. xv, 392.

The unabated flow of books on World War I demonstrates the spell the war continues to cast. The main area of interest remains military events, which these two studies examine in quite different ways. While claiming to offer a "vivid and dramatic account," Hayes' virtues lie more in clarity, conciseness, and coverage. Within relatively limited space, she discusses not only the familiar western front but also the Russian, Italian, and extra-European campaigns. The general metamorphosis of the war from mobility, through frustrated efforts to break the stalemate, to final battles emerges clearly. The author's approach is not analytical; she presents a narrative of established rather than innovative interpretations. Though comprehensive, the book deals briefly with particular events and individuals. Standard secondary sources are used; the facts are accurate; and the maps are helpful. In sum, Hayes provides a useful introduction to the war.

Barnett concentrates on four commanders and their major campaigns: von Moltke and the Marne, Jellicoe and Jutland, Pétain and the 1917 mutinies, and Ludendorff and the 1918 offensive. His engaging style makes men and events come alive, though sometimes he lapses into purple prose and theatrical terminology. Focusing on military events and personalities, he manages to relate them to political decisions, social structure, and technological developments. The account, based mainly on secondary sources, is historically accurate and is embellished with excellent maps and numerous photographs.

The author questions established judgments. Much history of the war has been involved with "might-have-beens"—the possibility that individual decisions or coincidental events might have altered the course of the war. Barnett generally rejects this view, arguing that the outcome of supposedly critical events was largely determined by circumstances. The war broke out as a result of "international problems insoluble in a world of

sovereign powers except by war" (p. 11). The German failure to gain a decisive victory at the Marne occurred because "the two sides were at rough parity of physical and moral forces," less because of immediate decisions than long-term developments such as German technical training and above all lack of actual war experience since 1870 (p. 96). After the Marne, neither side could win on the battlefield. The Central Powers and the Allies "were in a state of balance in terms of military and industrial power," the origins of which "lay in the long and roughly equal preparations of each side for war" (p. 97). British failure to win at Jutland was likewise attributable not so much to "accidental [events] . . . , bad luck . . . , [or] blame on individual officers . . . [as to] a vast process of dissolution that began about 1870" in British technology and society (p. 188). The German attempted breakthrough in 1918 was likewise virtually doomed from the start because of insufficient superiority (pp. 295, 297, 301). Even had it occurred, Allied leaders would probably not have surrendered since they could count on imminent American aid (p. 282). Above all, "once the war had begun, it moved according to its own inner dynamics, unrelated to policy or intent" (p. 208).

It is unfortunate that Barnett obscures this thesis by declaring in his introduction that "the theme of this book is the decisive effect of individual human character on history" as shown at moments "when the course of the war pivoted on [individual] judgement and will" (p. xv). "Vast historical consequences depended on Moltke's qualities as a man and as a commander . . . for he alone of all the sanguine generals of 1914 enjoyed a genuine chance of making it a short, old-fashioned war" (p. 19). Jutland "might have broken the deadlock" (p. xv). This tendency may stem from a desire to attract readers by heightening the dramatic, but it does so at the cost of analytical consistency. The outcome of battles was determined either by general circumstances or "individual human character," but not both.

Barnett's ambiguity is nonetheless useful in demonstrating the need for clarity. It is necessary to distinguish between the hopes of participants and the actual possibilities as evaluated by historians. Historical importance should not be confused with drama. Turning points such as great battles must be distinguished from developments which caused and perhaps determined them. Above all, in calling attention to the military, political, technological, and societal constrictions upon military commanders, Barnett encourages re-examination of the often facile condemnation of World War I strategists.

L. L. FARRAR, JR.
Boston College

JOHN WHEELER-BENNETT. *Knaves, Fools and Heroes in Europe between the Wars*. New York: St. Martin's Press. 1975. Pp. 200. \$10.00.

Sir John Wheeler-Bennett's monographs and biographies have always had a veiled element of the memoir about them. This, the last book before his death, is frankly a memoir, and an excellent one. It covers the first half of his life, concentrating on experiences in Germany, Austria, and Czechoslovakia between the wars.

The work is in part analytical, but chiefly anecdotal and personal, effectively drawing individual portraits. A moderate conservative with champagne tastes, a love of horses, and a private income, the author seems to have fancied himself the Lord Peter Wimsey of high diplomacy—a serious dilettante desiring proximity to the centers of power, expert at assessing the actions of the great and near great; someone who moved comfortably in high society and had access to Lord D'Abernon, Brüning, von Papen, Gröner, von Seeckt, Dollfuss, Jan Masaryk, Beneš, and many others. He generally drew the line, however, at Nazis, whom he despised.

Sir John records some of his surprises, such as how he found the exiled Trotsky almost friendly, yet for the first and only time he felt himself "in the presence of someone who was motivated largely by hatred . . . abstract hatred" (p. 104). In one of the book's best anecdotes the exiled Wilhelm II described how on Sundays during his boyhood, when he was with his imperial grandfather, the latter would take up a bottle of champagne, pour himself a glass, one for the boy, and in time another for himself. "Then he would carefully re-cork the bottle and mark the height of the contents on the label with a pencil" (p. 187). How the butler must have cherished the emperor's Prussian thriftiness!

Former President Beneš told Wheeler-Bennett how in the Munich crisis the Soviet minister repeatedly conveyed escalating assurances from Stalin and Litvinov that the USSR would stand by Czechoslovakia, regardless of French action. Beneš added that he gave in to the Munich diktat not because of nonsupport from his allies, but because key elements in his own state—chiefly the army and the Agrarians—would not risk securing Russian support exclusively and indeed preferred the Germans to the Russians.

All in all, this is a memoir that is always interesting, often absorbing, and occasionally startling.

EDWARD V. GULICK
Wellesley College

MICHAEL ARTHUR LEDEEN. *Universal Fascism: The Theory and Practice of the Fascist International, 1928-1936*. New York: Howard Fertig. 1972. Pp. xxi, 200. \$9.50.

NICOS POULANTZAS. *Fascism and Dictatorship: The Third International and the Problem of Fascism*. Translated from the French by JUDITH WHITE. Translation editors JENNIFER and TIMOTHY O'HAGAN. Atlantic Highlands, N.J.: Humanities Press. 1975. Pp. 366. \$16.50.

GERHARD SCHULZ. *Faschismus-Nationalsozialismus: Versionen und theoretische Kontroversen, 1922-1972*. Berlin: Propyläen. 1974. Pp. 222.

The phenomenon of fascism continues to provoke an inordinate variety of scholarly interest. It is still possible to find an author (Michael Ledeen) able to plunge enthusiastically into an examination of the "emotional element" in the doctrine of fascism. Another (Gerhard Schulz) can put together a whole book of synopses of the historiography of fascism-National Socialism. And a third (Nicos Poulantzas) has spun out webs of convoluted rhetoric in an attempt, welcome only to the already initiated, to bring the structuralist message of Lévi-Strauss and the reshaped Marx of Louis Althusser to bear upon, of all things, the Comintern's mistakes in its analysis of fascism.

Ledeen provides a well-documented study of the doctrines known as *Fascismo Universale* that were in vogue with younger Fascist intellectuals in the late 1920s and 1930s. He holds that the generation coming to maturity during the first decade of Mussolini's rule became disenchanted with the achievements of the Duce, in particular because he had failed in the mission of transforming the world through the spread of fascism on the energies of youth. This criticism, Ledeen shows by detailed study of the plethora of journals of the movement, was expressed openly, though in the required Fascist mode, by older theorists like Giuseppe Bottai and younger activists like Gastone Spinetti and Mussolini's own son Vittorio. The doctrine, however, was as mushy in its themes as most of the rhetoric of Italian fascism; and Ledeen fails, though through no fault of his own, to provide proof that there was a serious political doctrine available for export. The failure of the Congress of Montreux, organized in 1934 by the Action Committees for Roman Universality to prepare the way for foundation of a Fascist International, was only to be expected.

Ledeen provides an interesting glance at side-lights of the Mussolini regime, such as the School of Fascist Mysticism, and he gives a glimpse into the thinking of a restricted group of younger Fascist leaders. But it would have been far more valuable if he had provided a genuine study of a generation, an analysis of the cohort with which he seeks to deal that would go far beyond quotations from obscure journals.

Schulz has impeccable qualifications for producing a historiographical synthesis of writing on fas-

cism and National Socialism. He is Professor of Contemporary History at the University of Tübingen and author of several well-received books on the Weimar and Nazi periods. The present book, however, is something of a disappointment. He admits that this compilation of summaries of interpretations is a *Nebenarbeit*, the necessary accompaniment to the writing of a bigger book. One cannot avoid the impression that the author felt that since he had to read through all these works anyway, he might as well put his notes together as a book.

Schulz makes insufficient effort to analyze the historiographical trends within the fifty years he is considering. No pattern of synthesis emerges in his treatment, in part because he wishes on principle to place his authors in their historical context. Had he chosen to organize his materials around themes like those suggested by Karl Bracher in the opening chapter of *The German Dictatorship* (1972), we would have had a more useful study. Instead, Schulz limits himself to giving, one by one, a brief exegesis of the opinions expressed by almost two hundred authors. But his treatment is very uneven. His primary interest is in the 1930s. As a result, the early writers on fascism, such as Luigi Salvatorelli, receive only brief mention, while the vast mass of writing since 1945 receives even shorter shrift. By contrast there is extended treatment of such authors as the Flemish socialist-turned-nationalist Hendrik De Man or the secessionist German socialist Fritz Sternberg. The author unfortunately makes little effort to place these figures in a historical context, either by nation, by ideology, by methodology, or by intervening historical event. The book is thus a cursory introduction to a number of neglected writers of the interwar years. It is not the needed historiography promised by its title.

If occasionally one wishes Schulz would give us more, with Poulantzas one frequently wishes he would stop. This is a book for the inner circle, the disciples of that little group of political philosophers who gathered around Louis Althusser at the École Normale Supérieure in the 1960s. At that time Althusser, a distinguished teacher in that hothouse of the French intellect and a member of the Central Committee of the Communist party, was arguing that the lessons of the structuralist movement in linguistics and anthropology could be applied to a fresh study of Marx. In two intricate studies now published in English as *For Marx* (1969) and *Reading Capital* (1970), he proclaimed the uncompromisingly dogmatic Marx, a structuralist before his time, as George Lichtheim has shown in a delightful essay in *From Marx to Hegel* (1971). Poulantzas established his credentials with the Althusser group in 1968 with *Political Power and Social Classes* (English translation, 1973), an enormously involved study of the capitalist

state. For the historian, *Fascism and Dictatorship* is a more interesting book. The author's method is to pose a series of theses and then to take concrete examples from Germany and Italy as proof of the correctness of his views. For example, in his chapter "Fascism and the Class Struggle," he posits that fascism is a specific stage in the rule of the dominant classes. Finance capital or big monopoly capital enters into conflicts with the other members of the dominant classes. It attempts to assert its pre-eminence in the political sphere. There is a breaking of ties between the existing political parties and the newly assertive forces of finance capital. A crisis occurs in the dominant ideology, which further strains the relationship between the political parties and the makers of ideology. The working classes are defeated in a trial of strength, and the dominant classes in turn go on the offensive. The Fascist party comes to power because it can mediate the contradictions within the dominant classes and between the dominant classes and the dominated classes.

Throughout the book this form of analysis is accompanied by a study of the mistakes of the Comintern, on the grounds that an understanding of its policy and principles not only illumines the nature of fascism but also explains the situation of the labor movement in Europe that still bears the stamp of the Comintern. The later chapters contain detailed studies of the dominant classes, the working class, the petty bourgeoisie, and the inhabitants of the countryside.

At the end, when the constant repetition, the wearisome asides, and the Marxist jargon are removed by the reader, the historian finds few ideas that are suggestive of new ways of interpretation. He may, however, come away with a greater understanding of the Comintern. He will definitely better comprehend what is fashionable today among one section of the Paris intelligentsia.

F. ROY WILLIS
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Davis

CHARLES S. MAIER. *Recasting Bourgeois Europe: Stabilization in France, Germany, and Italy in the Decade after World War I*. Princeton: Princeton University Press. 1975. Pp. xiv, 650. Cloth \$27.50, paper \$11.50.

MARTIN KOLINSKY. *Continuity and Change in European Society: Germany, France and Italy since 1870*. New York: St. Martin's Press. 1974. Pp. 234. \$16.95.

Charles S. Maier has written a long and remarkable book in which he asks the reader to rethink the years 1919-29 and to accept his cogently argued thesis that "rescuing bourgeois Europe meant recasting bourgeois Europe." The First

World War and its aftermath, he maintains, led to less social and economic leveling in Western Europe than might have been expected; prewar political and economic elites staved off all threats and were restored to power, albeit in a changed way. It is this change, the "recasting" of bourgeois Europe, that he examines in this exhaustive individual and comparative study of the three West European countries.

The fulcrum of decision-making shifted, he seeks to demonstrate, from parliamentary political parties to organized interest groups representing the industrial, commercial, labor, and agricultural classes, all more concerned with economic self-interest than with political issues and willing to desert older political allegiances; the line between economics and politics became blurred, and everywhere there was an interpenetration of state and economy. To describe the phenomenon, despite some understandable misgivings of the term for the Italian experience, he uses the word corporatism. In development of his thesis he analyzes closely—this is no narrative history—internal and external sociopolitical conflicts over such issues as nationalization, taxation, public finances, reparations, and tariffs. No one will close the book without a deep respect for Maier's mastery of detail, his ease in handling the comparative history of the three separate countries, his impressive familiarity with the monographic and archival literature, and his arresting conclusions. The equilibrium he describes, and the concomitant preservation of the European class structure, would be important but less striking if it were not the case that the arrangements of the 1920s, interrupted though they were by the Depression, the emergence of the Third Reich, and the Second World War, foreshadowed the political and economic settlements of post-1945 Europe, in a sense ushering in a "transformation of capitalism" that has lasted for over half a century. It is no small accomplishment to have reassessed in this thorough and original way the generally unsung conservative achievement of the 1920s.

In the second volume under review a political scientist at the University of Birmingham assesses "continuity and change" in European society by examining the three major West European countries in the years from 1870 to the present. With the disastrous experience of fascism in Italy, Nazism in Germany, and the Vichy regime in France in mind, he asks whether democratically constituted political systems can hope to operate successfully in environments where nondemocratic traditions are strong. For developments since 1945, the most rewarding part of the book, the author's conclusions are sobering; he sees, with variations for each country, a failure to sustain a democratic center, the division and alienation of the labor

movement, the blocking and postponement of reforms, and the continuity of economic and administrative elites from the past.

Kolinsky's study as a whole reveals no startling new insights or conclusions, but represents a fairly conventional, though sensible, summary of major European developments written with a sympathy for labor and liberal aspirations. No new ways emerge of viewing the last hundred years, nor is there any effort to reshape conventional periodization. The exclusion of the German Democratic Republic, though justified on the ground that the book deals with "self-determining" societies, limits the usefulness of the study. The professional historian will be unhappy with the sketchy documentation, the poor editing, the spotty bibliography, and the ignoring throughout of a score of key scholarly writings on France, Italy, Germany, and European integration published in the past ten years.

JOEL COLTON
Rockefeller Foundation
and Duke University

ROLAND N. STROMBERG. *After Everything: Western Intellectual History since 1945*. New York: St. Martin's Press. 1975. Pp. 274. Cloth \$12.95, paper \$4.95.

In many ways this is an impressive book. In nine short chapters Roland Stromberg has undertaken an amazingly comprehensive survey of major trends in recent philosophy, literature, art, music, the social sciences, political attitudes, and social patterns on a broadly Western scale.

Yet this is more than a useful supplement to Stromberg's *Intellectual History of Modern Europe*. It is a critical examination of the quality and direction of civilization in a postmodern world. Stromberg's thesis that "the whole pervasive life-world of urbanized, democratic, technologized man germinates the doctrine of incessant mobility" (p. 90) and the search for "incessant novelty" (p. 117) is not new. He carries this thesis to the extreme conclusion that all common values of society are in the process of collapse, that intellectual creativity has run its course, and that the art of modern times, as well as literature and philosophy, have "become absurd, meaningless, rooted in nothing except the stray and puerile insights of rootless individuals" (p. 94). "Mere anarchy is loosed upon the world" as the new "barbarians rise from the catacombs" (p. 235).

The pervasiveness of this thesis leads to a certain one-sidedness in presentation. Stromberg is a political conservative, who however realizes that no return to the past is possible. He maintains a special ire for welfare economics, Marxism, and the New Left. He fails to consider the economic

and social problems of modern industrial societies that have led to these responses. Marxism is treated as an economic superstition of the nineteenth century that has little relevance to the analysis of the modern social order. The importance of the New Left is very much overstressed; the political aims of the protests, such as occasioned by the Vietnam War, are virtually overlooked; and members of the student movement are portrayed as "barbarians from within," "potential destroyers of a weakening civilization" (pp. 141-43). Even the mild concern of secular humanists or Christian theologians with problems of social reform is berated as a dangerous betrayal of their intellectual and religious integrity.

This tendency to oversimplify the causes and manifestations of modern malaise detracts from the seriousness of a basically informative, stimulating book that deserves to be read and discussed.

GEORG G. IGGERS

*State University of New York,
Buffalo*

GEORG G. IGGERS. *New Directions in European Historiography*. With a contribution by NORMAN BAKER. Middletown, Conn.: Wesleyan University Press. 1975. Pp. ix, 229. \$16.00.

This new book confirms Georg Iggers' place as the foremost guide to the historiography of Continental modern Europe. He established this position in his earlier volume, *The German Conception of History*, in the Ranke anthology he co-edited and introduced with Konrad von Moltke entitled *The Theory and Practice of History*, and in assorted articles, of which the most general is the essay on historicism in the *Dictionary of the History of Ideas*. None of these earlier studies ignored recent historiography, but they focused on what might be called the classical period of modern historical literature—that is, the "scientific" history of the nineteenth century and its modulation around the turn of this century—with more current tendencies serving as its diffracted conclusion. Now the author has reversed the emphasis. In the book under review he does not neglect the nineteenth- and early twentieth-century background, but this context is now prolegomenous to Iggers' main concern with the striking and provocative innovations in historical thinking and historiographical practice that have marked our own generation.

It is important to understand what Iggers means to include and exclude, for his book is not intended to survey the entire spectrum of recent historiography. Consonant with his stress on the autonomous "scientific" strand of nineteenth-century historiography and its end-of-the-century crisis, Iggers' "new directions" are selections from the

three most prominent European efforts to retrieve the scientific claim of history—the structural history of collectivities associated with the *Annales*, the social history of politics, and the professional sophistication of Marxism. In Iggers' laudably qualified approach to historiography he sees it not as an insulated set of technical methods but rather as an expression of the cultural era to which it belongs, and by dint of this contemporary influence, he tends to identify scientific history with social history (taken in the intermediately broad sense to include economic history). The author's persistent references to the institutional conditioning of historical scholarship lead him to define historiographical tendencies in national terms: structuralist history is French; the social history of politics is West German; even Marxism, superficially transnational, is suborganized into separate East German, East European national, and French sections, with the British variant segregated in a separate contribution by Norman Baker.

Within these self-imposed limits, Iggers' analysis exhibits all the virtues we have come to expect. It is thorough in its coverage, apt in its choices, penetrating in its perceptions, balanced in its judgments, and thematic in its construction. The theme accommodates the blend of sympathy and criticism that is the hallmark of the book, and it provides a substantive unity to the parts over and above the formal coherence supplied by the emphasis on scientific desiderata and the social dimensions of history. Iggers' theme is the historiographical necessity of joining general truths, delivered by the methods and concepts of the new social history, to the individual life of men, events, and ideas, which has long constituted the truth of traditional history. Not only does this theme work in that it makes an authentic book out of four component essays, but, in my opinion, it is precisely right: the juncture of generality and individuality remains the crucial task of modern historiography.

Because the "European" in Iggers' work patently refers to historians rather than to history, we of American vintage are not immediately, but indirectly, affected by it. Those of us who are not participating in the kind of historiographical movements that Iggers covers will perhaps learn more from him than those who are. But his focus also leaves those not engaged in the new scientific social history with a final ambivalence, with a feeling of unease at being out of the swim, and with a sense of pride at our nonconformity.

LEONARD KRIEGER
University of Chicago

J. R. RAVENSDALE. *Liable to Floods: Village Landscape on the Edge of the Fens, AD 450-1850*. New York:

Cambridge University Press. 1974. Pp. xi, 206. \$15.50.

Thanks to the earlier pioneering work of Professors Hoskins, Finberg, and Beresford, much is now known about the general history of the English landscape and the marks that man has made upon it. In more recent times the general survey has given way to the regional study, often that of a county, sometimes that of a district with a particularly interesting tale to tell. Thus M. Williams' interesting monograph showed how the marshes known as the Somerset Levels were drained and became a highly productive agricultural area. Now J. R. Ravensdale has published for comparison a very valuable study of changes in Cambridgeshire.

Liable to Floods is a detailed topographical and social history of village landscapes on the edge of the Fens from 450 to 1850. The villages are Landbeach, Waterbeach, and Cottenham, whose very names stimulate the reader's interest. Wisely taking a long period, Ravensdale analyzes these three communities' management of their resources: water, fen, meadow, pasture, and arable. He demonstrates the changing importance of such occupations as fishing and fowling, as well as the eagerness of the fenmen to defend their customary rights. He shows how the communities responded to changes in both population and the water level, and he analyzes the field patterns and the topography of the villages themselves.

This is, then, a very interesting book that includes much detail, not all of it well digested or elegantly composed, though it remains true that in works of this kind it is detail that matters. Although the author has tested a number of accepted generalizations about the Fens, it is a pity he has seen few opportunities for comparison with other localities. Nevertheless, his conclusions are both carefully drawn and satisfying. The value of this scholarly monograph is enhanced by its thorough documentation and its clear, informative maps.

GORDON C. F. FORSTER
University of Leeds

FRANCIS PIERREPONT BARNARD, editor. *Edward IV's French Expedition of 1475: The Leaders and Their Badges Being MS. 2. M. 16. College of Arms*. Reprint. Gloucestershire: Gloucester Reprints; distrib. by Rowman and Littlefield, Totowa, N.J. 1975. Pp. xv, 162. \$17.50.

Edward IV's expedition to France in 1475 was the largest any English king had ever led to the Continent, but it was inconsequential for it returned home without striking a blow after Charles the Rash of Burgundy deserted his English ally. The book under review, a reprint of a 1925 publication,

is not an account of that expedition but a listing of the leaders, the number of their men, their pay, and their badges.

The book is based on an eight-page manuscript belonging to the College of Arms, which is both reproduced in facsimile and transcribed. The remainder of the volume consists of biographical sketches of the sort found in the *Dictionary of National Biography*. "Biographical" is perhaps the wrong word, although it is used on the dust jacket (substituting for the author's "personal," which is not better), for the short pieces consist of parentage and provenance, wives' names and origins, titles and offices held, battles at which they were present, and other dry bones, useful for reference, but enlivened only very occasionally with a tiny bit of flesh and blood. The last few entries include clergy, kings of arms, ordnance specialists, and engineers. An earlier document provides the names of physicians and the king's secretary.

It is the armorial badges that really interest the author, although not this particular reader. Almost all the entries in the manuscript contain a description of the leader's badge and a crude drawing of it. Along with each biographical sketch is another entry describing the leader's badge (a black bull, a silver scallop, a blacked ragged staff, and so forth) with its derivation (a pun on the surname or place name, from the wife's family, and so on) and other armorial information.

Both biographical sketches and essays on the badges are done with a great deal of erudition, which many historians of the new school will no doubt feel might have been expended toward a more significant end. Even they may find the work of some use, however, because it gives the organization and pay of a very large, late medieval army.

DONALD E. QUELLER
University of Illinois,
Urbana-Champaign

ALISON HANHAM. *Richard III and His Early Historians, 1483-1535*. New York: Clarendon Press, Oxford University Press. 1975. Pp. vi, 230. \$23.00.

Fans of Josephine Tey's *Daughter of Time* and Fellows of the White Boar notwithstanding, Richard III should now be placed back into history's rogues gallery. With far more detailed detective work than Tey's imaginary inspector, Hanham presents a critical portrait of Richard based on accounts, which could not be distorted by Tudor propaganda, written before Bosworth. Horace Walpole's essay of 1768 (*Historic Doubts on the Life and Reign of King Richard the Third*) accused Henry VII of employing his reign "not only in extirpating the house of York, but in forging the most atro-

cious calumnies to blacken their memories and invalidate their just claim." Now that Walpole's hypothesis has been disproved, his criticism of blind, dogmatic historians can be applied to the "fanatics of his own party." The obscurity of Richard's reign is mainly the result of the paucity of fifteenth-century records. It is doubtful whether "consistent distortion for political and dynastic ends" can be charged against any of the historian-chroniclers Hanham examines. The serious distortion occurs in Shakespeare's *Richard III* where, for example, the playwright turns crooked shoulders into a hunched back.

Hanham's book is mainly a series of historiographical essays on the historians and chroniclers who wrote about Richard III between the time of his usurpation in 1483 and the death of Thomas More in 1535. Following several of the essays is an excursus or kind of biblical exegesis of the documentation. The most fascinating chapter, "Sir Thomas More's Satirical Drama," analyzes the philosopher-saint's *History of King Richard the Third*, which many literary critics and historians consider factual because of "the superior illusion of reality that is the product of art." More's use of parody can be subtle and tricky, especially if taken at face value.

Hanham's study requires careful reading and a good knowledge of the period. With its skillful probing and speculation, however, it is a model historiographical essay.

JOSEPH M. HERNON, JR.
University of Massachusetts,
Amherst

HENRY ANSGAR KELLY. *The Matrimonial Trials of Henry VIII*. Stanford: Stanford University Press. 1976. Pp. xii, 333. \$15.00.

Henry Kelly's study concentrates on neglected procedural and canonical aspects of the matrimonial trials of Henry VIII. The largest portion of the book examines the king's efforts to obtain a divorce from Queen Catherine of Aragon, but it also includes accounts of litigation leading to the dissolution of his marriages to Anne Boleyn and Anne of Cleves. Throughout, the author concerns himself with issues of canon law rather than with the conflict of personalities that makes the king's "Great Matter" one of the great dramas of the sixteenth century. Kelly contends that the trials have never been properly understood because the implications of Henry's "relationships with Catherine and Anne have been improperly evaluated" and "key documents detailing his program of justification have been ignored or forgotten." He argues specifically that the records of the three trials convened in England to pass judgment on the

king's marriage to Queen Catherine have been neglected. According to Kelly, J. S. Brewer, the nineteenth-century editor of *Letters and Papers of Henry VIII*, was the last person to study the official record of the trial initiated by Wolsey in 1527. Twentieth-century historians have likewise ignored manuscript records of the trial convened by Wolsey and Campeggio in 1529 and Cranmer's trial in 1533.

The author, in re-examining the king's much-studied conscience, finds evidence in Cranmer's *Articuli Duodecim* that Henry was deeply troubled by memories of his wedding night when he found Catherine a virgin. The resourceful Cranmer salvaged the king's conscience with the argument that her previous marriage to Prince Arthur could have been consummated with carnal copula even without the loss of her irrecoverable virginity.

Kelly reveals the complexity and ambiguity of late medieval canon law affecting marriage, and to at least one reader makes Henry's vexation with the Church more understandable.

BARRETT L. BEER
Kent State University

G. D. RAMSAY. *The City of London in International Politics at the Accession of Elizabeth Tudor*. Totowa, N.J.: Rowman and Littlefield. 1975. Pp. viii, 310. \$21.50.

Many historians of Tudor England have long been concerned with the problem of the national government's finances and the Tudor price revolution and inflation. We now have a detailed study of how the government of Elizabeth raised funds and made fiscal policy prior to the rise of Sir Thomas Gresham as the Queen's royal agent. This work, however, is more than just a financial account, and it is more than an account of royal finances. G. D. Ramsay deals with the city of London and its merchants and with private as well as public funding that established the base for the extensive growth of both London's and England's foreign trade.

This work is part of a projected study on the ending of the Antwerp Mart as the center of international finance, and it fills a major gap in our knowledge of the period. Starting with an analysis of Antwerp at its height and with London as a "satellite" city, the work deals with the political and diplomatic background of the attempts of London and its merchants to establish a national domestic money market. It then treats the larger aspect of Anglo-Netherlands politics and trade. Attention is given to various attempts to establish alternate financial centers both to the north along the Baltic and to the market at Emden—all attempts to cut into Antwerp's control.

Ramsay's study is an excellent start and, we hope, will be followed by the much-needed and long-overdue studies of Gresham, the shifting of international financing that began in the late 1560s, and the gradual establishment of London as an important money center. We have in this volume a well-documented study of the initial phase of this change. It is regrettable that the price may discourage some from obtaining this study, but graduate students and others interested in this problem will not err in starting with this book to learn about early Elizabethan international finance.

M. C. ROSENFELD
Southeast Massachusetts University

S. J. WATTS with SUSAN J. WATTS. *From Border to Middle Shire: Northumberland, 1586-1625*. Leicester: Leicester University Press; distrib. by Humanities Press, Atlantic Highlands, N.J. 1975. Pp. 288. \$21.75.

Despite being "altered and rewritten," and although improved by studies on geography, agrarian economy, and enclosures by the author's wife, this book has not progressed beyond the useful dissertation that preceded it.

All students of the local history of Tudor and Stuart England will welcome the facts on border institutions, the description of land tenures and regions (and how these affected peace and productivity there), and the identification of eighty-nine heads of the leading gentry families. This covers about half the book and constitutes "the economic and social situation" in 1603. But readers will look in vain for any detailed survey of the actual lands—their locations and tenurial conditions—belonging to particular gentlemen, or for any hint that landed wealth had something to do with local factions, marriages, offices, and ancestors.

The authors show the connection between royal favor and landed wealth, but they do not analyze sufficiently this connection so that one can determine how eighty-nine families became the leading ones. Moreover, they give no indication of the extent of their evidence, without which their conclusions must appear impressionistic. And what of the aristocracy and the merchants? The former are dubbed inconsequential because nonresident, while the latter soiled themselves in the coal trade. But had the Percys no local patronage after the late 1580s? Did none of the twenty-three armigerous Newcastle burghers have landed ambitions or business dealings with the gentry? It is good to have the demonic myths about Northumberland Catholics laid to rest, as the authors do, but how well did Catholics integrate into the social, economic, and political hierarchy of the county? Such questions have not been adequately explored.

In addition, unless the personal basis of wealth, local power, and rivalries are set out more systematically, one cannot make sense of the "complex and often devious political history of Northumberland from 1586 to 1625," which is the subject of the second half of the book. What should have been a decisive integration peters out in following the shuffling of officers on the commissions of the peace, tiresome border disputes, and the machinations of the Privy Council in far away London.

FRANK F. FOSTER
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 Denver*

DEREK HIRST. *The Representative of the People? Voters and Voting in England under the Early Stuarts*. New York: Cambridge University Press. 1975. Pp. xii, 306. \$21.00.

Derek Hirst has written the first book-length study of "voters and voting" in early Stuart England. Divided into three parts, "The Electorate," "Elections," and "After the Election," and with eight appendixes, his book identifies the 1620s as "the decisive period" in the development of the electoral process. The research is impressive and the bibliography useful. Hirst has discovered pertinent material, including little-known corporation records and voting lists, in all major national archives in England and in local archives in each county.

Hirst's conclusions are avowedly "contentious." Contemporaries were confused and uncertain about the composition of the electorate and the electoral process. The size of the electorate was much larger than scholars have thought, hypothetically "some 300,000" persons by 1640. Inflation had the effect of enlarging the number of '40s freeholders who traditionally had voted in the counties. In over thirty boroughs the franchise was also broadened as a result of disputes that involved essentially local oligarchic rivalries and gentry ambitions focused on winning a parliamentary seat rather than a commitment to enlarging the franchise. The House of Commons' decisions to widen the franchise in the 1620s, promoted by men of diverse partisan interests, also manifested self-interested political considerations. Elections were freer than has been acknowledged, and the number of disputed elections increased markedly. Moreover, the electorate was increasingly aware of national issues, and the relationship between the House of Commons and the voters was much closer than has been thought. Hirst believes that by the time of the Civil War there was "some justification" for Parliament to regard itself as the "representative of the people." Such conclusions amend understanding of aspects of local and national politics and of the nature of the Civil War.

Some of Hirst's conclusions must be regarded as tentative. As the author himself repeatedly notes, the data for such topics as voting behavior are fragmentary and distorted. The most successful sections are those that deal with the social composition and expansion of the electorate, the nature of the franchise disputes in the boroughs, and the relationship between Parliament and the electorate. Less successful are the pages on the elections in 1640. Hirst dismisses in a paragraph the question of whether women voted; he omits by design the subject of patronage; and he does not always present effectively quantitative data. Furthermore, Hirst writes in graceless prose, replete with errors in grammar and syntax. As a result, many sentences are so opaque as to be impenetrable. Despite these weaknesses, the importance of the topic, the wealth of data assembled, and the potential significance of the conclusions make this book valuable to seventeenth-century scholars.

LOIS G. SCHWOERER
George Washington University

CHARLES P. KORR. *Cromwell and the New Model Foreign Policy: England's Policy toward France, 1649-1658*. Berkeley and Los Angeles: University of California Press. 1975. Pp. x, 268. \$12.50.

Charles P. Korr examines Cromwell's foreign policy from the viewpoint of relations with France during the Protectorate. The first six chapters discuss events between 1649 and 1654 for background; chapters 7 through 12 explore episodes in those relations; chapters 13 through 15 touch on the high points of Cromwell's diplomacy (the Treaty of Westminster [1653], the mission of Sir William Lockhart, and the occupation of Dunkirk after the battle of Dunes [1658]). Chapter 16 gives a retrospective view of the whole. Korr finds Cromwell motivated by strong religious persuasions, by a Puritan hatred for Spain, and above all by a need to maintain a stable, secure regime in England. To get what he wanted in foreign relations, Cromwell put off decisions, to the point that France and Spain, worried about the Protector's army and navy, eagerly competed with each other in making concessions to his demands.

Korr's study is not a work of major importance. It has no new discoveries—none being likely anyway, after the comprehensive work of Gardiner and Firth seven decades ago—and Korr's scholarly diggings, as revealed in thirty-nine pages of notes and nine pages of bibliography, seem to reinforce interpretations made by Christopher Hill, Michael Roberts, and others. When Korr has a strong opinion of his own, as he does in chapter 7, notes 17 and 19, he asserts his judgment without giving an indication of how he got there.

Two problems undermine the value of the monograph. The first is methodological. The study is "to show how Cromwell's perceptions of events caused him to construct a foreign policy that represented the priorities he had established for his rule in England" (pp. 2-3). The narrative is not congruous with that purpose. Rather than getting inside Cromwell's process for establishing priorities, Korr gets inside John Thurloe's collection of state papers. The result is a mire of diplomatic opinions, suspicions, and unverified information, the consideration of which shoves Cromwell out of the picture much of the time. The second problem is technical. The notes reveal flaws in typography and inconsistencies in form. More important, over half of the citations of documents in volume 2 of *A Collection of State Papers of John Thurloe* have deficiencies ranging from minor misquotations to the major problem of no correspondence between citation and document.

The study does not satisfy the expectation explicit in its title. A modern study of Cromwell's foreign policy remains to be written.

W. KENT HACKMANN
University of Idaho

ROYCE MACGILLIVRAY. *Restoration Historians and the English Civil War*. (International Archives of the History of Ideas, 74.) The Hague: Martinus Nijhoff. 1974. Pp. xi, 264. 70 gls.

Macgillivray is not concerned with memoirs, diaries, or autobiographies. He is concerned with seventeenth-century historians of the English Civil War and what they had to say about its origins and aims. These historians did not intend to be dispassionate; in their view, history was a polemic art. They meant to explain as well as recount what had happened. Their work was not easy. Sources were limited and partial—newsheets, letters, personal recollections, and, in many cases, each other. Official censorship, personal interests, and the outcome of events marked their attitudes. Their histories are often incomplete and uneven. Taken collectively, however, they had much to say.

Macgillivray is clearly more concerned with questions of causes and aims than were historians of the seventeenth century. His questions are pointed, their answers scattered and diffuse. Yet Macgillivray does not falsify. If the historians tended to see the hand of God in events, they did not fail to also see the shortcomings of kings, the design of conspiracy, and the importance of social and economic aspiration. Macgillivray finds some of them aware on various points. What part, for example, had Archbishop Laud's ambition played in the developing conflict, what part the in-

decisiveness of Charles I? Was the revolution a plot? Who plotted and to what end? Was religion a major issue, or did it merely mask others? Did the acquisition of monastic lands set the gentry against the king, or ally them with him? Was England too rich or too poor?

The author deals with the work of historians before the Restoration (special attention is given to Peter Heylin and Thomas Fuller), then with historians writing between 1660 and 1702, first those of royalist persuasion (Thomas Hobbes and John Hackett), then parliamentarians and Whigs (Edmund Ludlow, Lucy Hutchinson, and Gilbert Burnet). Separate chapters deal with Rushworth, Nalson, Whitelocke, Baxter, and Clarendon. Each writer is considered individually—the various printed and manuscript versions of his history, the sources and experience upon which he drew, his originality (or lack of it), and his particular interests and emphases. The historians' work is aptly characterized and the special questions they raised are explored. Why, for example, did Hobbes choose to call his history *Behemoth*? What use did Whitelocke make of the manuscripts available to him? Why did Clarendon so prize the virtues of friendship?

Useful footnotes and an excellent bibliography enhance the study.

CAROLYN A. EDIE
University of Illinois,
Chicago Circle

COLONEL BENJAMIN CHURCH. *Diary of King Philip's War, 1675-76*. Edited with an introduction by ALAN and MARY SIMPSON. (A Tercentenary Edition.) Chester, Conn.: The Pequot Press, for the Little Compton Historical Society. 1975. Pp. xxi, 226. \$12.00.

A new edition of Benjamin Church's account of his campaigns during King Philip's War is most welcome. This present volume, issued to commemorate the tercentenary of the conflict, comprises only the first part of the Church memoirs, dealing with the years 1675-76, and eliminates the second section concerning the "Eastern Expeditions" in Maine during King William's War and Queen Anne's War. It is a beautiful production, with modernized spelling, punctuation, and paragraphing, fifty-seven illustrations, notes, bibliography, index, and a long and perceptive introduction by Alan and Mary Simpson.

The book is valuable not only for the accuracy of its history but for its ethnological detail and its presentation of racial attitudes. Church liked and got along well with Indians. He was always fair in his dealings with them and inspired such admiration as a military leader that many of his Indian

captives were soon eager to switch their allegiance and fight under such a brave and resourceful war captain against their former ally, Philip. Despite this friendship, Church never understood Indian grievances or the complex causes of the war. He condemned simplistically Philip's warriors as "bloody wretches" who "thirsted after the blood of their English neighbors, who had never injured them, but had always abounded in their kindness to them" (p. 72).

The Simpsons have recognized the root cause of the conflict in the "collision of cultures which set the pattern for almost all future relations between white men and red men in English America" (p. 1). At their best, the English were paternalistic and condescending to the Indians; at their worst, they were arrogant imperialists, insisting that Indian nations be subject to them, conform to their ways, and abide by their laws. The war begun by Philip was a last desperate attempt at maintaining Indian sovereignty.

Indicative of extreme carelessness in proof-reading is the fact that not one of the cross-reference footnotes is accurate. The temporary page references in the manuscript and galleys were not altered once the page proofs were completed, and we are treated accordingly to twelve footnotes that refer to the wrong page numbers or to no pages at all.

BARBARA GRAYMONT
Nyack College

NEIL MCKENDRICK, editor. *Historical Perspectives: Studies in English Thought and Society in Honour of J. H. Plumb*. New York: International Publications Service. 1974. Pp. x, 319. \$16.00.

The names of two historians dominate the study of eighteenth-century England. Sir Lewis Namier profoundly changed ways of looking at the century and its politics and has affected the study of politics in many other times and places as well, always stimulatingly, not always rightly. His own researches were intensive but narrow; his assumptions about the nature of politics and of history were similarly narrow and sometimes malign. He has had disciples, and his methods and interpretations still bear fruit; but—to repeat a commonplace in the profession—much of that production has come since his death, perhaps because he was so devastating a critic.

The fact that Namier was and remained (or was kept) an outsider has often been remarked. J. H. Plumb, too, is an outsider: on that the personal tribute which prefaces this volume is quite candid. Yet how different has been the pattern of Plumb's writing and influence. The range of his work is

enormous, indeed prodigious—and nearly all of it, as the valuable select bibliography makes plain, has appeared since 1950. And if Plumb has not always spoken with equal authority on all the times and subjects that have attracted him, he has never failed in enthusiasm or to be stimulating, even provocative. Again, the editor's tribute shows how these qualities informed his teaching, as lecturer, tutor, and guide to research students on both sides of the Atlantic. For all the wide influence that can rightly be ascribed to it, Namier and his method have given rise to far less than we might have expected in studies of his own century. Plumb, too, has generated fewer eighteenth-century studies among his students and friends than we might like to have. Fear of the master is not at fault, however; rather the very breadth of Plumb's interests and the kind of encouragement he has given have led to historical exploration in all directions.

The ten essays in *Historical Perspectives*, all by former pupils of Plumb, make up one of the best *Festschriften* that has appeared in English history. McKendrick's prefatory essay is astonishing: a speech given at the dinner honoring Plumb on his retirement from his personal chair, funny and serious by turns, it is a far more frank and accurate assessment, both of Plumb and of his place in Cambridge, than is usual in such compositions. The final essay is Eric Stokes' inaugural lecture as Smuts Professor of Commonwealth history, but if it was not originally intended for this volume, it is in keeping in its arresting insights into its subject—the complex, symptomatic figure of Rudyard Kipling. The other eight essays, deliberately restricted to Cambridge associates and to English subjects, range from the sixteenth through the nineteenth centuries. If one seeks an underlying theme or approach among them, it lies—apart from E. V. Bennett's fascinating account of the precise way in which the fear of Jacobitism and Jacobite activity served the rise of Walpole—in the relation between thought (if one extends that to include assumptions and prejudices) and action, which may say more about a preoccupation of our time than of Plumb's direct inspiration.

In the classical mold of history of ideas are the elegant essays by J. P. Kenyon and Quentin Skinner, the first on the irrelevance of the steadily reiterated Whig theme of contract and resistance to royal oppression in the defense of the Glorious Revolution; the second on the "patriotic" posture adopted by Bolingbroke—a study in the deliberate, cynical adoption of accepted rhetoric that neatly cuts through the confrontation between Namier's (and others') dismissal of Bolingbroke and his admirers' defense of his intellectual commitment. A. Rupert Hall's judicious assessment of

the relationship (or, more properly, lack of it) between science and technology is a major contribution to a continuing debate and, like Skinner's article, directs attention to the *logic* of the argument. J. J. Scarisbrick analyzes the contents of and reaction to the Jesuit Robert Persons' detailed plans for the reconquest of Protestant England. Written in 1596 but not published for nearly a century, Persons' plans serve Protestant polemic "as one of the most influential unread books ever written." Taking up themes Plumb himself has dealt with, as do Bennett and Skinner, McKendrick examines the part played by the labor of women and children during the Industrial Revolution, not as a newly realized horror but as a major contribution to the growth of demand through increased family income. Barry Supple traces the patterns by which working-class thrift was encouraged through a variety of institutions and relevant government policy in the nineteenth century. And, finally, J. W. Burrow effectively demonstrates the differing uses of the concept of the village community by Maine, Freeman, Stubbs, and Maitland according to their responses to contemporary political challenges.

Each of the essays merits far more than a few compressed lines of description. They are all substantial contributions and, as rarely happens with the genre of the *Festschrift*, the whole is greater than the sum of the parts—a remarkable tribute to a remarkable historian.

R. K. WEBB
University of Maryland,
Baltimore County

PAUL LANGFORD. *The Excise Crisis: Society and Politics in the Age of Walpole*. New York: Oxford University Press. 1975. Pp. 187. \$17.00.

Paul Langford has written a satisfying and much-needed study of one of the major political crises of the eighteenth century. The narrative is familiar, although there is some new material, but it is good to have in one volume the story of the excise bill and the election that followed it. Langford explains well each stage of the crisis: Walpole's decision to present the bill, the sources of resistance to the bill, the role of the court and the peerage in Walpole's defeat, the emergence of the bill as a major issue in the election of 1734, and the effect of the crisis on Walpole's government.

If there is a weakness in the book, it is that Langford does not bring out clearly enough the solidarity, persistence, and resourcefulness of the opposition in the parliamentary contest and the election. The strength of the book lies in its coverage of the role of public opinion. Initial opposi-

tion to the bill came from mercantile interests, but in the election of 1734 the striking feature was the intense hostility toward the bill in rural areas, where the excise was seen as an unwarranted extension of central authority into local and personal concerns. Langford's book is a useful contribution to our understanding of those court-country conflicts that erupted sporadically throughout the century.

Langford writes briskly, generalizes effectively, supports his statements with well-chosen illustrative material, and deals convincingly with a major topic in 179 pages of text and appendixes. Let us hope this volume heralds the end of the 300-page monograph, often nothing more than an inflated article crammed with every note the author ever took. If Langford's commendable economy is the result of the current crisis in publishing, we are the better off for it.

E. A. REITAN
Illinois State University

JOHN BARKER. *Strange Contrarities: Pascal in England during the Age of Reason*. Montreal: McGill-Queen's University Press. 1976. Pp. xiv, 336. \$15.00.

This monograph describes Pascal's reputation in England from 1657, the date of the first English translation of the *Lettres Provinciales*, to 1844, the date of the first full French text of the *Pensées*. John Barker has examined a prodigious number of library catalogs, manuscript sources, and printed works, thereby uncovering all the important references to Pascal to be found in England.

Pascal was first read in England as an enemy of the Jesuits. Champions of Protestantism regarded him as an ally in the fight against popery and either forgot that he was a pious Catholic or assumed that he was a Protestant at heart. In the reign of Charles II, during the formative period of the Royal Society, he came to be greatly admired for his scientific and mathematical genius by Hooke, Boyle, Newton, and others. Finally, once the *Pensées* became known in England, Pascal enjoyed an enormous vogue as a religious philosopher. Here historians of ideas will note with interest that Englishmen of the period read an edition of the *Pensées* that contained less than half of what Pascal wrote and that omitted many of the most important passages.

Rationalists were both fascinated and repelled by Pascal's reflections on science and religion, reason and faith. Pietists and Evangelicals admired his emphasis on free grace and human depravity. The Romantics were interested in his religion of the heart.

Barker's work is tough to read: it is as if file after file of note cards had been carefully arranged un-

der topics and methodically reproduced. The book will be most useful as a work of reference. Unfortunately, the bibliography is divided into ten parts, and the reader never knows in which part to look. The reader has to leaf back and forth to find which notes refer to which chapters. A reputable publisher ought to know better!

RICHARD SCHLATTER
Rutgers University

JOHN B. OWEN. *The Eighteenth Century, 1714-1815*. (A History of England, volume 6.) Totowa, N.J.: Rowman and Littlefield. 1975. Pp. xv, 365. \$12.50.

John B. Owen offers an unabashedly if attenuated "Namierite" political history of eighteenth-century England. There are appropriate references to Scotland, more extended discussions of political crises in Ireland and America, and occasional ventures into India. Four chapters on economy, society, culture, and the arts are sound, if highly compressed, summaries of the literature. These are slightly dated, sadly tangential, and almost irrelevant to the principal thrust of Owen's volume. The political narrative, however, is rich, firmly textured stuff. His acknowledged mastery of the world of Henry Pelham and the first duke of Newcastle gives Owen a refreshing perspective on the years between the fall of Walpole and the ultimate dissolution of the Old Guard Whigs. Chapter 5 is a splendid introduction to the subtle working of the eighteenth-century world of high politics, drawing on all that is best in recent scholarship. Chapter 12 is an intelligent, balanced account of how that pattern changed by the 1780s while laying proper stress on the elements of continuity. Although firmly in the Namier-Pares corner, Owen points to the merits of the more traditional "Whig" approach and presents, among other things, a particularly acute analysis of the Rockinghams and their evolution from Tong to Quasi-party. While ideas, even political ideas, are not Owen's long suit or principal interest, Edmund Burke certainly receives his due.

Owen's political narrative moves smoothly through the labyrinth of eighteenth-century dynastic foreign affairs and the rabbit warrens of oligarchal domestic factional squabbles. The detail and the subject matter will make this useful, but dense, material for the student, while Americans seeking a fuller background in English politics will find it a sound selection.

I have some serious reservations, most of which Owen addresses himself to in his introduction. For him history is essentially narrative. Too much analysis confuses rather than enlightens. Ideas had relatively "little influence on political or economic life" in this "oligarchic and pragmatic age" (p.

xii). Even in these terms, however, surprisingly little space is devoted to exactly how the political system worked and in what ways it involved people other than the charmed parliamentary circle. Owen barely touches on these points. There is a sprightly discussion of the borough system, some paragraphs on local government, but very little else except repeated general statements that the oligarchy continued to make itself the effective voice of a nation that, in at least half of the period covered, was undergoing dramatic change. "An oligarchy which could ensure hundreds of years of internal peace, establish an undisputed hegemony in Europe, acquire an Empire, preside over quiet revolutions in agriculture, industry and religion, and agree to share its power when the national interest so required, is deserving of respect, even of admiration" (p. 339). Every statement has a measure of truth, but it is unclear from this book exactly what sustained internal peace, whether it was the oligarchy or some select (usually eccentric) few among them who had the interest and capability to advance British hegemony in Europe and the world. It is even less clear what they did to further the agricultural, industrial, commercial, or demographic revolutions. What was the relationship, if any, between the oligarchy and the developments in religion, the British Enlightenment, and the evolution of romantic culture? Was the bulk of the oligarchy as oblivious to commercial development, trade, banking, and industry as Owen's presentation implies?

Culture seems painfully compressed. While Owen's themes are correct, the proportions are odd. Surely J. W. M. Turner, England's greatest modern painter, deserves more than one line. History deserves better from a historian. The Scottish Enlightenment receives short shrift. Even political thought, Viscount Bolingbroke's, for example, could have been enriched by a wider reading in the literature. It is perhaps appropriate that dilettante culture appears more significant than high intellectual and artistic achievement. That was, after all, the world of the oligarchy.

EUGENE C. BLACK
Brandeis University

JOHN FOSTER. *Class Struggle and the Industrial Revolution: Early Industrial Capitalism in Three English Towns*. With a foreword by E. J. HOBSBAWM. New York: St. Martin's Press. 1975. Pp. 346. \$16.95.

In the first ten lines of his introduction Foster states briefly his purpose. His subject is the labor movement in Northampton, South Shields, and Oldham from 1790 to 1860. His "central theme" is the formation and decline of a revolutionary class consciousness in this period. His "basic aim" is to

improve our understanding of the development of industrial capitalism. The "main problem" to which he addresses himself is the change in the nature of English capitalism in the later years of his period. The adjectives central, basic, and main suggest some jostling among emphases, but the chronological pattern of the book mitigates it and assists the reader's understanding.

Generalizing primarily from the experience of Oldham (to which Foster devotes the most attention) in the 1790-1860 period, he finds three major changes in the social structure. Out of economic crises in the first thirty years a labor consciousness developed, while simultaneously the preindustrial authority systems collapsed. Next, a class consciousness formed locally, within industry, not as the work of an organized revolutionary party but by a "careful, conscious process" guided by "vanguard" radical leaders of an anti-capitalist persuasion. Then in the late 1840s a social restructuring began that Foster calls liberalization. Locally and nationally an establishment solution emerged as the will to oppose change weakened. A new authority system took form in which a new labor aristocracy participated. When made in these surroundings, concessions such as household suffrage appeared almost conspiratorial.

Foster's evidence, literary and quantitative and primarily local in origin, apparently shaped his description of the pattern of local history. Yet quotations from Marxist-Leninist scriptures seem to suggest influences that inform the use of evidence and facilitate leaps from evidence to conclusions that other persons might not be able to make so gracefully. Foster's discussions also suggest some personal involvement. He uses the word feudal in a pejorative sense (pp. 14-15). In asking whether certain developments helped produce an anticapitalist "mass perspective," Foster says that "the answer must be much less hopeful" (p. 42). In a certain respect, South Shields and Northampton "point in the right direction" (p. 107). These are loaded words—"hopeful" and "right" from a certain perspective. Influencing the inquiry by suggesting questions to be asked of the evidence and manners of addressing the questions, this perspective relates to other recent searches into the history of the Industrial Revolution, but the substantive quality of the evidence produced by Foster's research is unusually impressive.

CARL B. CONE
University of Kentucky

RAYMOND CALLAHAN. *The East India Company and Army Reform, 1783-1798*. (Harvard Historical Monographs, 67.) Cambridge, Mass.: Harvard University Press. 1972. Pp. xiv, 242. \$11.00.

When the British lost much of North America in 1783, the possibility of losses elsewhere became a natural concern. Although the French threat to the British presence in India was checked, it had been a close call, and London assumed that when another European war broke out, there would be a renewed struggle in the distant subcontinent. Both the directors of the East India Company, whose enormous treasure was at stake, and the leaders of government, whose newly formed Board of Control now supervised policy for India, understood that everything depended on the company's Indian Army. That army's special practices and peculiarities, lucidly summarized by Raymond Callahan, were a source of anxious concern in London. Reform efforts begun in 1783-84 were still fruitless by 1793 when war with France made the matter even more urgent.

Despite fifteen years of determined effort by many powerful people, including George III and William Pitt, the reform effort failed. Callahan's account is both a good story and a good analysis. The reader will share the author's wonder that Cornwallis escaped serious censure, not only for the debacle at Yorktown, but again for disasters in India apparently caused by Cornwallis' mistakes. His successor in India, Sir John Shore, has commonly borne the brunt of criticism for the failures there during the 1790s, and Callahan succeeds in a moderate resurrection of Shore's reputation. The book does a superb job of explaining the astonishing ability of the Indian Army officers to preserve their perquisites, and perhaps even to improve them despite powerful efforts in the highest places to instigate change.

This illuminating account of one special subject is a small part of the work to be done on the story of the last hundred years of the East India Company's adventures. The authoritative writing that is pertinent needs a good deal of revision. Of some sixty entries in Callahan's bibliography of secondary works, over half were published before 1945 and about a third before 1900. More special studies are needed, and so is a larger account of the development of British India before 1858—a monumental achievement based almost completely on archive.

RICHARD D. MILES
Wayne State University

JOHN WOODWARD. *To Do the Sick No Harm: A Study of the British Voluntary Hospital System to 1875*. (International Library of Social Policy.) Boston: Routledge and Kegan Paul. 1974. Pp. xii, 221. \$17.25.

This small book (146 pages of text) brings together some useful data on the operation of voluntary hospitals in Great Britain during the eighteenth

and nineteenth centuries. The author introduces his work as a study "concerned principally with the contribution of the general voluntary hospitals in Britain to the health of the population."

Woodward then examines the converse, exploring whether or not the voluntary hospitals were "Gateways to Death" as charged by William Farr and Florence Nightingale. Within the limited and frequently controversial statistics of the period, using hospital death rates and the length of bed occupancy, he concludes that the hospitals achieved "what appears to be a remarkable degree of success in treating their patients and the mortality remained at a low level" (ten percent of admissions).

The book contains some interesting chapters on the characteristics of eighteenth- and nineteenth-century hospital nurses, on the types of surgery performed in voluntary hospitals, and on admission policies. The voluntary hospitals, which were funded by donations from the prosperous upper middle class and aristocracy, were established to serve the laboring, "deserving poor." Except for those injured in accidents or who were otherwise in acute conditions, a prospective patient of an English voluntary hospital needed a card or other recommendation from a contributor before admission to certify that he was a "proper object of charity." The chronically ill, maternity cases, and patients with apparent infectious disease were generally excluded.

It is obviously impossible to determine the relationship of any one type of medical facility to national health, given the multiplicity of biological, environmental, social, and economic factors that enter into the health of any population. But the merit of a study such as that by Woodward is that it extends the detailed, historical exploration of health care to which a number of British and American historians have contributed in recent years.

JEANNE L. BRAND
National Library of Medicine

PAUL HAYES. *The Nineteenth Century, 1814-80*. (Modern British Foreign Policy.) New York: St. Martin's Press. 1975. Pp. xi, 334. \$16.95.

This study by Oxford Lecturer Paul Hayes is part of a five-volume series that will describe British foreign policy from 1485 to the present. The format is unusual. Following a discussion of the Vienna settlement, Hayes provides brief surveys of Britain's relations with the major powers, including Austria, Prussia, Russia, and France. Then he evaluates each of the foreign secretaries of the period from Castlereagh to Salisbury. Finally, he discusses Britain's relations with the minor pow-

ers, as well as the place of India and economic considerations in shaping foreign policy.

Adopting an attitude that so gravely offended Americans in the nineteenth century, the author relegates the United States to the obscurity of the last of the sections between disunited Italy and dying Turkey.

The discussions are uneven in quality. Hayes seems most at ease when describing events in Eastern Europe and least so when he takes up the New World. The section on France is disappointing. No treatment of Anglo-French relations during this period that virtually ignores naval technology can have much validity.

The author's evaluations of the foreign secretaries follow traditional lines. The strong nationalists, Canning and Palmerston, receive his accolades. The chief virtues of Lord Castlereagh, the internationalist, are said to have been depressingly humdrum—steadiness and hard work. Lord Aberdeen, of whose rational outlook the British should be justly proud, is depicted as weak and even stupid and mean-minded.

Many of the author's interpretations come from early twentieth-century authorities such as H. W. V. Temperley and C. K. Webster and are sometimes modified by the more recent contributions of Kenneth Bourne. Hayes raises no questions of interpretation in areas where scholarly controversies still exist, nor, indeed, does he even make the reader aware of them.

Hayes states that the work is not comprehensive, and it is perhaps all too easy to criticize any study that undertakes a project much too large for its physical dimensions. *Hoc opus, hic labor est*. As an introduction to the period, it serves its purpose.

WILBUR DEVEREUX JONES
University of Georgia

BRUCE MAZLISH. *James and John Stuart Mill: Father and Son in the Nineteenth Century*. New York: Basic Books. 1975. Pp. xii, 484. \$16.95.

ALAN RYAN. *J. S. Mill*. (Routledge Author Guides.) Boston: Routledge & Kegan Paul. 1975. Pp. xii, 283. Cloth \$12.50, paper \$6.25.

James and John Stuart Mill have long been sitting ducks for analysis. For some decades there have been occasional excursions into their unconscious by psychologists, literary critics, and historians. John Mill's cool, brilliant *Autobiography*, which Alan Ryan insists is a theoretical work and not "to be relied on as evidence about Mill's education and the effect of that education upon him," has nevertheless always been the starting point for any psychological inquiry into the formative influences on his mind and character. The presence of a

strong-willed father and the absence of a loving mother immediately call forth the lexicon of contemporary Freudian theory, which, when aided by developmental psychology, gives us the story of a life told in terms of oedipal and "Iphigenia" complexes, displacement, symbiosis, anality, latency, surrogate and intrusive parents, aggression, over-determination, and, to bring the account up to date, bisexuality.

Psychoanalytical explanations have long since entered our daily lives and to some extent have an accepted place in contemporary culture. It is one thing, however, to assess the emotional effect of a particular upbringing on the mind and life of a thinker, to attribute his political science to a psychic need for domination, his social theory to a need for liberty, and his economics to sexual expression; it is quite another to use psychoanalytic theory to explain whole transformations of thought and values in the historical life of societies. If Mazlish had stopped at the former, we might agree that his account of the Mills and Harriet Taylor is interesting and certainly helps shape our picture of their relationships. We would at the same time have to bear in mind Ryan's more conventional but no less convincing explanations of some of the same events—for example, John's famous breakdown and crisis. Mazlish is of course after bigger game. His psychohistory seeks to explicate the whole industrializing or modernizing process itself, and he uses Freudian theory to understand how new character types were created. He is in some respects following in John Mill's ethological footsteps. It is possible, he asserts, to see constants such as our psychic nature uniting with changing social and intellectual circumstances to produce wholly new sequences of behavior and thought.

This statement seems unexceptional, and the outlines of Mazlish's general argument are certainly well known. The characteristics of industrialization or modernization are work, achievement, a new time discipline (all of which are intimately connected to Protestant individualism), the rise of the self-made man, the alienating effect of modern economic systems, the rise of the conjugal family, the child-centered family and adolescence, Victorian sexual repression, the subjection of women, and what Carlyle called a "diseased introspection"—the evangelical emphasis on sin and guilt. To these broad and familiar ideas Mazlish adds the interpretive support of psychohistory; the result is an elaboration of the ideal-type capitalist given to us by Weber, which has always been as much a psychological construct as a sociological one.

The advantages of modernization theory are not at the moment apparent. There are problems of chronology and weighting. Is the modernization

process mid-seventeenth, early eighteenth, or early nineteenth century? Are religion and education as important as industrialism? There are major disagreements about when supposedly typical or decisive character traits first become visible or dominant. The interplay between financial capitalism and industrial capitalism, and between a profit-oriented English landlord stratum and a group of entrepreneurs engaged in overseas trade, has yet to be worked out in terms of modernization assumptions. Sociological definitions like "class," especially in connection with consciousness, lack a clear historical formulation. I wish Mazlish had not so quickly converted James Mill's "middle rank" into "middle class" or been quite so positive that "educated" means middle class and "rich" means upper middle class. The problem of the appearance of the conjugal family—where we first encounter it and among which groups it first appears—is still vexed, as is adolescence. The relationship between husbands and wives in all social groupings and in all historical periods is also up in the air. New evidence may well point to a far stronger role for mothers in child rearing than the study of James Mill and his wife indicates. In order to make sense of modernization, we have to get all the right variables together at the right historical moment; if we cannot, perhaps our assumptions and objectives should be re-examined.

Problems of chronology, labeling, and evidence appear throughout Mazlish's book. There is the large question of whether words that today convey explicit or implicit Freudian meanings could have carried those same meanings in the past. Gandhi cannot be used to define Georgian words like "manly," which is too readily given a psychosexual meaning here, and "Man," as it appears in Enlightenment philosophy, should not be emphatically declared masculine. We should not be told that Harriet Taylor's father, born in the eighteenth century, is Victorian, nor should we be left with the understanding that James Mill is Victorian as well. The result of this sleight-of-hand is to give us a whole series of supposedly Victorian traits and characteristics—the patriarchal family, the "strict and joyless tenets of Puritanism," the evangelical conception of the child as sinner, and the alleged irreligiosity of urban England as opposed to preindustrial, rural England. All of these hardly do justice to either the Victorians or the Georgians. Anyone who has worked through late eighteenth- and nineteenth-century Anglican theology will hesitate to say that "orthodox Christian religious conceptions" easily reduce to the idea of an all-powerful father-God. It is time for theologians to come to the aid of secular historians and psychologists. We have labored too long in overconfident isolation.

The student of English history will find other assertions and errors to contend with. Why, for example, are the electors of Westminster in the pre-1832 period referred to as if they were £10 householders when in fact the franchise was "pot-waller," that is, virtually democratic? Why is dependency-independency made a special problem for nineteenth-century industrial or modern man, and the need for personal autonomy related to "wider ideological needs, corresponding beautifully with the Utilitarian stress on individualism," when the entire eighteenth century was filled with unbeneficed clergy, disappointed Oxbridge fellows, ambitious artists, and frustrated young men of ambiguous origins who constantly decried the system of aristocratic patronage? There is no need to insist that James Mill, the proud young man forced to toady to the great, is somehow once again the harbinger of the future. Mazlish compares him to Stendhal's Julien Sorel, who "may be considered the prototype of what, along with the governess, was one of the major social types of the time." What happens to the point that the introduction of age-grading makes achieving more conscious when the evidence cited is Jeremy Bentham, who "at age twelve was not unique in being at Oxford University in the same class as a twenty-four-year-old"? As a matter of fact, he was unique. Only four percent of Oxford matriculants in 1760-61 were fifteen years of age or younger.

There are times when Mazlish is aware that his timing may be off or that his generalizations about family development are a bit unsteady, but his eagerness to find England in a fully industrial condition in 1800 carries all before it. He is too anxious to push conventional Victorian stereotypes back to 1800. He is too eager to employ psychoanalytic concepts in support of a modernization model that has long been available but has never been altogether conceptually or factually persuasive. He is undoubtedly correct in thinking that psychohistory will have a safer future when it ceases to claim a dominant or causal role in historical explanation and when the psychic life is viewed more modestly in relation to other biological or cultural factors. While historical phenomena or social relations can be theoretically reduced to psychoanalytic mechanism, we cannot reverse the process and reason our way to particular social and institutional structures from psychological data. Psychohistory, in facing up to this, will not receive the cold shoulder to which Mazlish refers in his opening pages. If the reader wishes to have a guide through the many writings of the Mills, especially John, that takes cognizance of inherited traditions of thought and offers as well some cautious advice on how to read the *Autobiography*, he

will peruse Ryan's successful contribution to the Routledge Author Guides.

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PHILIP ROSENBERG. *The Seventh Hero: Thomas Carlyle and the Theory of Radical Activism*. Cambridge, Mass.: Harvard University Press. 1974. Pp. xi, 235. \$10.00.

Are wives and husbands (dog-owners and dogs) attracted to one another because they are alike, or do they become alike because they are attracted to one another? It is a puzzle as great as the parallel one concerning authors and what they choose to write about. In any case, Philip Rosenberg shows in his study of Carlyle an arrogance and a contempt worthy of his subject.

With Podsnappery he states that Carlyle's early essays have "little intrinsic interest," while his later writings seem "scarcely worth reading and even less worth writing about." But the "certain sadness" to which Rosenberg "must confess" over his confining Carlyle's valuable work to the decade and a half ending with *Past and Present* (1843) need not bring the reader to melancholy madness, for he is quickly reassured "that rescuing fifteen years of Carlyle's career from oblivion is no disservice to his achievement." The tone here established leads unsurprisingly to the statement that "one of the primary purposes of this volume" (the others are not delineated) is "to demonstrate that Carlyle's writings, if properly interpreted, can be of considerable value in the development of a theory of radical activism at a time when, after the diffuse and unfocused activism of the 1960s, such a theory is badly needed."

The key to the whole work is that parenthetical phrase, "if properly interpreted"; Rosenberg assumes a rare hermeneutic authority over the history of ideas as well as over Carlyle's utterances, and with considerable sleight of hand he pulls from Carlyle's hat rabbits of uncertain ancestry and questionable vitality. The "might is right" debate, for example, arises because "liberals" (contractualists all) fail to recognize that the assertion of identity of might and right is a mere truism; those who have power have power, and all moral considerations are as dust beneath the crushing wheels of pragmatism. Similarly, Rosenberg, by equating the "rationalist-idealist side of the political spectrum" with "liberalism," can leave "realism" (concerning historical models) as the equivalent of radical Carlylianism—or almost anything else.

I must confess to a certain sadness that I discovered so little of the value discerned in the work by

others (most notably, Barzun and Trilling on the dust jacket). There is nothing new here about Carlyle, except the revisionism that insists on his special relevance to the 1970s; there is much that is mistaken about the nineteenth century; and there is considerable evidence of cleverness and commitment. As Max Beerbohm is reported to have said, "This is the sort of thing you'll like, if you like that sort of thing."

JOHN M. ROBSON
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A. J. MEADOWS. *Science and Controversy: A Biography of Sir Norman Lockyer*. Cambridge, Mass.: MIT Press. 1972. Pp. ix, 331. \$16.95.

From scientific controversy comes either new theories or affirmation of established ideas. The loser, in this case Sir Norman Lockyer, carries nothing away unless it is the satisfaction of having attention focused on his research, even though his results are rejected. A. J. Meadows believes that the "losing" scientist provides a useful perspective on his more successful contemporaries; he is a mirror of the beliefs of his time.

Sir Norman Lockyer was a self-educated scientist in an age when that was both possible and admirable. A pioneer in solar physics who developed important techniques for studying the sun during eclipses and at other times, he discovered helium through spectroscopy. He was also the founding editor of the British journal *Nature*. These activities spanned the period from 1860 to 1920.

The story of failures gives a seldom-used insight into scientific enterprise. In a scientific controversy, such as the one over Lockyer's dissociation hypothesis, the standards for verification are revealed and the growth of scientific ideas better understood. But in describing this as well as other controversies, Meadows devotes more space to the personal jousts than to the issues involved. Lockyer appears pitiful and frustrated, an intractable rather than an original thinker who was wronged by the conservatism of his time. His opponents are vindictive in denying him the gold medal of the Royal Astronomical Society and keeping his son out of the Royal Society. The seamy side of science is exposed, and Lockyer is seen as less heroic than picaresque.

Can this biography be the true picture of Sir Norman Lockyer, an important scientist during an era when British science was supreme? It will take another biography to unveil the Lockyer who was a positive force in Victorian science and society.

HAROLD ISSADORE SHARLIN
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DAVID MORGAN. *Suffragists and Liberals: The Politics of Woman Suffrage in England*. Totowa, N.J.: Rowman and Littlefield. 1975. Pp. vii, 184. \$12.50.

ANDREW ROSEN. *Rise Up, Women! The Militant Campaign of the Women's Social and Political Union, 1903-1914*. Boston: Routledge and Kegan Paul. 1974. Pp. xix, 312. \$21.75.

A prime requisite in a modern historical work is a rational explanation of an event in the past, reason being the historian's basis of both comprehension and communication. A controversy such as that over women's suffrage, in which attitudes were determined as much by emotional assumptions as by rational considerations, if not more so, creates a problem that taxes the ingenuity of the writer to make sense of the irrational for himself and his readers. All those involved, male and female, for and against, had motivations not always rational and not always acknowledged though frequently rationalized. To fail to recognize these complexities when discussing the volatile issue of votes for women is to distort the stuff of politics and to give the appearance of stupidity or double-dealing to those pulled in one direction by reason and another by prejudice.

Andrew Rosen in *Rise Up, Women!* faces the problem by adhering closely to a detailed chronological account of the activities of the suffragettes that fulfills his claim to be "faithful not only to the facts as [he] found them, but faithful in affect as well . . . evocative as well as accurate." By evoking the atmosphere, he makes irrational actions and reactions comprehensible. David Morgan seems unaware of the problem in *Suffragists and Liberals*, a study of liberal party politics and the women's suffrage question between 1906 and 1918, and he gives almost no weight to the irrational motives that affected the decisions of both male and female protagonists. He accepts the words, especially of the politicians, at their face value and seeks to present explanations based solely on rational political considerations for subsequent dilatory behavior.

Morgan's book has other irritating flaws that probably stem from this limitation and certainly reflect it. His idiosyncratic decision to use the term suffragist for supporters of both the Women's Social and Political Union and the National Union of Women's Suffrage Societies is at the very least confusing and, as in the title of the book, sometimes misleading. To speak throughout of "the campaign for what was called Woman Suffrage" by that name, which was felt to have pejorative connotations, reveals an insensitivity to, or ignorance of, the feelings of the participants who accepted John Stuart Mill's insistence on the term "women's suffrage" rather than "woman suf-

frage," the latter term then being most usually adopted by opponents of the women's movement. Other remarks such as, "Besides the obvious George Eliot there were polemicists such as Mrs. William Ellis who was not really a feminist writer," further indicate Morgan's lack of understanding of the movement and its aims, and of the values that men and women believed were at stake in the controversy symbolized by the demand for the vote.

Rosen, on the other hand, is always well aware of the charged atmosphere that surrounded the women's struggle. His exhaustive and meticulous search through the primary sources has not dulled but heightened his awareness of the angers, frustrations, and prejudices on all sides that created the atmosphere in which the irrational flourished. His interest in the movement was broadened by his realization that the "exasperation of the suffragettes at the apparently complete inefficacy of conventional methods of agitation" drove them to the extreme phase of militancy that "came to resemble, in certain aspects, millennial movements of other eras."

Both authors wisely refrain from attempting a dogmatic answer to the vexed question of whether the extremism of the final years before the First World War retarded the achievement of the vote for women, although Morgan accepts militancy as one of the reasons for procrastination. Reading *Rise Up, Women!* and *Suffragists and Liberals* confirmed my own feeling that to retard the consummation of a fifty-year-old campaign was at worst a semantic achievement and that the suffragettes may well have provided that advanced position for which Jessie Boucherette praised John Stuart Mill's *The Subjection of Women* in 1869: "There is a great body of people who endeavour to keep in the middle avoiding the two extremes of opinion as they think and I observe that the further the most advanced opinions go, the further they go. . . . They don't feel safe unless there is a body of opinion before them. They then decide in a judicial manner and looking immensely wise that there is something to be said on both sides but that the extreme of opinions on both sides are wrong."

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JOHN E. KENDLE. *The Round Table Movement and Imperial Union*. Buffalo, N.Y.: University of Toronto Press. 1975. Pp. xvi, 332. \$17.50.

The Round Table was the most important and most intellectually significant of the groups working for federation among the states of the British Empire in the early decades of this century. It had its inception among the group of young men who

formed Alfred Milner's "kindergarten" in South Africa after the Boer War. Inspired by their success in forwarding political unity there, they sought further triumphs in working for the creation of a form of organic unity among the nations of the Empire.

They attempted to accomplish this by creating discussion groups recruited from the ranks of sympathetic intellectuals and opinion makers in Britain and the major dominions. The members of the Round Table, however, were a singularly ethnocentric group. As John Kendle indicates, they were drawn from the Anglo-Saxon Protestant middle class, though the British participants had some aristocratic underpinnings. Through the pages of their journal and other contacts, like spoke to like across the waters. There was much preaching to the righteous but little effort to call sinners, such as Bourassa in Canada or the leaders of the Australian Labor party, unto repentance. For all of their imperial concerns, they seldom deigned to submit their ideas to the marketplace forces of political debate, and, as the author suggests, had they done so they would have met with little support. In the development of imperial relations, the Round Table groups were essentially side shows, clever and attractive in some ways but side shows nevertheless.

While the general story of the Round Table is well known, Kendle has fleshed it out with skill, using widely scattered sources. He has perhaps focused his efforts somewhat narrowly. His account might have been enriched had he discussed in greater detail the differing measures of acceptance or rejection given the movement in the various states of empire; but the work as it stands is a contribution to the understanding of an important aspect of British Empire-Commonwealth development.

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A. C. H. SMITH *et al.* *Paper Voices: The Popular Press and Social Change, 1935-1965*. With an introduction by STUART HALL. Totowa, N.J.: Rowman and Littlefield. 1975. Pp. 262. \$12.50.

A product of research conducted at the Centre for Contemporary Cultural Studies at the University of Birmingham, this study is an effort to use the approach to popular culture advocated by the Centre's former director, Richard Hoggart. The type of literary and linguistic analysis ordinarily reserved for works of "high culture" is applied to the *Daily Mirror* and the *Daily Express* during the election campaigns of 1945, 1955, and 1964. Vocabulary and images are examined to discover the

differing assumptions that the two newspapers made about their readers and the contrasting pictures of society that each implicitly conveyed.

While some of the findings may appear self-evident, this systematic investigation gives hard evidence to support what hitherto had been simply impressions. During the war the *Daily Mirror* evolved a "stylized working class speech" that associated it with its audience, the ordinary Britons, against the privileged "Them." Encouraging correspondence from its readers, its staff sensed the collectivist mood that was developing, and thus recognized, as the *Daily Express* did not, that such issues as housing would be important factors in the 1945 election. Amid the affluence of 1955 the *Mirror's* "mode of address" was inappropriate, and the *Daily Express*, which had throughout pictured the nation as individualistic and hierarchical, was in closer harmony with the public mood. The study concludes that newspapers are neither simple reflections of their society nor powerful independent influences on it. Rather, those newspapers that interpret social change in terms "congruent" with their readers' experience will reinforce existing trends; those that do not will have little effect.

This work is of interest less for its conclusions, however, than as a demonstration of the limits and possibilities of its methodology. As the author recognizes, his premise that the personality of a newspaper is a product of unconscious assumptions rather than of deliberate policy can only be established in conjunction with more traditional studies based on the papers of editors. Nonetheless, by pointing out the inferences that can be drawn from a newspaper's choice of language and of images, he has added a new dimension to the study of the press.

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MARTIN GILBERT. *Winston S. Churchill. Volume 4, 1916-1922: The Stricken World*. Boston: Houghton Mifflin. 1975. Pp. xvii, 967. \$27.50.

The sheer scale of this monumental study is staggering. Less than half of its documents come from the Churchill Collection; the majority, including many of Winston's important private letters, are found in such wide-ranging sources as the Margot Asquith Papers and the Shane Leslie Papers in private possession, the H. A. L. Fisher and T. E. Lawrence Papers at the Bodleian, the Samuel and Weizmann Papers in Israel, the Loucheur Papers at the Hoover Institution, and the Colonel House Papers at Yale. In quite readable, if uninspiring prose, Martin Gilbert meticulously elaborates six of the most complex years of Churchill's life.

This volume spans the period from December 1916 to November 1922. They were the years when Churchill returned to the corridors of power after the Gallipoli disaster to serve as a member of Lloyd George's cabinet, first as minister of munitions, then as secretary for war and colonial secretary. Among the many challenges Churchill confronted were the demobilization of the armies in January 1919, the British intervention against the Bolsheviks in Russia, the civil war in Ireland, and the innumerable problems in Britain's new Middle Eastern territories.

Several engaging chapters of the forty-eight in this volume are entitled "Mr. Churchill's Private War," "Palestine 1922: Defending the Balfour Declaration," and "The Mysterious Power of Ireland." In the first chapter and in two others on the intervention in Russia, Churchill's fanatical hatred of bolshevism is colorfully portrayed. He told the House of Commons in November 1919 that the Germans sent Lenin to Russia "in the same way that you might send a phial containing a culture of typhoid or of cholera to be poured into the water supply of a great city," to which A. J. Balfour retorted to Churchill: "I admire the exaggerated way you tell the truth." Churchill defended General Denikin until the bitter end.

There was a curious conjunction of imperial, religious, and racial attitudes during the Commons debate on the Amritsar Massacre in India. In defending the conduct of General Reginald Dyer, "a gallant officer of 34 years' service" who had ordered his troops to fire on an unarmed crowd killing over three hundred Indians, Sir Edward Carson, the father of Northern Ireland, capitalized on the anti-Semitic feeling aroused by Edwin Montagu, the Jewish secretary of state for India. Montagu had set up the government commission that condemned Dyer's actions, and so Montagu was attacked by many right-wing Tories for, in Carson's words, his "un-English" conduct in the case. Churchill delivered the crucial speech, winning over the House by characterizing the massacre as "an extraordinary event, a monstrous event, an event which stands in singular and sinister isolation." He proposed the sane doctrine "that no more force should be used than is necessary to secure compliance with the law."

As colonial secretary, Churchill attempted a very sane policy toward Ireland, but was puzzled by the "mysterious power" of that "small, poor, sparsely populated island" to topple British governments and bring the country to the edge of civil war in 1914. He did not want to abandon Ulster, but he did not want Ulster to be a barrier to a treaty with the "Irish Free State." He maintained that it was "our policy" to encourage Ulster "in her own time" to unite with "Southern Ireland";

in the meantime Britain had accepted "a complete obligation for the defence of Ulster." When civil war broke out in Ireland after ratification of the treaty in January 1922, Churchill told the Commons that "one would have to search all over Europe to find instances of equal atrocity, barbarity, cold blooded, inhuman, cannibal vengeance—cannibal in all except the act of devouring the flesh of the victim." Ireland therefore even defeated Churchill. Lord Hugh Cecil, leader of the die-hards, remarked to him bitterly but accurately about the Irish treaty and agreement: "Your statue is a statue of snow, which will dissolve under the sunlight of reality."

We occasionally get a glimpse of the private Churchill, especially in 1921 with the unexpected losses of his mother and his youngest daughter, Marigold. He ironically wrote that "another twenty years will bring me to the end of my allotted span even if I have so long. The reflections of middle age are mellow. I will take what comes." Clementine spoke strongly to her husband about his character when he was in France on a mission in 1918 as minister of munitions: "I would like you to be praised as a reconstructive genius, as well as for a Mustard Gas fiend, a Tank juggernaut & a flying Terror."

It is unfortunate that Gilbert's prodigious effort in amassing sources is not matched by a skillful editorial job. This book is supplemented by a mammoth companion volume of sources in three parts or volumes, making the lack of selection and condensation especially unfortunate. For the years covered in the volume under review, one page is devoted roughly to every sixty hours of Churchill's life. One shudders at the prospects for 1940 to 1945. Gilbert makes little use of secondary scholarly works on the period, and the book often reads like the worst Victorian biographies, a collation of primary sources—many of them fatuous or repetitive. Churchill deserves a well-written and thoughtful, if controversial, synthesis, not a monumentally boring compilation.

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FRANÇOIS LEBRUN. *Les hommes et la mort en Anjou aux 17^e et 18^e siècles: Essai de démographie et de psychologie historiques.* (École Pratique des Hautes Etudes—Sorbonne. VI^e Section: Sciences économiques et sociales. Centre de Recherches Historiques. Civilisations et sociétés, number 25.) Paris: Mouton. 1971. Pp. 562. 88 fr.

Working in the *Annales* tradition, François Lebrun has added to our knowledge of a region of western France that did not undergo the same changes as

the rest of the kingdom before the Revolution. Anjou's population stagnated during the eighteenth century. Although its linen and canvas industries showed signs of a new vitality after 1750, they declined shortly thereafter. The enlightened efforts of royal administrators and local agronomists like the marquis de Turbilly to expand land cultivation had only limited success because the Angevin peasantry clung stubbornly to outmoded agricultural methods. Most important, despite the hospital reforms of the Catholic Reformation and the new empiricism of medical pioneers like the Hunaulds and Michel Chevreul, in the eighteenth century, hospital facilities remained woefully inadequate, and medicine continued to be practiced by swarms of surgeons (*internes* and *externes*), *sages-femmes*, *empiriques*, and even parish priests upon whom the new medical methods had little, if any, influence. Improved royal administration during the eighteenth century made possible more effective help in times of epidemic, but the death toll from disease remained high among artisans and peasants sunk in a chronic state of misery.

Lebrun divides his study into three sections. In the first two he charts demography and makes estimates of mortality rates. Although he does a commendable job in overcoming a tremendous handicap caused by fragmentary source material, he does not marshal his diffuse data to determine the precise nature of the relationship between crises of subsistence and periodic aberrations of climate, nor does he investigate the connection between crises of subsistence and outbreaks of epidemics.

In the third section he provides fascinating information about the differing mentalities of parish priests who considered illness a divine warning or punishment, *empiriques* and conjurers who treated illness by utilizing magical powers, and doctors who denied the supernatural character of illness, thereby incurring the suspicion of the Angevin peasants. His research substantiates the thesis of his mentor Pierre Goubert that popular religious belief merely cloaked an elaborate set of deeply ingrained pagan superstitions. But when he turns to such eighteenth-century developments as the decline of the custom of interment in churches and the decrease in the number of legacies left to religious orders, he deals much too briefly with the complex subject of the profound transformation of Angevin attitudes toward death. If death remained omnipresent in Anjou during the eighteenth century, as Lebrun's research indicates, why did it lose its centrality to the Angevin outlook? Certainly Lebrun's explanation of this apparent paradox in terms of the waning of the Catholic Reformation in Anjou is unsatisfactory after the work

done by John McManners calling attention to powerful secular forces of reform. In spite of these reservations, Lebrun's study of Anjou is important because it points the way to research yet to be done.

RONALD I. BOSS
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DALE VAN KLEY. *The Jansenists and the Expulsion of the Jesuits from France, 1757-1765*. (Yale Historical Publications, Miscellany, number 107.) New Haven: Yale University Press. 1975. Pp. x, 270. \$15.00.

In this study of the complex events leading to the expulsion of the Jesuits from France, Dale Van Kley has offered a perceptive and important contribution to our knowledge of the political and cultural history of eighteenth-century France. Never have the byzantine interactions of this affair been explicated with such surety and evenhandedness.

In part, Van Kley's success grows out of his careful attention to archival materials. His striking addition to our archival knowledge of the event—full use of the papers of Louis Adrien Le Paige, an influential Jansenist lawyer in the Parisian Parlement—has allowed him to reconstruct the politics of the expulsion with a high degree of certitude. He is careful not to go beyond his evidence, yet he is able to offer a convincing model: that despite the variety of motives which contributed to the expulsion, a small, close-knit, theologically serious party of Parisian *parlementaires* and parliamentary lawyers orchestrated events in a self-conscious conspiracy.

In large measure Van Kley succeeds because of an admirable sense of how political processes work. Eschewing one-dimensional models—for example, that the expulsion represented simply Gallican defiance—he is careful to delineate the displacements and curious alliances allowing a small activist party to capitalize on such a seemingly trivial event as the La Valette affair. Van Kley clearly delineates how Jansenists became fixated with the Jesuits, how Gallican and Jansenist ideas were mated without either destroying the other, how the Jansenist party—which after the expulsion became furiously antiphilosophie—welcomed the collaboration of an apparent secularist such as La Chalotais, and so on. He brings to his account a fine sense of the ironies of political events and, important in a story of this complexity, a highly readable style.

If there is a weakness in the book, it lies in the conclusion he draws as to the role of Jansenism in the Enlightenment—that the success of the Enlightenment in France by 1789 must be in part

attributed to its becoming "more Jansenist" (p. 237). To be sure, as Van Kley points out, the philosophes capitalized mercilessly on the Jansenist success, and no doubt they also picked up on some of the antidespotic and curiously anticlerical language of the Jansenists. But this hardly seems proof of a fundamental interaction; rather it points to the variety of the philosophes' tactics and the multiple sources of constitutionalist doctrine in the eighteenth century.

Still this minor criticism should not detract from what ought to be the central judgments on the book—that it is an admirable contribution to the study of eighteenth-century France and early modern politics, and that it ought to provide a model for the further development of the political history of the period.

HARRY C. PAYNE
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GABRIEL ARDANT. *Histoire de l'impôt*. Book 2, *XVIII^e et XIX^e siècles*. (Les grandes études historiques.) [Paris:] Fayard. 1972. Pp. 870. 60 fr.

Gabriel Ardant uses taxation as a frame in which to portray the economic development of the West and the role of government within it. Volume 2 of his *Histoire de l'impôt* is a long and detailed account of the development of taxation from the end of the seventeenth century to the European Economic Community of today, focused mostly on France, but touching Britain and the European continent at many aspects and extending at the end to the USSR and China. The story is brought from taxes in kind, in services and the *corvées*, to money payments; from tax farming to centralized collection; from tithes, *dîmes*, capitation levies, taxes on windows, hearths, doors, and land especially surveyed cadastrally for the purpose, to levies on commerce; from taxation primarily within nation-states to that between them; and from salt and bread taxes to those on wine, tobacco, coffee, tea, and cocoa. The income tax appears at the turn of the eighteenth century in Britain and with the Third Republic in France. Indirect taxation is streamlined in the twentieth century as excise and sales taxation is replaced by the elegant tax-on-value-added, which conforms to Ardant's criterion of neutrality as opposed to taxation that is distortionary.

Gunnar Myrdal once said that France is the country with the best professors of public finance and the worst national accounts. Ardant claims great importance for public finance, especially in war and recovery from war, and credits British successes in the Queen Anne's and Seven Years' War to the superior ability of the crown to extract

revenue from its subjects. The European enemies of France were unable to fight without subventions. Mistakes of taxation led to the American and French Revolutions; Pitt's income tax defeated Napoleon at Waterloo. There is something to the point, but it is easily exaggerated.

Ardant misses an opportunity to pursue the economic aspects of foreign policy when he ignores the public finance side of the *Zollverein*. A new paper by Rudolf Dumke notes that the elimination of thousands of miles of tariff boundaries among the members of the *Zollverein* drastically reduced heavy costs of customs collection, to constitute a real saving for all. In addition, the technique adopted by Prussia of dividing customs revenue by population in effect bribed the princes of the states along the southern tier to join by guaranteeing them income raised without necessity to levy unpopular taxes. Nor does he contemplate the possibility that Holland's failure to industrialize at the time of the British Industrial Revolution was the consequence of the mercantile oligarchy's insistence on taxing the cost of living of the working classes, rather than foreign trade, thereby raising wages and making industry uneconomic, as a recent thesis by Joel Mokyr at Yale contends.

Main sections of the book are devoted to the eighteenth century up to the American and French Revolutions, to revolutions themselves, and finally to the Industrial Revolution. These are introduced by extended bibliographical sections, devoted largely to the French literature in the field. The use of the book for reference is handicapped by lack of an index.

CHARLES P. KINDELBERGER
Massachusetts Institute of Technology

FELIX MARKHAM. *The Bonapartes*. New York: Tapplinger. 1975. Pp. 224. \$14.95.

Felix Markham has set himself a formidable task in dealing, in slightly more than a hundred pages of text, with all the Bonapartes from their Corsican origins to the present day. Such a book could not be expected to contain anything really new, and it will appeal to the layman rather than the historian.

Unfortunately, the attempt does not succeed as well as it might have. The volume contains many trivia. Many of the numerous inaccuracies could easily have been eliminated with a little less haste in writing and a little more care in editing. Borodino was fought in 1812, not 1811; Napoleon did not get to England in the winter of 1805 (p. 134). In addition to such slip-ups, there are several errors of statement, some because of the need for condensation. For example, the second coalition was not formed because of Nelson's victory at Abukir

Bay. The author also makes definite statements about matters historians are still disputing.

There is another kind of difficulty. Felix Markham, who has written two earlier books on Napoleon, knows the subject so well that he does not furnish the uninformed reader with necessary identifying material. He refers to "the coup" or "the telegram"; the reader might not know that Louis Napoleon carried through a coup, or that there was an Ems dispatch.

But the book has good points. It ties together the various threads of the Bonaparte fabric. The material on the fate of the family after Napoleon's fall is interesting. Throughout, the author serves up tidbits. There is a genealogical chart, which takes a little puzzling over because of its form; a bibliography of approximately two dozen secondary works, mainly popular; and an index. But above all, there are over a hundred pages of fresh and felicitous illustrations.

This work will not enhance the author's scholarly reputation; it might well enhance his income tax.

ROBERT B. HOLTMAN
Louisiana State University

JEAN-PAUL ARON *et al.* *Anthropologie du conscrit français, d'après les comptes numériques et sommaires du recrutement de l'armée (1819-1826): Présentation cartographique.* (École Pratique des Hautes Études—Sorbonne. VI^e Section: Sciences économiques et sociales. Centre de Recherches Historiques. Civilisations et sociétés, number 28.) Paris: Mouton. 1972. Pp. 262. 85 fr.

This volume summarizes and analyzes military recruitment data primarily for the years 1819-26 and mostly in the form of maps and tables, supplemented by introductory and concluding essays. It examines the conscripts, classifying them by stature, vocation or profession, literacy, and the infirmities or deformities that excused some from service. Nineteenth-century military administrators used a system of classification that often does not respond to the twentieth-century historian's questions. Thus the volume lumps various kinds of urban and industrial workers together into an "other" category, while noting individually the numbers of shoemakers, tailors, masons, and carter—men whose skills would be immediately useful to the army.

Nearly half the volume consists of departmental maps of France indicating in percentage terms the number of men possessing a particular occupation or excused from service by a specific disease or deformity. The maps use the Bertin method whereby each department contains black circles or dots, the size of which corresponds to the percent-

age of the category present. This method permits comparison within a category, but since the scale varies, one cannot usually compare one page with another. For example, the maps indicating the number of workers in iron and other metals and the number of laborers look superficially similar in terms of the amount of darkened space, but the former ranges no higher than five percent of the departmental class, while the latter ranges as high as eighty percent. Most of the maps are clear and understandable, but one concerning the ability to read must surely be incorrect or mislabeled, because its figures are inconsistent with the other maps conveying the level of literacy.

As Dumont and Le Roy Ladurie note, the study contains few surprises. Northeastern France supplies young men who tend to be taller and more literate than those of the rest of the country. Most diseases and deformities follow no consistent geographical pattern. Occupations in agriculture are most common in the underdeveloped southern and western departments. Jean-Paul Aron's essay in "anthropological history" discusses the operation of the medical councils that examined conscripts. Aron struggles vainly to find significant relationships between height and disease, and he concludes with random comments on attitudes toward size, sickness, and race.

The volume exemplifies a danger of quantitative methodology, presenting data of limited scope and significance with clarity and detail, but failing effectively to relate the information to the larger issues of nineteenth-century French history.

JAMES F. TRAER
Hamilton College

SHERMAN KENT. *The Election of 1827 in France.* (Harvard Historical Studies, volume 91.) Cambridge, Mass.: Harvard University Press. 1975. Pp. xi, 225. \$12.50.

After Waterloo, the Revolutionary and pre-Revolutionary notables throughout France engaged in a bitter civil war that raged in government offices, in private salons, in the press, and especially in the Chamber of Deputies until the 1830 Revolution finally resolved the matter by force. Sherman Kent's *The Election of 1827 in France*, a triumph of classic historical scholarship, is the story of one crucial battle in that fifteen-year civil war.

Kent brings to light important new material. He is the first to use Villèle's revealing diary, and he has found in the archives of sixty departments both reports on local opinion and ministerial circulars not preserved in Paris. He shows convincingly how the Villèle ministry, which tried to work for the interests of the pre-Revolutionary notables within

the new constitutional system, was betrayed rather than defeated in the 1827 elections. Kent's revised statistics show that the government would have won the elections, both in the number of ballots cast (appendix III C and page 170) and in the number of deputies elected (appendix III B and pages 161-70), if the far right had not joined the left in a "monstrous alliance" against Villèle. Furthermore, the local prefects found the right-wing Friends of the Freedom of the Press a more effective enemy than the much-praised *Aide-toi, le ciel t'aidera*, dominated by the liberal revolutionary-imperial "new men" (pp. 81-88, 170).

Kent's new material gives us a sharper picture of the Restoration. He shows us (pp. 7, 15-16, 35n., 41, 55, 58, 159-60, 188-89) how Charles X appears in Villèle's diary as a conscientious and concerned monarch, agonizing over each decision, determined to lead his nation. In the correspondence of the prefects, we see the ministry's concern for the rule of law taking precedence over its will to win (pp. 130-56, 85-86n., 77, 78, 115, 128) and, among other things, allowing 18,000 voters to be added to the prefects' first electoral lists, mainly through the efforts of the two oppositions (pp. 59-61, 116-20). Kent points out how the constitutional, parliamentary government of the Restoration had awakened the political pulse of a nation that had become cynical during the "absurd electoral ballets of Napoleonic times" (p. 105).

The 1827 election doubled the representation of businessmen (to fourteen percent) and tripled that of professionals (to eleven percent) in the Chamber of Deputies—perhaps this was the "bourgeois revolution" that David Pinkney has shown did not happen in 1830. But this question goes beyond the scope of Kent's book, which is a tightly focused, splendidly documented reconstruction of a single event.

EDGAR LEON NEWMAN
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ALAIN CORBIN. *Archaïsme et modernité en Limousin au XIX^e siècle, 1845-1880: La rigidité des structures économiques, sociales et mentales*. Paris: Éditions Marcel Rivière. 1975. Pp. xii, 693.

In the sixty-odd years since André Siegfried published his great study of voting patterns in western France, French thesis writers have delighted in the massive regional monograph. To the social structure and political behavior of which Siegfried constructed his analysis, scholars schooled by Simiand, Labrousse, and Braudel have added *conjoncture*: the swings and transformations of economy and population. Thus the classic model exemplified by the theses of Georges Dupeux, Paul Bois, and André Armengaud runs from a detailed geography of landholding, population, and pro-

duction, to temporal fluctuations in these and other regards over the region as a whole, to the historical geography of political behavior.

It will be hard to execute that model more faithfully, thoroughly, and competently than Alain Corbin has done for the Limousin—the departments of Corrèze, Creuse, and Haute-Vienne—during the middle decades of the nineteenth century. When it comes to the distribution of landholding, for example, Corbin analyzes the cadastres of a full sixty-eight communities covering almost a quarter of the region's total area. When it comes to popular culture, Corbin provides evidence, ordinarily built around a nucleus of statistical description, concerning literacy, school attendance, bookpeddlers, religious practice, newspapers, and theaters. There are long, rich sections on food consumption, transportation lines, agricultural practices, poverty, and voting behavior.

In comparison with work other French researchers have done on similar materials, Corbin's study contains only a rudimentary demographic analysis, little effort to apply statistical models to the data, and not much in the line of textual analysis or collective biography. But description there is, in cornucopian abundance. Unifying themes are less abundant. Migration is one; the Limousin was one of the great exporters of migrant workers to other parts of France. Corbin circles continuously around the causes, correlates, and effects of temporary migration. Among other things, he disposes of the simple notion that the great seasonal migration of construction workers to Paris and other cities provides the essential explanation of the region's rural radicalism. Instead, he offers substantial evidence that the migrant areas (and, very likely, the migrants themselves) wavered between leftism and Bonapartism, while the industrial workers of Limoges and an important segment of the region's petty bourgeoisie each developed their own reasons and their own organization for supporting radical programs by word, vote, and deed.

In this and other regards, Alain Corbin stays almost obsessively within his regional boundaries, and he rarely breaks out of his temporal limits—there are no comparisons with other regions, no comments on France in general, and nothing about the ways in which yesterday's Limousin shaped the Limousin of today. Within his boundaries, he paces off the ground with relentless, resounding steps.

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RULON NEPHI SMITHSON. *Augustin Thierry: Social and Political Consciousness in the Evolution of a Historical Method*. (Histoire des idées et critique littéraire,

volume 129.) Geneva: Librairie Droz. 1972. Pp. 316.

"This present study," writes Rulon Smithson, "does not attempt to systematically and generally alter judgments made of Thierry nor to arbitrate between him and his critics. Moreover, while it includes some of those judgments as illustrative of themes discussed, it is not in any sense a summation of all that has been written on the man and his works. It is, rather, an attempt to synthesize his works, philosophy, and method, and particularly to probe into their *raisons d'être*." Such is the stated purpose of this book, and that is precisely what Smithson achieves in this excellent study of the works of Augustin Thierry.

The author approaches his subject chronologically, beginning his study with background material on the life of Thierry and ending with a general conclusion. Thierry was, of course, very much the product of French culture; indeed, it would be hard to imagine any other European society producing the man that Thierry represents, especially the intellectual. He was a mild sort of rebel, in a gentlemanly sort of way, who, once obtaining acceptance by the powers that be, gradually conformed more and more to the accepted values and power structure (that is, Catholicism and even the monarchy). He aimed for the traditional rewards of his society, whether as a member of the Legion of Honor or the Académie des Inscriptions. He suffered perhaps the worst of all possible fates for a young scholar, when at the age of twenty-nine he found he was going blind.

After completing his studies at the École Normale of Paris, he bypassed teaching for research, beginning with the controversial Saint-Simon (1814-17), then graduating to the writing of mainly political articles for various papers, including the *Censeur européen* and the *Courrier français*, in which he expressed his views on liberty (a compulsive subject for young Thierry) and the need for major changes in government. At this stage he started work on his various books, beginning with his famous *L'histoire de la conquête de l'Angleterre par les Normands*, *Lettres sur l'histoire de France*, and *Récits des temps mérovingiens*.

Smithson is an old-fashioned scholar; that is to say, he loves his work and does it impeccably. Unlike so much scholarship today, Smithson's study of Thierry reflects calm, meticulous, and thoughtful work. Not only has he gone through everything Thierry ever wrote, as would be expected, but he has made careful comparisons of texts and different editions of each work, noting the changes in each and commenting on their significance.

From a physical viewpoint, this book has two minor drawbacks. The table of contents is found

on page 305, sandwiched between the concluding chapter and the bibliography, which is very awkward. There is no index. The book is admirably footnoted, however, and contains an excellent bibliography.

Augustin Thierry: Social and Political Consciousness in the Evolution of a Historical Method has immediately established itself as a work of major and long-lasting importance for anyone interested in Thierry and historical scholarship in the first half of nineteenth-century France.

ALAN SCHAM
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JAMES FRIGUGLIETTI. *Albert Mathiez: Historien révolutionnaire (1874-1932)*. Translated from the English by MARIE-FRANÇOISE PERNOT. Introduction by JACQUES GODECHOT. (Bibliothèque d'Histoire révolutionnaire, third series, number 14.) Paris: Société des Études Robespierriennes. 1974. Pp. viii, 261.

This carefully researched and well-written biography of the distinguished French historian Albert Mathiez can be read on three levels. As a case study in institutional history, it sketches the career of a member of the first generation of students trained in the methods of "scientific" historical scholarship that had been introduced in the French university system toward the end of the nineteenth century. Its treatment of Mathiez's political activities highlights the close connection between historical writing and politics in France that Mellon, Farmer, and others have examined in previous works. On the psychological level, it portrays an irascible misanthrope whose obsessions and resentments adversely affected his professional and political behavior.

By focusing on Mathiez's checkered career, Friguglietti exposes the institutional framework of the French historical profession, notably the informal system of rewards and penalties employed by the masters of the discipline to enforce professional standards. The book reveals how the son of a provincial innkeeper was able to gain entrée to the centers of academic prestige in Paris and reap the benefits of that privileged status. It then proceeds to demonstrate how Mathiez's subsequent refusal to observe the established conventions of the profession impeded his advancement and denied him his rightful place in the professional hierarchy.

The link between historical writing and contemporary politics constitutes a dominant theme of the work. Friguglietti skillfully traces the origins of Mathiez's scholarly preoccupation with religious developments during the French Revolution to the historian's own participation in the anticlerical campaign launched at the turn of the century. Later, Mathiez's mounting interest in the economic problems of France during the Revolution-

ary wars is attributed to his disillusionment with the French government's economic policies during the First World War. Such observations serve as instructive reminders that history is never written in a vacuum, even by scholars devoted to the ideal of scientific detachment.

Unfortunately, the psychological sources of Mathiez's compulsive behavior elude Friguglietti's probing analysis. One gets the general impression of a troubled individual laboring to resolve in his professional life traumas of his childhood, but the precise nature of these formative experiences remains a mystery. The famous vendetta against his mentor Alphonse Aulard, for example, cannot be accounted for merely by the two scholars' historiographical disagreements. The ferocity with which the disciple hounded the master right up to his death demands a more convincing explanation. Friguglietti's failure to supply one is due not to any defect in his scholarship but rather to the dearth of primary sources: Mathiez's papers were destroyed and his close associates were all dead before the study was begun. The author makes an admirable effort to piece together the disparate parts of the puzzle, exhausting all available sources and subjecting the meager data to an imaginative evaluation. He cautiously ventures the hypothesis that Mathiez's vituperative treatment of Aulard may have represented displaced hostility toward his hard-drinking, wife-beating father. But the psychohistorical speculation ends on a frustrating note of uncertainty, as indeed it must in the absence of sufficient documentation.

Notwithstanding the minor disappointment caused by the unanswered questions of psychological motivation, this biography represents an important contribution to the growing body of scholarship on the institutional and political context of the historical profession in France.

WILLIAM R. KEYLOR
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DORIS S. GOLDSTEIN. *Trial of Faith: Religion and Politics in Tocqueville's Thought*. New York: Elsevier. 1975. Pp. xi, 144. \$10.00.

The subtitle of this book aptly expresses its principal theme, which the title may obscure. Carefully researched and well worth reading, this study especially examines the place of religion in Tocqueville's writings and, correlatively, in his political actions. Only to a far lesser extent does it examine religion in his personal life, although it consistently makes clear that the ambivalence of that personal "faith life" influenced his speculative and practical perception and expression of religious matters.

Doris Goldstein notes that "perhaps the chief

lesson to be derived from close examination of Tocqueville's religious outlook is the inextricable meshing in his mind of 'faith,' 'truth,' and 'utility.' " This characteristic did not make it easy for his contemporaries to judge that outlook, and it has been no easier for later interpreters. In accord with their outlooks, Tocqueville emerges as a secular liberal or a secular conservative or an ultimately convinced Catholic. Goldstein has performed a singular service in disentangling the separate strands that went into Tocqueville's thought on religion and then in uniting them again in a way which makes clear that "in Tocqueville's social and political theory both religion and political participation are held in balance as the two irreducible elements necessary to a good society."

The somewhat repetitious and prosaic style of the book is more than compensated for by care, sobriety, and nuance. It starts with the problem of Tocqueville's personal religious belief. It proceeds to his perception of religion in America as the success of an experiment, the effort at reconciliation of Church and State in the July Monarchy, and religion in the shadow of revolution. The Church during the old regime and in the Second Empire exemplifies the roots of discontent. Religion East and West are compared, and there is a summary chapter on Christianity, politics, and history.

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Weston School of Theology

WILLIAM R. TUCKER. *The Fascist Ego: A Political Biography of Robert Brasillach*. Berkeley and Los Angeles: University of California Press. 1975. Pp. x, 331. \$21.50.

French fascist intellectuals, and the groups they represented, are becoming more and more the subjects of study and analysis. Nolte, Tannenbaum, Weber, Soucy, and others have made important contributions; more recently Frank Field has examined the careers of Bernanos and Drieu la Rochelle; and the January 1975 *Revue d'histoire de la deuxième guerre mondiale* was devoted to "Visages de fascistes français." To this tradition of scholarship is to be added William Tucker's detailed study of Robert Brasillach, a distinguished author, critic, and long-time contributor (later editor) of the fascist review *Je suis partout*.

Tucker devotes more than half his book to Brasillach's intellectual growth prior to the outbreak of war in 1939. In the thirties Brasillach developed an admiration for the "revolt of youth," especially as manifested in comradeship and in a joyful attitude toward life, elements he observed in German youth movements. The book is, however, a political biography; Tucker is trying to discover why a

French intellectual should have leaned so strongly toward fascism. What emerges is the portrait of a brilliant compulsive writer who, while filled with internal contradictions and in many instances with a blindness toward obvious facts, nevertheless retained a stubborn, indeed tragic, steadfastness to the convictions he acquired in his formative years. The nature of Brasillach's thought, anarchist and antimodernist on the one hand but imbued with nationalist loyalties on the other, impelled the writer to criticism of the Pétainist state and finally, completely disillusioned with Vichy, to a total reliance on Germany. By 1943, despairing of any viable unity or spirit among French fascists, Brasillach had to face the reality that the German mastery of Europe was not all joy and chivalry. He resigned the editorship of *Je suis partout* but could not deny his consistently pro-German stance. When Paris was liberated he voluntarily surrendered; he was tried as a collaborator, found guilty, and executed.

In portraying the attitudes of French intellectuals (for the book has broader coverage than the biographical format would suggest), Tucker reveals a facet of French collaboration that complements the economic and political studies of Milward and Paxton. In so many instances initiatives came from the French, who saw the German occupiers through their own self-serving lenses and who failed to see, or noted too late, that German cooperation continued only so long as it served Nazi interests. The fact that Tucker's book is not restricted exclusively to Brasillach but reveals a panorama of the French Right, provides the volume with value that the title does not explicitly reveal. As an admirable contribution to the growing literature on French fascism, this book is worth attention.

ARTHUR L. FUNK
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CLAUDE BELLANGER *et al.* *Histoire générale de la presse française*. Volume 4, *De 1940 à 1958*. Paris: Presses Universitaires de France. 1975. Pp. 486.

In modern times the French press has been the drudge of the political establishment and, more recently, of big business ("les trusts") as well. This servile condition was present even before the defeat of 1940. The Nazis knew that a captive press is not for the expression of public opinion but for its conditioning. Under the *Propaganda-Abteilung* and the German Embassy in Paris, the French press in the northern zone became entirely dependent upon the occupier. The Germans, however, were not satisfied with the vociferous support of collaboration by this press, whose chief magnate was Jean

Luchaire ("louché Herr" in Galtier-Bossière's phrase) and that boasted such anti-Semitic, anti-communist, and anti-Masonic rags as *Au Pilon*. A *Propaganda-Abteilung* agent named Hibbelen acquired direct control over forty to fifty percent of Parisian publications by May 31, 1944. Ambassador Otto Abetz, also extremely active, distributed a billion francs in subsidies to such newspapers outside Hibbelen's empire as Maurice Bunau-Varilla's *Le Matin*, Marcel Déat's *L'Oeuvre*, and others.

Vichy, too, was a strict master of the southern press long before the German "supervisors" arrived with total occupation. The *Secrétariat Général à l'Information*, directly subordinated to Pierre Laval and headed by such zealots as Paul Marion and the charismatic Philippe Henriot, exercised effective censorship by means of *notes d'orientation* and prohibitions (*consignes*). The subsidized Vichy press displayed the same servility as its occupied partner to the themes of anticommunism, anti-Semitism, and collaboration, to which were added three new passwords: *Révolution nationale*, anti-Gaullism, and anti-"terrorism." After November 10, 1942, an occasional smuggled Swiss newspaper and the British radio seemed the only sources of unbiased news available to the French public.

This gap was soon bridged by the clandestine press. Beginning with a pamphlet printed in August 1940, the "press of the shadows" grew after 1942 to include 1,106 publications of the Resistance organizations themselves and the former political parties. A clandestine news agency was in liaison with London, which supplied the necessary funds. Always anticollaborationist and, after November 1942, anti-Pétainist, the personnel of the Resistance press yearned for a drastically reformed post-war republic, for social justice, and for international peace. In March 1944 it created a national body to draw up plans for the seizure of the collaborationist presses and plant when liberation arrived.

The great moment in Paris came on August 21, 1944, when eleven Resistance newspapers occupied the buildings of the Vichy and pro-German press, left vacant by their owners two days before. In the "distribution of the spoils," as former Vichyites and former collaborationists would term it, the Communists got the major share, followed by the Socialists, and finally by the Christian Democrats. These confiscations were legalized by the law of May 11, 1946, which created an agency to oversee the liquidation of 482 publications. Meanwhile, a purge swept through the ranks of the *ci-devants*: some, including Luchaire, were executed; others were either given long prison sentences or denied their press identity cards.

But the morning after the euphoria of liberation was bleak indeed. Too many dailies, inflationary

costs, bad management, and crippling strikes dealt a terrible blow to the postwar press. By 1958 the number of Parisian dailies had fallen from 29 to 12, of provincial dailies from 161 to 110. Certainly there were some brilliant successes, such as Pierre Lazareff's *France-Soir*. The press had a majority in the governing board of the central news agency, France-Presse; censorship and prior authorization to print (*l'autorisation préalable*) were ended. But Hachette still retained its strangle hold over the distribution of printed material. The state continued to control the main agent of advertising, Havas-Publicité. Above all, a general statute concerning the press had not been passed by the hurried assemblies of the Fourth Republic by the time of the regime's demise in May-June 1958.

This volume brings together widely scattered and out-of-print materials. As a synthesis, it is especially successful in the first section by Henri Michel and Claude Lévy. The second and last sections are comparatively less objective, owing to the fact that the author, Claude Bellanger, was an important press personage during the period covered. A highly interesting, if intricate and prolix, chapter by Fernand Terrou concerns the evolution of French law dealing with the rights of the press. Close attention is paid to provincial publications. There are informative tables, graphs and charts, and two indexes, one of individuals, the other of press titles. The illustrations are well chosen: the last one shows a miniskirted girl reading *Le Monde* on a restaurant banquette, as a Parisian matron, holding *Le Figaro*, gives her a sharp glance of disapproval.

There are errors. Pierre Dominique is correctly identified as the director of Vichy's news agency, but his postwar editorial work in the most important pro-collaborationist weekly, *Rivarol*, is not mentioned. Joseph Rossé did sell his *Elsässer Kurier* to the Germans in 1941, but the fact that his publishing house, Alsatia, became the major source of illegal religious books for the wartime Reich is omitted.

Despite these and other inaccuracies, this volume is an invaluable resource for the historian working in the field of recent French history. It is not likely to be surpassed for some time to come.

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STANLEY G. PAYNE. *Basque Nationalism*. (The Basque Series.) Reno: University of Nevada Press. 1975. Pp. xii, 291. \$9.50.

Stanley Payne finds the genesis of a "sense of identity" within the Basque territories in the tenth century, indicates that this Basque nationalism

reached its apogee after the fall of the Spanish monarchy in 1931, and confirms that a strong sense of nationalism continues to this day. Discounting discussions of a Basque "race" as scientifically unsound, he attributes the Basque sense of ethnic identity to language, culture, and economics, and he emphasizes that these bonds are, paradoxically, also barriers to unity since there are several tribal dialects of *Euserka* that are mutually unintelligible and since the four Basque provinces have traditionally been divided socially, politically, and economically between the two western, industrialized and progressive provinces in contrast to the two eastern agricultural provinces. A primary reason for unity during the one-thousand-year history expertly documented by Payne has been the attempt to maintain the *fueros*, the local rights and privileges (especially favorable tax privileges) granted to the Basque territories as early as the eleventh century and swept away finally by Franco eight hundred years later.

In *Basque Nationalism* Payne offers not only a well-documented history of the Basque territories, but he also provides a clear view of the Catalanian independence movement, a movement that led the way for the Basque quest for autonomy. Of special interest in the book is the Basque perspective of the Spanish Civil War, a war that the author also describes as a civil war between the Basques of the four provinces.

A detailed map of Spain and perhaps of the four "separatist" regions described (Galicia, Valencia, Catalonia, and Las Vascongadas) would make the volume more accessible to the reader not totally familiar with the peninsula; there is only one map of the Basque provinces with a small inset of the Iberian Peninsula. A selection of photographs illustrates the present-day Basque nationalist movement. Charts abound throughout, describing political inertia, vote counts, book publication, percentage of wealth, blood types, and political crimes. The author's clear and straightforward prose style, his insight into the nature of the Basques and of the Basque territories, and the thoroughness of his research will excite the Hispanist as well as the student of ethnic enclaves. *Basque Nationalism* is a cornerstone of Basque historiography that will well serve the historian in this post-Franco era.

ROGER D. TINNELL
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GEOFFREY PARKER. *The Army of Flanders and the Spanish Road, 1567-1659: The Logistics of Spanish Victory and Defeat in the Low Countries' Wars*. (Cambridge Studies in Early Modern History.) New York: Cambridge University Press. 1972. Pp. xviii, 309. \$23.50.

This book goes well beyond military history by providing information that should prove valuable to political scientists, sociologists, and psychologists interested in Spain, France, Germany, and the Netherlands during the Eighty Years' War.

Geoffrey Parker uses Spanish, Dutch, French, and English documents with commendable skill, and he includes statistical illustrations and maps that are imaginatively conceived and invariably helpful. Spending just the right amount of time on technical matters, he gives a comprehensive picture of the Spanish army and of warfare during the sixteenth and seventeenth centuries. The reader gets a close look at Spain's great war machine (prone to sudden and disastrous breakdowns) and at the faces of the soldiers themselves—the noblemen, adventurers, and conscripts—marching from Italy along the Spanish road, over the Alpine passes, and into the Low Countries, where they quickly became food for powder or fell prey to unscrupulous captains and creditors.

There is a wealth of detail about the long march of over seven hundred miles. Parker furnishes several maps that reconstruct the main routes, and he gives a full account of what kept the army going: *étapes* (provisioning centers), road-clearing and bridge-building, food distribution, baggage transport, and financial arrangements with local magistrates and merchants. He discusses, too, the devastating effects of the army on friends and foe alike. "The moving army could become a vast incubator of plague," and inevitably it strained the agricultural resources of every town along the way.

No work that I have read offers a comparable account of life in the Spanish army and of the bureaucracy that loomed behind it. Parker's explanation of Spanish advances, notably in 1583–87, is thoroughly convincing. Equally perceptive is his analysis of subsequent failures—the result of political miscalculation; dwindling resources; corruption; mutinies (which "became virtually an institution of military life and form one of the earlier chapters in the history of collective bargaining in Europe"); the necessity of waging war on two fronts; and, above all, the resurgent power of France. Of special merit is Parker's analysis of Philip II's war policy, which was doomed from the moment the Dutch first realized that they could hold out and win if they had the will to do it.

Every chapter in *The Army of Flanders and the Spanish Road, 1567–1659* should be read appreciatively. Parker has given us a book that is necessary, readable, and definitive.

JOHN X. EVANS
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MARTIN BLINKHORN. *Carlism and Crisis in Spain, 1931–1939*. New York: Cambridge University Press. 1975. Pp. xii, 394.

Carlism, as the most enduring grass-roots right-wing movement in modern Europe, has long fascinated historians. From the mid-1820s to 1936 it offered an ultramontane, "throne and altar" alternative to the sporadically liberal Bourbon monarchy and to the shortlived republics of 1873 and 1931–36. Its popular support lay among the independent peasants and the small-town artisans and middle class primarily of Navarre, and secondarily of the agriculturally poorer portions of Aragon, Catalonia, and the Levant. In three civil wars between 1833 and 1876 it had been defeated. After 1876 the Communion was weakened by doctrinal struggles, and frequently it lacked a credible candidate for the throne. Nevertheless the Carlists played a significant role in the politics of the Second Republic and in the Civil War of 1936–39, which for them was "the fourth Carlist War."

In 1931 the Carlists moved from uneasy alliance to bitter hostility in their relations with the neighboring Basque nationalists. They dominated local government in Navarre and Alava under the Republic, and they were able during the Civil War to share power to a degree with the military authorities. While the left in 1934 was convinced that the entry of Gil Robles and the CEDA into the government would lead quickly to fascism, the Carlists were afraid that such participation would stabilize the hated Republic. At all times they asserted that the true purpose of the Republic was to introduce Marxist revolution and turn Spain over "to the lackeys of Moscow and of international Jewry." Although much of their membership came from the unprivileged classes, their deputies consistently voted against even the mildest land reform laws. During the war all but a few partisans of Fal Conde and the little-known, absent pretender Don Javier accepted Franco's imposed one-party framework.

Martin Blinkhorn has made thorough use of the *Archivo Histórico Nacional*, the Carlist press, government documents and bulletins, and biographies and autobiographies of numerous participants. The practical politics of Carlism and the leadership struggles are clearly expounded and interpreted. There is one minor weakness. The author repeatedly states that Carlism included a vital undercurrent of radical-populist protest. Except for quotations from ephemeral student publications, however, he does not offer even rhetorical, let alone practical, evidence for a social protest element before the 1960s, after which there have indeed been Marxist-Leninist Carlists and occasional Carlist members of the illegal Workers' Commissions.

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JOHN F. COVERDALE. *Italian Intervention in the Spanish Civil War*. Princeton: Princeton University Press. 1976. Pp. xxi, 455. \$18.50.

Based in large part on hitherto unexplored archival materials in Italy, Spain, Britain, and the United States, this book is the first thorough and systematic study of the history of Italian intervention in the Spanish Civil War. After tracing the background of events from 1922 to 1936 that led to Mussolini's decision to support Franco, the author provides a detailed examination of Italy's military contribution to the victory of the Spanish Nationalists, together with a parallel account of Italian diplomatic and political relations during the Civil War with Nationalist Spain, Germany, Britain, and France.

John Coverdale's basic contention is that strategic calculations played a much more decisive part in Mussolini's decision to intervene in Spain than the various ideological justifications that were trumpeted to the world by the propaganda agencies of the Fascist government. He shows, however, that despite the overriding importance for Mussolini of "traditional foreign policy considerations," there were "prestige factors" and a sense of common cause with the militantly anti-communist and counterrevolutionary posture of the Spanish Nationalists that also influenced the Italian dictator and his followers to a considerable degree.

Two fundamental conclusions that emerge from Coverdale's book are, first, that "Italian diplomatic and military backing was a crucial element in Franco's victory," and, second, that throughout the Civil War General Franco succeeded in maintaining his autonomy and freedom of action despite the heavy pressures exerted on him by his German and Italian supporters. Both of these conclusions are buttressed by abundant documentation and by a careful analysis of the available evidence.

Coverdale's rigorously objective and dispassionate attitude toward his material has enabled him to offer a balanced account of Italian intervention in the Spanish Civil War. But even emotion-free detachment has its pitfalls. Occasionally, in his effort to avoid the risks of bias in any direction, the author tends unwittingly to present as verified fact what is in actuality a highly dubious and partisan interpretation of a particular event or motive. One of several possible examples of this tendency is the statement on page 60 that, in view of the disorder and violence which characterized Spanish political life between February and July 1936, "it is not surprising that plans soon began to be laid for a military coup to restore order," as if the restoration of order, and not the

establishment of what Coverdale himself (adopting the terminology of Juan Linz) later calls "a bureaucratic-military authoritarian regime," was the primary motive behind the July 1936 insurgency against the Spanish Republic.

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KLAUS-JÖRG RUHL. *Spanien im Zweiten Weltkrieg: Franco, die Falange und das "Dritte Reich."* (Historische Perspektiven, number 2.) Hamburg: Hoffmann und Campe. 1975. Pp. 414. DM 58.

The foreign relations of the Franco regime have become the object of considerable study during the past twenty years, but most of this work lies hidden from view in unpublished dissertations. The content of Klaus-Jörg Ruhl's book—itsself based on a former dissertation—is indicated by the subtitle. This is not exactly a history of "Spain in the Second World War," or during the war, for the real study of Spanish society in that period of privation and suffering is still largely unexplored. Rather, it deals primarily with German-Spanish relations, with the general policy of the Spanish state during the war years, and with crucial maneuvers within the circle of Spanish Nationalist politics directed either toward provoking or avoiding Spanish entry into the war. The history of the Spanish Blue Division on the Russian front, by now the subject of a massive bibliography all its own, has prudently been for the most part excluded.

This is clearly the best and most comprehensive study that has yet appeared of Spanish-German relations and their influence on Spanish government and policy. The previous books by Donald Detwiler and Charles Burdick dealt with more limited or specialized aspects, while the recent work by Raymond Proctor centered in large measure on the activities of the Blue Division. Ruhl's contribution consists not merely in achieving a more comprehensive scope but in a more extensive and thorough exploitation of German and English materials, both published and unpublished, than has previously been made. If the result does not produce many new surprises on the level of official diplomatic history, some very interesting new data are offered on the machinations of NSDAP representatives, certain other German officials vis-à-vis dissident radical elements of the Falange, and, by 1942, General Muñoz Grandes (first commander of the Blue Division), to induce a radical change in the course of the Spanish government, a falangization of the regime, and the possible ouster of Franco himself. The nearest thing to a hero produced by this story is probably Eberhard von

Stohrer, the German ambassador from 1937 to 1942 who sought to avoid direct German interference in internal Spanish politics and thereby limited the possibility of Spanish intervention in the war.

In the process, Ruhl provides a slightly different perspective from that frequently encountered in commentaries on the Franco regime. He does not stress as heavily as others the astuteness of Franco; while recognizing the latter's undeniable capacity for dissimulation, temporization, and maneuver, Ruhl tends to place more emphasis on the influence of objective external factors and limitations that were beyond Franco's power to manipulate. Though he does not propound any general theory or interpretation of fascism, he also avoids the danger of reading the post-1945 doctrines and structure of Franquism too far back into the regime's founding decade, when certain parallels with other authoritarian regimes in Europe were more pronounced and seemed capable of further development. In general, this work stands as a significant contribution to the understanding of Spanish-German politics during the war years.

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HERVÉ HASQUIN. *Une mutation: Le "Pays de Charleroi" aux XVII^e et XVIII^e siècles. Aux origines de la Révolution industrielle en Belgique.* (Études d'histoire politique, économique et sociale.) Brussels: Éditions de l'Institut de Sociologie, Université Libre de Bruxelles. 1971. Pp. 383. 498 fr. B.

Charleroi, the great Belgian industrial city, started in 1660 as a fortress overlooking the river Sambre and was named after young king Charles II of Spain. By 1800 it had become the locus of incipient coal, steel, glass, nail, and textile industries. In the first part of his book, Hervé Hasquin relates government policies and the story of private entrepreneurs to the early growth of this industrial center. He narrates the construction of roads to Brussels and to the ancient mining areas of the surroundings and the granting of royal privileges and other measures favoring industrial development, such as the implantation of fairs and markets and the grant of liberal import and export duties.

The role and achievement of a few successful entrepreneurs is described with a great wealth of detail. This part of the book is naturally geared to legislative history, to the exhaustive compilation, study, and exploitation of charters, resolutions, protocols, ordinances, correspondence, inquiries, and notarial records. The next part of the book is an attempt to locate turning points in the growth of particular industries and of aggregate economic activity, and to relate demographic trends to the

record of economic growth. This quantitative study and the establishment of a time series of production and trade lead to the observation that the period 1763-74 was one of rapid growth in the level of economic activity and to the conclusion that economic growth led to rapid population growth by way of immigration and high birth rates.

On the one hand this book is disappointing. Although the author attempts to convey his enthusiasm about the subject by way of a very strong, sometimes bombastic, vocabulary, his work is essentially a narrative lacking a thesis and an analytical backbone. The book cannot be characterized as innovative in its method or in its use of sources. Yet the author can be commended. He is remarkably familiar with the Belgian literature for both parts of the country in both French and Dutch—a not so frequent occurrence in Belgium—and often brings in references to the economic history of France and even England during this period. Above all, Hasquin should be congratulated for having written a thoroughly researched and documented narrative work of local history to be used profitably for reference and comparison by other historians.

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B. A. SIJES. *Studies over Jodenvervolgung.* Assen: Van Gorcum. 1974. Pp. viii, 182.

This volume is a collection of studies prepared in connection with the work of the Rijksinstituut voor Oorlogsdocumentatie (Netherlands State Institute for War Documentation) with which the author has been connected since its establishment in 1945. The individual papers were prepared for a variety of purposes including postwar trials (such as the paper on Eichmann's role in the Netherlands). The collection covers a wide variety of topics within the general theme of the Holocaust.

The first paper, "The Ostmark as Testing Ground," deals with certain aspects of anti-Semitism in Austria since the nineteenth century. Sijes points out that anti-Semitism proposing extreme solutions for the "Jewish question" was rife in Austria at least since the middle of the century. In the final section of the article the author describes the methods used after the *Anschluss* to force quick emigration of Jews.

The second selection, covering emigration from Germany, deals with Nazi measures used to accelerate the rate of emigration of German Jews after the *Kristallnacht*. Of more general interest is the third chapter, "The Final Solution of the Jewish Question," which explores the timing of the deci-

sion to carry out total extermination. Sijes believes that the basic notion to exterminate the Jews was in Hitler's mind as early as the 1920s, but it took concrete shape in 1938-39. Sijes considers the killings by the SS during and after the Polish campaign as one more indication of Hitler's long-range plan. The comprehensive extermination by gas is believed to have been decided by March 1941. Sijes doubts that the German leaders ever considered the Madagascar plan a serious alternative, certainly not after the outbreak of the war.

The fourth paper deals with the activities of the SS officer, Erich Rajakowitsch, who had been one of Eichmann's deputies in Vienna since 1938. Rajakowitsch is shown to have played an active role in the deportation and to have taken extreme and harsh positions with regard to exemptions and the speed of deportations. Chapter 5 deals with Eichmann's personal and direct contribution to the extermination of Dutch Jews. Eichmann is shown to have urged a radical and accelerated effort on the part of the Germans to deport all Jews, including persons who, within existing regulations, could be exempted from "resettlement" (Jewish husbands of Aryan partners, etc.). Sijes makes the interesting point that Eichmann's low position in the organizational table of the German police and SS and his low rank are no indication of the importance of his role. In the Netherlands Eichmann dealt directly with the highest German officials such as Reichskommissar Seyss-Inquart on questions of policy, while directing lower police officials on specific questions dealing with the implementation of such policies. Sijes points out that Seyss-Inquart insisted on keeping ultimate control over the measures against the Jews, but that the police in effect managed to eliminate to a considerable extent interference from lower-echelon representatives of the civilian administration as deportations got underway. For all practical purposes, it was Eichmann who from Berlin controlled the rate at which Dutch Jews were deported after June 1942.

Chapter 6 contains some general observations about the position of the Dutch Jews during the occupation. Sijes' assessment of the role of the Secretaries-General (the highest Dutch government officials left in the country) and of the Amsterdam Jewish Council is harsher and contains fewer qualifications than do other treatments of the same issues (de Jong, Presser, Herzberg). On the question of why so many Dutch Jews perished (eighty percent—the highest percentage of any occupied country in the West), he makes a judicious and unemotional assessment, ending up with the conclusion that the Dutch Jews reacted to the German threat as most other groups in Dutch society did or would have done under similar cir-

cumstances. While recognizing anti-Semitic tendencies among the Dutch people, he pays tribute to the many non-Jewish Dutchmen who in helping the Jews go into hiding risked and often lost their own lives. The seventh chapter, an appreciation of Simon Wiesenthal and his work, is the text of a talk given by Sijes on the occasion of the twenty-fifth anniversary of the inception of Dr. Wiesenthal's internationally known work in locating war criminals.

The final chapter, the text of a lecture given in 1969, deals with the then current issue, whether three German police officials, the last Germans held in Dutch prisons, should be released before the end of their term. Sijes takes the position that these officials indeed carried major responsibilities for the destruction of Dutch Jewry, and that the purposes of justice would not be served by a premature release.

In summary, this collection of papers, while somewhat uneven, contains many new facts and insights which will be of use primarily to the specialist dealing with the Holocaust and the German occupation of the Netherlands during the Second World War.

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ELOISE ENGLE and LAURI PAANANEN. *The Winter War: The Russo-Finnish Conflict, 1939-40*. New York: Charles Scribner's Sons, 1973. Pp. xv, 176. \$7.95.

Few modern wars have received the kind of awed global attention accorded to the 105-day-long Russo-Finnish Winter War of 1939-40. It seemed for a while to be a case of David defeating Goliath. Some 450,000 Soviet troops supported by 1,000 tanks and 1,000 planes embarked on what Moscow expected to be an easy two-week excursion through Finland, only to be spectacularly routed by 215,000 Finns (including reserves) who had no tanks and only 75 combat aircraft. Regrouping for a second effort, the Russians advanced with no less than forty-six divisions, more planes, and an abundance of artillery, and by March 13 the Winter War was over. The gallant Finns counted 80,000 casualties, including 22,900 dead and 10,000 permanently disabled, while the Russians admitted a loss of 217,500 dead. Thirty years later Khrushchev raised that figure to 1,000,000.

The military history of the Winter War has appeared before in many languages, and in English most recently by Richard W. Condon in *The Winter War* (1972) and Allen F. Chew in *The White Death* (1971). Eloise Engle's and Lauri Paananen's book adds nothing to those studies; indeed, it falls far short of their quality. Only the battles of Tolva-järvi and Suomussalmi and the Finns' *motti* tactics

are reasonably well handled. The perfunctory historical introduction is a mishmash of tired clichés, uncritical tributes, and misinformation, while the book's source base is utterly insufficient for a serious effort. Typically, the bibliography omits the Chew and Condon books along with some basic Finnish works, while it includes the Finnish-language editions of some works available in English (for example, Douglas Clark's *Three Days To Catastrophe*). In sum, this book has nothing to offer serious students of the Winter War.

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ALAN MAYHEW. *Rural Settlement and Farming in Germany*. New York: Barnes and Noble. 1973. Pp. 224. \$14.00.

The literature of German rural settlement history seems a choppy sea of diffuse local studies from which arise some misty islands of inconclusive controversy. The working descriptive vocabulary is specialized but at the same time imprecise; definitions of settlement patterns and tenure arrangements are likely to disintegrate when applied to concrete situations. Ethnocultural issues that gave the field its form are outdated. This situation allows ample room for more special studies and for theoretical speculation, but anyone seeking a general understanding of German rural settlement history is confronted by a morass of esoterica, qualifications, and exceptions. The signposts we need are missing or obsolete.

Alan Mayhew's short summary cannot solve the scholarly problems of the field. He offers no new general synthesis, but he performs the very important service of providing his readers with some basic equipment for coping with materials on German rural history. His narrative begins with the earliest known settlement forms, a description that must be anecdotal and localized because close research has discredited the sweeping patterns postulated by earlier nationalist scholarship. His chapter on medieval settlement offers a taxonomy of settlement forms, whose patterns result from the planning roles of ecclesiastical and noble entrepreneurs and their professional *locatores* or developers, not the ecological or cultural circumstances of peasant settlers. Mayhew accepts Abel's "late medieval agricultural depression," perhaps without presenting enough detail or analysis to make that characterization satisfactory. He locates a clear separation between East and West Elbian patterns in the early modern period, ascribing this also to diverging forms of political authority. In a final chapter on enclosure and consolidation he traces modern developments: toward the family farm in

the West, confirmed by the *Flurbereinigung* in the Federal Republic and the *Gutsbetrieb* in the East, from Junker to collective.

If one seeks the peasantry itself through these descriptions of their settlements, the picture that emerges is one of lonely and passive families without real autonomy or mutual association; they are manipulated by the powerful, and by accident; their affective relations are with physical property and with force beyond their influence; and if they have cultural identity it leaves little mark. This impression surely results in part from the nature of the evidence, but it is enough to suggest that the peasantry may be the last place to look for the kind of intrinsic anthropological or sociological structure that scholars have traditionally sought among them most earnestly; it suggests again the nagging question of whether a category of people without an autonomous and conscious history of their own is amenable to historical description.

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DIETER SCHWAB. *Die "Selbstverwaltungsidee" des Freiherrn vom Stein und ihre geistigen Grundlagen: Zugleich ein Beitrag zur Geschichte der politischen Ethik im 18. Jahrhundert*. (Giessner Beiträge zur Rechtswissenschaft, number 3.) [Frankfurt:] Athenäum Verlag. 1971. Pp. 164. DM 36.

For a statesman who occupied high office only a very short time and who is remembered more for what he wanted to do than for what he did, the Freiherr vom Stein has received a remarkable amount of attention from historians, and not only from those of his own country. In part, at least, the sheer quantity of this writing reflects the liveliness of the debate that raged for years concerning the interpretation of Stein's ideas for Prussian reform. A decade ago, however, in a judicious survey, "Stein in German Historiography" (in vol. 5, *History and Theory*), Klaus Epstein concluded that most of the points once in dispute could now be considered as "definitely settled." In particular, Epstein noted that "there is now general agreement on the origins of Stein's ideas." Max Lehmann's thesis of Stein as the liberal disciple of the ideas of 1789 no longer had adherents, whereas post-Lehmann scholarship, whose crowning achievement was Gerhard Ritter's biography, had established that Stein's ideas, in so far as they derived from intellectual "influences" at all, linked up with Möser, Herder, Montesquieu, and Stein's friends of the Anglo-Hanoverian circle, Brandes and Rehberg.

This interpretation is now challenged by Dieter Schwab. Taking as his point of departure Stein's

"idea of self-government" (actually a complex of ideas that, it is generally agreed, pose a central problem for the understanding of Stein), Schwab concludes that the reformer's true intellectual home is found among the English and Scottish moral philosophers: Shaftesbury, Hutcheson, Lord Kames, Adam Ferguson especially, Paley, and their German disciple, Christian Garve. Not one of these names is found in the index of Ritter's biography. Schwab bases his conclusions on a study of Stein's library and a close analysis of his writings. He has produced a tidy, original, and persuasive book.

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RAINER POSTEL. *Johann Martin Lappenberg: Ein Beitrag zur Geschichte der Geschichtswissenschaft im 19. Jahrhundert.* (Historische Studien, number 423.) Lübeck: Matthiesen Verlag. 1972. Pp. 352. DM 54.

Johann Martin Lappenberg (1794–1865) is a historian about whom relatively little has been written either in Germany or elsewhere. After reading Rainer Postel's study, it is easy to see why. This is not meant to denigrate the author's work, which is a well-written, painstakingly researched study of a historian who was rather well known during his lifetime. Lappenberg's contacts with such better-known historians as Ranke, Niebuhr, and Dahlmann are described with convincing thoroughness. Nevertheless, as Postel himself points out, Lappenberg was not well known as an author of major historical works; basically, he was a major contributor to the establishment of *Quellenkunde*, that tedious but necessary aspect of the historical discipline quite often (and inaccurately) identified as an achievement of Ranke.

Lappenberg, born and raised in Hamburg, was primarily concerned with gathering, collating, and editing sources, especially those concerning the history of northern Germany. In this regard he was also interested in editing and bringing up to date major secondary works, such as Georg Friedrich Sartorius's documentary history on the origins of the Hansa. Lappenberg also tried his hand at literary history and here was primarily concerned with describing the historical contexts within which literary figures wrote and from which they drew models for plots and characters. It is perhaps ironic that the only major historical work which Lappenberg actually published was a history of England, a country for which he had a great deal of admiration. Thus, as a practicing historian, Lappenberg's contributions were minimal. In truth, he was more of an archivist.

What makes Postel's work of interest is how his subject reflected major concerns and prejudices of

his time. Politically conservative, Lappenberg was concerned that the apparently moribund Hamburg Senate, as well as the society it represented, not be swept up in putatively democratic currents, such as those revealed in 1848. At the same time, though, he obviously was very much influenced by the upsurge of cultural nationalism that was occurring throughout Germany during the post-Napoleonic and *Vormärz* periods. This came out most clearly in his history of England, where he favored the early Britons and the Anglo-Saxons over Roman and Norman conquerors. Like Father Jahn, he chose to view the English as being Germanic in character, possessed of a sort of honesty and robustness that set them apart from those from southern climes.

Furthermore, Postel succeeds in pointing out the general nature of the move toward historicism. In this work, it emerges as being a sort of fusion of Romantic views toward history and a more rigorous philological-critical method. While Lappenberg may not have written many original historical works, he was definitely a part of a historicist trend, as reflected in his own concern that those who write history do so with a scrupulous concern for capturing a past epoch as it actually was. He also expressed another belief central to the historicist method, namely, that the historian, being *zeitgebunden*, is writing history for his own time even though he might well be attempting to capture the past *wie es eigentlich gewesen ist*. What Postel establishes is that historicism was a general trend in Germany during the *Vormärz* period and further, that historicism and political conservatism went together. Also, Postel's treatment of Lappenberg suggests the need to qualify Ranke's position as a sort of "founder" of modern *Quellenkunde* and of modern historicism.

Postel's work does little to further illuminate Johann Martin Lappenberg as an important creative historian. Rather, the value of the work is that it reveals just how widespread were certain fundamental attitudes and trends in German historiography during the first half of the nineteenth century. In Lappenberg, we can see an unsynthesized amalgam of concerns; it required seminal figures such as Droysen, Sybel, and Meinecke to provide the required synthesis. This achievement would have results for German historiography that were both awesome and tragic.

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KLAUS J. BADE. *Friedrich Fabri und der Imperialismus in der Bismarckzeit: Revolution—Depression—Expansion.* (Beiträge zur Kolonial- und Überseegeschichte,

volume 13.) Freiburg: Atlantis. 1975. Pp. 579. DM 42.

Friedrich Fabri belonged to that remarkable group of secular evangelists who during the 1870s and 1880s helped convert Europe to the new gospel of colonialism. Some of them were explorers who returned from distant lands with stirring tales of the wealth and glory to be won beyond the ocean. Others were politicians seeking to gain or retain power on a platform of overseas expansion. Still others were jingo publicists who taught a doctrine of national greatness built on the conquest of dark races in far-off continents. Fabri himself was a member of the theological contingent of imperialism. A clergyman and the son of a clergyman, he started his public career as the administrator of a missionary society in the Rhineland. Disturbed by what appeared to him to be the spread of materialism among the lower classes, he began to see the specter of class conflict looming over the horizon. But might not the diversion of national energies to the task of colonial expansion alleviate this domestic discontent? His *Bedarf Deutschland der Colonien?*, like Seeley's *Expansion of England* and Leroy-Beaulieu's *De la colonisation chez les peuples modernes*, helped prepare the way for the last feverish crusade of the white man to subjugate the world.

Fabri has now found a serious scholarly biographer in Klaus J. Bade, who deals in minute detail with Fabri's life from the early days in the mission house in Barmen to the period of national prominence when he was one of the leaders of the colonial movement. The author, accepting the recent thesis of Hans-Ulrich Wehler, believes that German imperialism was the product of internal tensions which state and society sought to externalize through the acquisition of overseas possessions. The depression of the 1870s and the growth of radicalism persuaded the ruling classes that only success abroad could divert public attention from mounting discord at home. Fabri, of course, like most ideologues of expansionism, never admitted to others or to himself that he was merely trying to palliate the domestic crisis of a nation torn between authoritarian traditions and democratic aspirations. To him the establishment of colonies was motivated by the same selfless concern for the welfare of man as his own early interest in the poor and exploited textile workers of the Wupper Valley. The objective of imperialism, he argued, should be the "education of the native for labor," the improvement of his material condition, and the elevation of his spiritual outlook. Without realizing it, he provided a mantle of idealism for a policy of ruthless oppression.

Bade pursues the career of his protagonist through all its winding byways. Although from

time to time he links Fabri to the broad currents of German political and social life, his emphasis rests largely on the man and his activities. The scholarship is beyond reproach, at least in the technical sense; how can anyone quarrel with a work in which some 150 pages of notes and 40 of bibliography buttress a text of 360 pages? The research that has gone into the making of this book—unpublished sources, newspapers, journals, minutes and countless secondary works—is prodigious. But does Fabri deserve all this labor of love? Perhaps it is unchivalrous even to ask the question. Those who are very much interested in his life can now turn to a biography that will satisfy the most voracious appetite. Those, on the other hand, who care less about him than about the movement he represented will find occasional insight and illumination, even if they do not follow in all its intricacies the career of the Rhenish clergyman who became an apostle of imperialism.

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CONSTANTIN FRANTZ. *Briefe*. Edited by UDO SAUTTER and HANS ELMAR ONNAU. Wiesbaden: Franz Steiner Verlag GmbH. 1974. Pp. xii, 180. DM 32.

Constantin Frantz is one of those twilight figures of history known not for what they did, but for what they opposed. Frantz's target was Bismarck's Prussianized German state that he wished to replace with a German-led Central European federation extending from the Low Countries and Switzerland to Poland and the Balkan Peninsula. This scheme, derived from a romantic medievalism and a misreading of more recent history, was ignored in his lifetime, but it received some attention after 1945. Under the impact of the Nazi catastrophe it was felt that Frantz's proposed federation might have offered a more palatable alternative to Bismarck's centralized German state. On closer examination, however, his views were quickly dismissed again as dangerous and impractical.

The letters collected in this small volume contain little new information on these plans, but they are of some psychological and sociological interest. These are the *cris de cœur* of a lonely, frustrated individual, unable to hold any regular job and barely supporting himself and his family by an endless torrent of articles, pamphlets, and other tracts. Ever in need of encouragement and new sources of income, he kept ingratiating himself with the influential and affluent: Bismarck prior to his minister-presidency; Georg von Cotta, the publisher; and Richard Wagner, to mention the best known. He peddled his writings to the few sympathetic editors who would read them.

Gradually, plagued by ill-health, nonrecognition, and financial worries, Frantz found a refuge in anti-Semitism, anti-Catholicism, and other "anti" positions, and he joined the ranks of such like-minded apostles of gloom and doom as Paul de Lagarde, Julius Langbehn, and Ludwig Schemann. After Wagner started publishing his *Bayreuther Blätter*, Frantz had a ready outlet for his tirades.

Udo Sautter has written a useful introduction and has provided helpful annotations to the letters. One might question, however, the need for the vague and often redundant cross references.

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ROGER CHICKERING. *Imperial Germany and a World Without War: The Peace Movement and German Society, 1892-1914*. Princeton: Princeton University Press, 1976. Pp. xiv, 487. Cloth \$27.50, paper \$10.50.

Roger Chickering's study of the German peace movement is a "traditional" monograph on a hitherto neglected aspect of Wilhelminian society. It does not belong to the Wiesbaden Peace and Conflict Research genre, led by D. Senghaas, K.-J. Gantzel, and G. Kress. The author depicts the German Peace Society (founded in 1892) as a non-rural, middle- to lower-middle-class movement dominated by persons from the world of commerce, the lower educational system, professional organizations, and the lower municipal bureaucracies. The constituency of the society was more elderly than young, with almost thirty percent females and few Jews. By 1914 there were nearly one hundred regional peace groups with a membership of 10,000—of whom two to three hundred were active. Over one-half of these groups were in Württemberg; a mere nine were east of the Elbe River. A second peace group, the Union of International Cooperation, was founded in 1911, and three years later numbered three hundred, including many prominent historians and economists. George Hallgarten described it as a "brilliant gathering of officers without an army."

What few antecedents the German peace movement could claim stemmed mainly from the Enlightenment and romanticism. When peace societies were established in the 1880s, it was often with the aid of foreigners (England's Hodgson Pratt and Denmark's Frederik Bajer), as well as foreign financial backing (the Carnegie Endowment).

A "smug elitism" permeated the German peace movement. "The devoted pacifist was a militant crusader, convinced of his own righteousness and intolerant of criticism." Leaders such as Foerste, Fried, and Quidde sought a "facile transition from

Hobbesian assumptions to Kantian conclusions" through educating the masses, simultaneously limiting armaments of all nations, and creating an international political organization to regulate conflicts among the powers. They generally failed to influence the press, the educational system, political parties, the imperial government, and the churches (by 1912, only 117 of 35,000 Protestant and Catholic clergymen belonged to the Peace Society). Nor did they find converts among Masons, business leaders, or university professors. The "progressive bourgeois left," united in 1910 in the Progressive People's party, alone supported the German Peace Society. And although politically liberal and democratic, the movement's members were socially conservative, and hence they had little influence upon Social Democratic policies until 1911-13. The peace groups also failed to attract members beyond left liberals and socialists (Bismarck's *Reichsfeinde*) because they championed a number of unpopular causes, such as sex education, cremation, temperance, natural healing, and vegetarianism, and opposed capital punishment, vivisection, dueling, and vaccination.

The leaders of the peace movement ultimately failed to recognize the connection between arms limitation and international arbitration and liberalism and democracy. They could not see in their idealism that their criticism "touched the very core of the imperial political system." "The corroding influence of peace," as Treitschke called it, did not appeal to a Germany that cultivated the notion that international quarrels would most likely be resolved by violent conflict. Here Chickering emerges as a disciple of the "social imperialism" or "Kehrite" school of historiography led by, among others, Wehler and Berghahn. "The political culture of Imperial Germany was hostile to this vision of a world without war, in part because the perception of international conflict was vital to the preservation of the domestic *status quo*." In short, overseas conquests were required by Berlin in order to offset domestic political and social stress.

One wishes that the author had ended his story at this point. The chapter dealing with the French peace movement might well have been published as a separate article and the concluding section on the "antipacifists" integrated into an earlier chapter. In addition, some of the literary translations are awkward, and it is time that Chickering ended his love affair with certain pet phrases. One might also point out that there existed no monolithic German army prior to 1914; it would have been better to deal with the various particularist forces independently. Some indication of the financial backing received from the Carnegie Endowment would have enhanced this study. And might it not have been beneficial to investigate the one

occasion on which Imperial Germany submitted an international dispute (the Venezuelan blockade of 1902-03) to the Hague Court, as the pacifists so ardently desired?

To be sure, these are minor points. The work represents a major contribution to Wilhelmian studies. It is well researched in United States and German archives and complemented by judicious selection of secondary works. Moreover, the author's objectivity is remarkable. He notes that the German pacifists supported the *Weltpolitik* of Bülow and Tirpitz—and also that their counterparts in Italy championed the war in Libya, those in England expansion of the Royal Navy, and those in France recession of Alsace-Lorraine. Nor does Chickering fail to point out that German peace crusaders in 1914 went to war believing that they were fighting in the name of international justice. Yet, in the last analysis, I cannot escape the conclusion that the German peace movement was, as Carl von Ossietzky noted of Bertha von Suttner, permeated with "a gentle perfume of absurdity."

HOLGER H. HERWIG
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BARBARA VOGEL. *Deutsche Russlandpolitik: Das Scheitern der deutschen Weltpolitik unter Bülow, 1900-1906*. (Studien zur Modernen Geschichte, number 11.) [Gütersloh:] Bertelsmann Universitätsverlag, 1973. Pp. 335. DM 38.

This detailed study places Bülow's pro-Russian policy in the context of Germany's thrust for world power and demonstrates the impact of domestic policies on foreign affairs. Based on a wide range of unpublished sources from East and West German archives, the volume emphasizes two developments: the successful struggle, at home and abroad, to obtain ratification of the German-Russian trade treaty of 1904 and the meeting of Wilhelm II and Nicholas II at Björkö on July 24, 1905.

In connection with the trade treaty, Vogel shows how powerful German economic and business interests combined with Prussian dynastic traditions and ideology to influence Bülow's economic policy. The author believes that the meeting at Björkö, far from being a personal escapade of the emperor's (which is the conventional view) was carefully prepared for by Bülow and Holstein and formed an intrinsic part of the chancellor's foreign policy.

Vogel's thesis is that Bülow's aim was to attain continental supremacy for Germany through a Russo-German alliance in which Germany would be the senior partner. This aim was thwarted by the Russian refusal to ratify the Treaty of Björkö and the diplomatically unfavorable outcome of the

first Moroccan crisis. The author argues convincingly that a major factor contributing to this development was Tirpitz's reluctance to risk the fleet in a possible confrontation with the Royal Navy at the time of that crisis. As a result, Germany's goal shifted from Continental to world supremacy, including the building of a powerful navy in preparation for a challenge to Britain's hegemony. The conviction that 1904-06 was a period of lost opportunities became widespread in Germany and contributed to the decision not to allow the July 1914 crisis to pass without some decisive action by Germany.

The study is organized on a partly topical, partly chronological basis, which makes it somewhat repetitive and confusing for the reader. Aside from this it is a solid contribution to the growing literature on Germany's prewar policies, and it fills a gap in our knowledge of the Bülow period.

GEORGE O. KENT
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ROSA LEVINÉ-MEYER. *Leviné: The Life of a Revolutionary*. With an introduction by E. J. HOBSBAWM. Farnborough, Hants.: Saxon House, distrib. by Atheneum, New York, 1973. Pp. x, 225. \$7.75.

This book, written with feeling by a widow about her first husband who was executed in 1919, needs re-evaluation in light of historical events and elucidation of facts as we know them.

Eugen Leviné was not a victim of fascism or of "evil forces." He was not an innocent dreamer; he was a leader of the Munich rebellion to establish a Bavarian Soviet Republic and to ready Germany for the "Revolution" preached by Lenin. At the time of the Munich revolt, Leviné should have known that in Russia thousands of innocent people were being killed by the Bolshevik regime. *Reichswehr* Minister Gustav Noske, defending the Weimar Republic, had no choice but to liquidate the Munich soviets, "even at the cost of bloodshed," to prevent the bolshevization of Germany.

Leviné and other alienated Russian intellectuals wanted to save the world, but the world did not need their means toward salvation. This is the lesson derived from this memoir: violence is always bad, particularly and also pragmatically, especially when directed toward "instant" utopia.

Leviné's life had a tragic history; he was always, so to speak, in the air, suspended in space. Socially, he belonged to the wrong movement, a fact arrived at with the benefit of hindsight.

There are some facts and spellings that warrant correction: in early 1920, a Hebrew art studio was established at the Moscow Art Theatre, which continued the work initiated in Bialystok by N.

Zemakh in 1912. This Hebrew theater, called later Habimah, was subsequently transferred to Palestine. The only pre-World War I Zionist Congress was held in Vienna in 1913. The prominent Jewish Menshevik is R. A. Abramovitch.

LEON A. FELDMAN
Rutgers University

ALAN F. WILT. *The Atlantic Wall: Hitler's Defenses in the West, 1941-1944*. Ames: Iowa State University Press. 1975. Pp. xi, 244. \$7.95.

General Günther Blumentritt, chief of staff in the West, after the war stated that "the reader would find it wearisome to read long descriptions of the day-to-day happenings" in France prior to D-Day. Wilt has overcome this handicap. In *The Atlantic Wall* he analyzes the three major time phases of Germany's so-called Atlantic Wall, which encompassed the Dutch, Belgian, and French coasts as well as (after 1942) the Mediterranean beaches of Vichy France: the pre-Atlantic Wall period between October 1940 and December 1941, the formulation of the defensive line between December 1941 and October 1943, and the months preceding the Allied invasion on June 6, 1944. During the latter the Germans sought feverishly to construct a formidable barrier consisting of concrete artillery installations, foreshore barriers, barbed wire, mines, and the like.

Wilt readily admits that the Western theater was secondary to the Russian front. He attributes the eventual failure of the Atlantic Wall to lack of equipment, dearth of mobile artillery and armor, interservice rivalry (especially between the army and the navy), the independent posture of Organization Todt, which poured 17.6 million cubic meters of concrete for the Wall, lack of clear-cut lines of command, and conflict in command. Concerning the last point, Wilt refutes the common notion that the quarrel concerning deployment of panzer units in France was between Field Marshals Gerd von Rundstedt and Erwin Rommel. Rather it was between the latter and the tank commander west, General Geyr von Schweppenburg, supported by General Heinz Guderian. Whatever the case, Hitler's fateful decision on April 27, 1944, to retain personal command of four of the ten armored divisions in France proved decisive during D-Day.

Wilt has relied exclusively upon captured and incomplete German records available on microfilm in Washington. This is unfortunate. A research trip to Germany would have put him in touch with several of the main characters in the book as well as the air force and navy records and personal papers in the Military Archive in Freiburg. More serious is Wilt's virtual omission of the strategic

role that Norway already had assumed by 1940 in Hitler's and Raeder's strategies; the former regarded it as the "decisive zone of this war." While Narvik and Bergen—according to Vice-Admiral Friedrich Ruge, navy liaison officer with Rommel—were protected by the entire remaining German surface fleet and 114 artillery batteries, the 625-mile coastline between the estuary of the Somme and that of the Loire was defended by a mere thirty-seven heavy guns.

This notwithstanding, Wilt's well-written and organized book represents a significant contribution toward our understanding of the German background to the Normandy and Provence invasions in June and August 1944. It is unfortunate that the typescript reproduction is riddled with typographical errors and further marred by poor typesetting and inadequate paper binding. Both Wilt's treatment of the subject and the book's price deserve better.

HOLGER H. HERWIG
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HERBERT SCHINDLER. *Mosty und Dirschau 1939: Zwei Handstreich der Wehrmacht vor Beginn des Polenfeldzuges*. (Einzelschriften zur militärischen Geschichte des Zweiten Weltkrieges, number 7.) Freiburg: Verlag Rombach. 1971. Pp. 167. DM 9.

HORST ROHDE. *Das deutsche Wehrmachtstransportwesen im Zweiten Weltkrieg: Entstehung—Organisation—Aufgaben*. (Schriftenreihe des Militärgeschichtlichen Forschungsamtes. Beiträge zur Militär- und Kriegsgeschichte, number 12.) Stuttgart: Deutsche Verlags-Anstalt. 1971. Pp. 439. DM 58.

KLAUS REINHARDT. *Die Wende vor Moskau: Das Scheitern der Strategie Hitlers im Winter 1941/42*. (Schriftenreihe des Militärgeschichtlichen Forschungsamtes. Beiträge zur Militär- und Kriegsgeschichte, number 13.) Stuttgart: Deutsche Verlags-Anstalt. 1972. Pp. 355.

VICTOR ISSRAELJAN. *The Anti-Hitler Coalition: Diplomatic Co-operation between the USSR, USA and Britain during the Second World War, 1941-1945*. Moscow: Progress Publishers. 1971. Pp. 422.

E. H. COOKRIDGE. *Gehlen: Spy of the Century*. New York: Random House. 1972. Pp. xxii, 402. \$10.00.

WALTER ANSEL. *Hitler and the Middle Sea*. Durham: Duke University Press. 1972. Pp. x, 514. \$12.75.

CHARLES B. BURDICK. *Unternehmen Sonnenblume: Der Entschluss zum Afrika-Feldzug*. (Die Wehrmacht im Kampf, number 48.) Neckargemünd: Kurt Vowinkel Verlag. 1972. Pp. 128. DM 17.80.

The stunning result of the German Blitzkrieg against Poland suggests a nearly flawless execution of well-planned operations. Yet the opening phase was not an entirely unqualified success. Fall

Weiss planners had stipulated that the twin spans across the Vistula at Tczew (Dirschau) and the twin tunnels near Mosty be captured intact prior to the actual start of hostilities. These *Handstreich* operations proved utter failures; indeed, in the latter case, the attempt alerted the Poles who destroyed the tunnels on September 1. Herbert Schindler has traced in detail these operations and has laid to rest many exaggerated interpretations of the events. He has made excellent use of the resources of the *Militärarchiv Freiburg* and other pertinent sources and secondary accounts. The Mosty endeavor, executed by an irregular force recruited by the *Abwehr*, failed because it was carried out prematurely—namely, on August 26! Equally surprising was the very conciliatory attitude of the regional Polish command in this comedy of errors. The attempt to seize the Vistula bridges was at least not compromised by Hitler's belated reversal of attack orders. In fact, he had taken a personal interest in the plans for the operation, a joint undertaking of pioneers and dive-bombers. As Schindler shows, poor timing, inadequate contingency planning, and Polish *sangfroid* combined to deny the Germans the bridges. The German sweep into Poland, however, was not significantly delayed by these failures, and by the time the campaign in the West opened, similar German *Handstreich* operations, notably against Fort Eben Emael, had proved very effective.

Both Charles Burdick and Walter Ansel turned their attention to a much more significant issue in the German conduct of the war; they examined military operations that had a decisive impact on the strategic role of the Mediterranean area. Burdick has produced a brief, well-balanced history of the genesis of *Sonnenblume* for a popular series entitled *Die Wehrmacht im Kampf*. He relied largely on excerpts of OKW, OKH, and DAK war diaries, supplemented by studies written under OCMH auspices by former German officers, and on private diaries, memoirs, and secondary works. Although it adds little to the facts, the book offers an excellent critical summary of the developments that brought Rommel and the DAK into the Western Desert. German intervention grew out of the search for strategic alternatives to a cross-channel attack on Great Britain. Proposals for action in the eastern or western basin of the Middle Sea included plans for attacks on Gibraltar, Suez, and Haifa, support of the Italians in North Africa, and diplomatic efforts to entice the Soviet government into military action in the Persian Gulf area. Räder proved the most consistent advocate of action in the Mediterranean where, with French, Italian, and Spanish support, Germany could sever the lifelines of the empire and obtain access to a vast storehouse of raw materials. The vision of solving

the British and Russian problems simultaneously hovered into focus, only to give way quickly to new and harsh political and military realities. Burdick traces the long and detailed preparations that led to the first major clash between the DAK and the British near El Agheila in February 1941. In spite of Rommel's spectacular drive to Tobruk and beyond, Halder, among others, had few illusions about the venture once the Russian campaign opened. In conclusion Burdick offers a perceptive analysis of the many problems that ultimately defeated Rommel and his forces.

Ansel, whose long and rambling account covers much similar material, largely concentrates on the conquest of Crete. He has made exhaustive use of manuscript and printed sources and has interviewed many leading figures in the German airborne assault on the island. Although he adds many factual data on the Cretan campaign, he offers no substantive reassessment of German strategy in the eastern Mediterranean. Unfortunately, the quality of the book is flawed by a writing style that has produced careless sentence structure, imprecise language, misuse of words, and excessive minutiae concerning combat operations. Yet the book has much merit in that it places the campaign into strategic perspective and examines at length the considerations that produced *Marita* and *Merkur*, neglected the complete neutralization of Malta, and affected the Russian campaign. Can scholars, however, logically deduce that missed opportunities in the Mediterranean caused the "final fatal turning," namely, the defeat in Russia, given Hitler's virtually constant preoccupation with the Soviet threat? Furthermore, I would be reluctant to support Ansel's suggestion that if only "the professionals had been left to do their job" Moscow would have been conquered by September.

Klaus Reinhardt's incisive study of the failure of Hitler's strategy in the winter of 1941-42 offers a detailed, critical analysis of *Taifun*, the code name for the attack on Moscow. The author has made meticulous use of the *Militärarchiv Freiburg* and other primary resources as well as of an impressive number of German and Soviet studies on the battle for Moscow. He offers an informative introduction on the military, economic, and political situation in the summer of 1941, and he makes clear that operational plans had assumed the destruction of the bulk of the Soviet forces west of the Dnepr. The failure to do so had far-reaching consequences both for subsequent military operations and for the armament industry, geared, as it were, for *Breiten-* and not *Tiefenrüstung*. *Taifun* was to remedy the adverse implications of the military situation. Reinhardt's account of the military operations that unfolded between September and January 1941-42 is authoritative and compelling and shows

an excellent mastery of the human and materiel factors that affected the German offensive and the Soviet defense. The growing difference between field commanders and the OKW and OKH staffs in their appreciation of the military situation is carefully recorded, as are the dramatic efforts to stabilize the front after *Taifun* had failed. Reinhardt's monograph is a major contribution to the historical literature on the Russian campaign.

In a very useful study Horst Rohde deals with matters of transportation, which assumed increasingly serious proportions during the war, especially on the eastern front. His highly systematic description and analysis of the military transport system, from an administrative and operational standpoint, is based largely on OKH records, files of the Ministry for Armament and War Production, and archives of the former *Reichsbahn*. Rohde has also made good use of a great variety of printed sources and secondary works dealing with supply and transportation affairs. He highlights the very inadequate efforts to centralize control of the transport system, the lack of a flexible and integrated air, sea, rail, and road transport structure, the excessive reliance on rail transport, and the weak political and administrative prerogatives of the transport sector. Even within the military framework no centralized authority with corresponding executive powers ever evolved during the war. Compared to the situation in World War I, the power of General Gercke, *Chef des Transportwesens*, had in fact declined. Only within the command structure of the army did he play a very significant role and exert a measure of influence on military planning. Insofar as the OKW command structure allowed, Gercke made the most of a restricted role. Little was done to extend his control over those transportation facilities not traditionally under the control of the OKH. Air and naval commands as well as civil transport authorities rarely viewed Gercke as anything other than director of army transport. Insofar as military campaigns were concerned, Gercke found it difficult to assert himself. During the campaign in Russia, he, like others, had to face Hitler's tendency to underestimate Soviet capability, a matter compounded by the failure of the *Wehrmachtführungsstab* to note the true dimensions of the transportation problems. In his endeavor to describe and evaluate one of the less glamorous aspects of military operations, Rohde has made a very solid and needed contribution to German military history.

E. H. Cookridge, in contrast, has produced a book on military history that holds considerable fascination for a wide reading public. Moreover, Gehlen was not a man who craved or sought public acclaim, a fact that served to deepen the mys-

tery about him. As wartime chief of the German Army's *Fremde Heere Ost* (FHO) section he played the key role in the German collection of intelligence data on the Soviet Union. At the end of the war he surrendered to the Americans and placed his expertise, archives, and many staff members at their disposal. As a CIA subsidiary, the Gehlen Organization came to play a very vital intelligence role in the cold war. In 1956 Gehlen was named the first director of the Federal Republic's intelligence service, the *Bundesnachrichtendienst*, from which he retired in 1968. Cookridge, well versed in intelligence history and espionage affairs, has written a well-documented account of Gehlen's career. He had access to an impressive array of sources, not the least of which were supplied by former Gehlen associates, CIA operatives, and other members of the NATO intelligence community. Yet to speak of Gehlen as the "spy of the century" seems premature, if not naive, given the nature of the occupation. Gehlen's accomplishments are readily acknowledged, even by his adversaries; his intelligence work for the OKH and OKW, the CIA, NATO, and the Federal Republic were always highly productive. He possessed administrative flair, high professional dedication, an enormous capacity for work, and a gift for complex analytical study. To Halder, Guderian, and Heusinger, among others, he seemed to have all the qualities that Hans von Seeckt had laid down as requisites for general staff work. During the war Gehlen's estimates did not always meet with Hitler's approval, although the Führer appreciated the extraordinary abilities of the FHO chief. As the most productive of NATO's intelligence suppliers and as chief of West Germany's service, his career was not entirely untroubled. The Soviet and East German intelligence services were formidable opponents, and sloppy United States intelligence procedures did not ease Gehlen's task. Cookridge describes many of these incidents, including the Picard case that hastened the retirement of Gehlen.

Books of this type make for very interesting reading, but they usually fall far short of the true goal, namely, to account for the use to which intelligence material is put and the impact it has on the decisions made in policy formulation. Although Cookridge endeavored to present a balanced view of Gehlen's career, I take exception to the excessively flattering estimate of the accomplishments of the FHO. Critics of Gehlen in Germany have not been as kind as E. H. Cookridge.

Victor Issraeljan's book on the Allied coalition in World War II need not detain the historian in search of new Soviet interpretation or documentation. He offers the reader a traditional Soviet history of wartime diplomacy, replete with the cus-

tomary interpretations but with few references to Soviet sources. No great harm is likely to come from reading the book, a scholarly endeavor as poor in analysis as some American works of cold-war vintage.

WILLARD ALLEN FLETCHER
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GERALD STONE. *The Smallest Slavonic Nation: The Sorbs of Lusatia*. [London:] Athlone Press of the University of London; distrib. by Oxford University Press, New York. 1972. Pp. x, 201. \$12.00.

By the end of the twelfth century Slavic tribes in the area of the Saale, Elbe, and Oder rivers were either liquidated, Germanized, routed, or completely enclosed by German settlements. The Slavs of Upper and Lower Lusatia managed to survive and now inhabit a region along the Spree River from near Cottbus in Brandenburg to just south of Bautzen in Saxony. Formerly known as the Wends, a term no longer in use, these intrepid Slavs miraculously survived the horrible Nazi era, possibly because of delayed bungling by the SS, whose attention was diverted after the debacle at Stalingrad in 1943. Numerically the smallest of the Slavic peoples, numbering about 70,000 today, the Sorbs have retained their language and customs and now enjoy rights as a recognized minority in East Germany.

Stone's study, based upon interviews with Sorbian intellectuals and on-the-scene investigations, will probably remain the most complete study of these peoples. He gives equal attention to their history, literature, and language, and lesser attention to their folkways, folklore, music, and present status. Almost a curiosity or a lingering anachronism, the Sorbs (whose first serious exposure came in the late S. Harrison Thomson's chapter on them in Strakovsky's *Handbook of Slavic Studies* [1949]) must not become an endangered species. Stone's laudatory study could attract wider concern for the Sorbs and their apparent need of support for a perpetuation of their unique history and status.

SHERMAN D. SPECTOR
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HENRY KRISCH. *German Politics under Soviet Occupation*. New York: Columbia University Press. 1974. Pp. xii, 312. \$15.00.

This work relies on an extensive use of the sources available in the West (including private papers and interviews) and is in fact a detailed history of the short-lived Soviet Zone Social Democratic party, emphasizing its dialogue with the Communist party and its ultimate disintegration under Communist pressure. On the regional level Thuringian developments are featured. However, the realities

that lie outside this dialogue remain blurred, and the author does not provide details of the administrative development, the essence of the bloc policy, or the results of the factory council elections; nor can one find a convincing analysis of overall Soviet policies. Henry Krisch instead stresses attitudes and reactions, such as: "Soviet policy was carried out in the context of inherited attitudes, both German and Soviet, that influenced behaviour in 1945-1946. Above all, it must be remembered that a major and brutal military struggle between Soviet and German forces had just ended and the attitudes of that period [were] ever present" (p. 29).

Krisch does not go on to show to what extent the war experience molded Soviet postwar policy in Germany. To the contrary, he postulates a single European Soviet policy championing "national states" and favoring "all-German political institutions." Similarly, he regards a "standard Soviet policy on . . . Socialist-Communist unification" as "indisputable." But when Krisch ascribes the failure of this unification policy in Austria *inter alia* to the existence of "a single nationwide government," he leaves the reader (who recalls that Renner was instituted by the very Soviets) somewhat bewildered.

In sum, the body of this work represents a first-rate effort, which would have benefited from being put into its true perspective.

ARNOLD H. PRICE
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Konrad Adenauer, *Reden 1917-1967: Eine Auswahl*, ed. by HANS-PETER SCHWARZ. Stuttgart: Deutsche Verlags-Anstalt. 1975. Pp. 496.

HUGO STEHKÄMPER, ed. *Konrad Adenauer, Oberbürgermeister von Köln*. Festgabe der Stadt Köln zum 100. Geburtstag ihres Ehrenbürgers am 5. Januar 1976. Köln: Rheinland-Verlag. 1976. Pp. 858.

The literature on Konrad Adenauer's statesmanship is so multifaceted that it might seem as if all major aspects of his career had been thoroughly covered. The centennial of his birth brought forth, however, not only the customary solemn addresses and laudatory articles, but also some major publications that fill important gaps in our knowledge of his historical role.

After his rather involuntary resignation in 1963, Adenauer had still time and energy left to write a detailed account of his chancellorship (*Erinnerungen, 1945-1963* [Stuttgart, 1965-68]). But death prevented him from carrying out his plan to complement these memoirs by a volume of pertinent documents and speeches. Now Schwarz, who is at the University of Cologne, presents at least a selection of Adenauer's most characteristic addresses.

Many of them were not accessible to the public before. Schwarz chose them, with discriminating judgment, from about 3,000 speeches recorded in the Adenauer Archives in Rhöndorf.

The chancellor was not a brilliant orator like his contemporaries Gustav Stresemann and Theodor Heuss, but this volume impresses by the depth of his convictions and the incisiveness of his argumentation. The printed version confirms the feeling one had when listening to him: here was a statesman of dignity, forcefulness, and vision. It is fortunate that Schwarz included also some pieces from Adenauer's earlier career, beginning with his brief, but colorful inaugural address as lord mayor of Cologne (October 18, 1917).

The literature about this part of his life has been inadequate until now. Adenauer's memoirs start only in 1945, with his return to public life. The biographies by Paul Weymar (1955) and Terence Prittie (1970) give only a superficial notion of his accomplishment as chief administrator of his native town. It was, therefore, a constructive thought of the city fathers of Cologne to observe the centennial not by big festivities (such as those held in Bonn), but to honor Adenauer by a comprehensive scholarly appraisal of his career up to 1945. The editorial leadership in this task was entrusted to Hugo Stehkämper, director-in-chief of the Historical Archives of Cologne and an authority on the history of the Rhineland. He organized also, assisted by Everhard Kleinertz, a monumental exhibition in the Archives for the centennial.

Gustav Stresemann observed in 1925, having visited the magnificent Rhineland millennium exposition in Cologne, that the lord mayors (like the big industrialists) were the real kings of the Weimar era. They were elected for long terms, exerted more power than ministers, and often served also as political leaders. This remark certainly applied to Adenauer, as the memorial volume illustrates. Step by step he added new functions to his control of the city administration. He assumed a leading position in his party, the Catholic Center, especially in the Rhineland, gained key functions in the provincial self-administration, and served as president of the Prussian State Council, thus strengthening the basis of his power.

Only a few chapters in this remarkable volume can be mentioned here. Everhard Kleinertz gives a good account of Adenauer's training for leadership in the eleven years he was a member of the Cologne city administration, before being elected lord mayor. Marie-Louise Becker examines his relationship with the British occupation authorities after World War I. Friedrich-Wilhelm Henning analyzes his financial policies with fairness, but not uncritically. One of Adenauer's finest accomplishments, the revival of the University of Cologne

right after the November revolution, is described lucidly by Kurt Düwell. Ekkhard Häussermann presents fascinating new material about Adenauer and the press, especially his unusual friendship with his Social Democratic political opponent Wilhelm Sollmann, to which I had recently called attention in my essay on Sollmann in *Rheinische Lebensbilder* (vol. 6, 1975). The most important chapters for historians are by Stehkämper himself and by Rudolf Morsey, the leading expert on the Center party. Stehkämper deals with Adenauer's two unsuccessful candidacies for the chancellorship. In 1921 his own party elders preferred Joseph Wirth; in 1926 Stresemann was not keen to work with such a strong-willed man as head of the cabinet. Who dares to guess how German history would have been shaped if Adenauer had been able to form a government in 1926? Morsey's chapter on Adenauer and National Socialism is enlightening; using much previously unknown material, he demonstrates the incompatibility of his political philosophy with the outlook of Hitler and his henchmen. He also tells how Adenauer suffered for his convictions under Nazi rule. Out of the terrible experiences of these twelve years grew his determination that postwar Germany should have an ethical basis for its policies.

The carefully edited, richly documented, and well-illustrated volume is the model of a modern city history. It is commendable that the contributors did not produce a piece of hagiography but a judiciously balanced scholarly work that shows both the greatness and some limitations of Adenauer's administration of Cologne.

FELIX E. HIRSCH
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ROBERT A. KANN. *A History of the Habsburg Empire, 1526-1918*. Berkeley and Los Angeles: University of California Press. 1974. Pp. xiv, 646. \$25.00.

Robert A. Kann is the doyen of the American historians interested in the lands of the Habsburgs. The volume under review is the most recent in a long line of distinguished books and, at least in one sense, it is the most comprehensive of all of them. It goes beyond the traditional Vienna-centered narrative of the fortunes of the Habsburg family and gives us a history of the numerous people who lived under the scepter of this historic ruling house.

The initial date, 1526, is well chosen. The election of Ferdinand I to the thrones of Bohemia and Hungary in that year, followed in January 1527 by a similar event in Croatia, drastically changed the fortunes of these three peoples, but also determined that of the dynasty for roughly 400 years. It made the Habsburgs responsible for conducting

the protracted struggle against the Ottomans, turning their attention gradually away from their traditional field of action in Western Europe to their newly acquired possessions. By 1918, when the Habsburg Empire, then known as Austria-Hungary, collapsed, it consisted chiefly of these lands and the "hereditary provinces" (approximately present-day Austria) of the ruling house.

The various nations that first came entirely or partially under Habsburg rule in 1526-27 had long histories, traditions, laws, and languages of their own. Now they had to adjust to a "foreign dynasty," to the inevitable changes required by this very fact, to the absence of the ruler from the traditional capitals, and to the necessity to fit into a dynastically and politically dictated over-all scheme of which they were only a part. In many ways, the history of this new Habsburg domain is that of the successes and failures of peoples and rulers in understanding each other and working out a mutually satisfactory *modus vivendi*.

The work is organized basically along chronological-political lines. The first chapter gives the short, needed background for both the "hereditary provinces" and those lands that came under Habsburg rule in 1526-27. The fusion of these units created problems because the various lands brought different political practices and institutions into the "union." Overcoming these differences was made more difficult by the simultaneous difficulties raised by an external problem, the Ottoman danger, and by a domestic issue, the Reformation. The second chapter, ending with 1648, treats these three closely interrelated issues.

The next hundred years, discussed in the third chapter, saw the end of the Ottoman danger and the practical solution of the religious problem, but also witnessed the emergence of new problems as a result of the incomplete integration of the realm. They end with the ascension of Maria Theresa to the throne and the beginning of the War of the Austrian Succession.

The rule of Maria Theresa was a turning point in the history of the Habsburg lands. Having reached this point, Kann stops in his narrative and devotes a long, very interesting chapter to reviewing the socioeconomic trends, the estates, absolutism, Church-State relations, culture, and the administrative-judicial-defense systems during the years whose political history was the subject of the previous chapters.

The fifth chapter, taking the reader to 1815, is a combination of the political-narrative approach of the first three chapters and the analytical one of the fourth. Dealing with the reform attempts of the queen and the more radical approach of her son, Joseph II, this approach is more than justified. The various trends are so closely interwoven and

influence each other to such a large extent that the only acceptable approach is the one selected by the author, analytical-narrative.

The title of the next section, "Standstill, Decline, and Stabilization," bringing the story to 1879, is once again basically narrative. While correct in every detail and clearly presented, however, the chapter suffers from a relative overemphasis on the one hand and not enough attention given to other issues on the other. The next chapter dealing with the Enlightenment and liberalism was more successful. Here Kann's familiarity with the people of the monarchy and his understanding of them and their problems showed to great advantage.

The next two chapters take the chronological developments from 1879 to 1918. Good, solid history is presented in this part of the volume, but the treatment of these well-known years and events hardly lends itself to new interpretations. The last chapter brings cultural history up to 1918. Once again Kann proves that he understands his topic well. Together with the previous chapter devoted to this topic, this last one gives us the best short survey of the subject in English.

The volume ends with a bibliographic essay, useful appendices, and a set of helpful maps. The work before us will certainly enjoy the popularity of the author's previous volumes and will serve as a basic history and reference book for many years to come.

PETER F. SUGAR
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CLAUDE TAPPOLET. *La vie musicale à Genève au dix-neuvième siècle (1814-1918)*. (Mémoires et documents, 45.) Geneva: Alex. Jullien publiés par la Société d'histoire et d'archéologie de Genève. 1972. Pp. 215.

Claude Tappolet, author of the important account of modern Genevan musical life which appeared in the collaborative *Histoire de Genève* (1956), has now intensified his treatment of the subject in the present volume, one in the excellent series published by the Société d'histoire et d'archéologie de Genève. The basic thrusts of his narrative have remained the same: the rise of musical institutions; the contributions of gifted figures to the accelerating tempo of Genevan musical life; and the sustaining of that culture in part by an international community of French bankers, well-heeled emigrés, English gentlewomen and consuls, and the occasional political radical. The author's story is a progressive one—from the beginnings of musical life in voluntary, often secular organizations, through the founding and consolidation of the Conservatoire, the seconding of that in-

stitution's democratic impulse by cheaper public concerts, the appearance of a native school of composition, and the resiliency manifested by some of these institutions during the Great War.

In Tappolet's account the pathway to success of many contemporary Genevan musical organizations is littered with the exhumed remains of an extraordinary assortment of performing societies, most of which were the short-lived projects of some of the great *animateurs* of musical life. To the efforts of these men—and he shows how many were German or French—Tappolet pays generous tribute, and he sets forth the strengths and weaknesses of their aborted dreams.

In keeping with the format of the series to which this volume belongs, the author includes a collection of documents. Particularly interesting to the student of other Continental and English musical institutions is Tappolet's reproduction of the codes of regulations of the major successful Genevan societies. Here strike the notes, as they could not within the confines of his 1956 sketch, redolent of the problems of voluntary organization and artistic professionalism interacting within and with a society at once proudly knowledgeable of its republican heritage and proudly Swiss; at once increasingly secular, liberal, and occasionally radical, and yet deferential to the place owed the teachings and the officialdom of the Reformed religion.

It is disappointing that Tappolet largely fails to elucidate most social, political, and economic dimensions of musical life. I cannot but expect that such fruit might be yielded by the fine archives that Claude Tappolet has exploited well for his narrower purposes.

DAVID WARREN HADLEY
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J. G. A. Pocock. *The Machiavellian Moment: Florentine Political Thought and the Atlantic Republican Tradition*. Princeton: Princeton University Press. 1975. Pp. x, 602. Cloth \$22.50, paper \$11.50.

In this major work, John Pocock's analytic sophistication and formidable scholarship are employed on the tradition of "civic humanism"—a large subject. The story Pocock tells is how Machiavelli and other Florentine political theorists grappled with the problems of understanding and establishing institutions that would enable men to fulfill their natures as citizens (thus realizing eternal values) at a particular moment in time and therefore in specific conditions. The available concepts were severely limited, for time could be seen either as a sequence of experiences in a particular com-

munity, "custom," or alternatively a temporal instant could possess universal significance as an intrusion of the eternal, "grace."

Their answer was a mixed or balanced government allotting to the prudent, ambitious "few" and the numerous "many," functions suited to their natures (as had Aristotle); there might also be a leading, unifying "one" (as in Polybius). But the constitutional republic is a finite, "accidental" occurrence, achieved only in favorable circumstances, subject to external destruction by "providence" or "fortune" and to internal corruption if its citizens' virtues decay. For Savonarola and some of God's seventeenth-century Englishmen the virtuous republic could only result from grace given to a chosen people. Machiavelli explored the possibility of creating and renewing civic virtue through a variety of institutions, such as a militia; yet the *virtù* of leader or people was always threatened by time—the vicissitudes of *fortuna* and the tendency of success to cause corruption. Guicciardini advocated a larger role for the prudent and experienced few in view of the uncertainty of external affairs. For some, Venice provided ways to stabilize the republic and stop the wheel with a balanced machinery of government.

Thus we have arrived at part 3. Part 1 (pp. 3–80) sketched the notions of custom, grace, and *vivere civile*; part 2 (pp. 83–330) examined Florentine thought from 1494 to 1530; part 3 tells us how Englishmen (and North Americans) came to think of themselves as Machiavellian citizens notwithstanding their customary, ancient constitution and their puritan commitment. Pocock eschews the "rise of the bourgeoisie" and the "revolution of the saints," discovering a development through the 1640s and '50s to that moment in which James Harrington could reconceptualize English history and political thought. Later, Harrington's theory of the militant popular commonwealth was turned upside down to justify a country opposition, the House of Lords, and the ancient, Gothic constitution, and to attack standing armies, the "monied interest," stockjobbers, Walpole, and corruption. American patriots imbued with these ideas could not avoid seeing their liberty threatened by British corruption. Thus "the Revolution was paradigmatically determined and an essay in Kuhnian 'normal science'" (p. 508). Moreover, Madison's *Federalist* 10 and other American views can be seen as rearrangements of this pattern.

A work of this scope is bound to raise problems. Here I can touch only a few. First, what is the status of the conceptual apparatus deployed? Are these ways of political understanding specific to this tradition? Or are they (as Pocock's Chinese allusions suggest) general categories? The ideas discussed are occasionally called paradigms, or a

paradigm (Kuhnian?), sometimes called vocabularies or a political language, frequently modes of rendering things intelligible. (This list may not be exhaustive.) Their epistemological and ontological status needs further examination.

Moreover the book is written in several manners: part 1 explains and illustrates typical ideas, for example, "custom" by Sir John Fortescue. Texts are mentioned but rarely quoted. Part 2, however, engages in extensive detailed, even line-by-line, exegesis accompanied by quotation by the paragraph in the original language of the text. Thus the argument is microscopically focused on particular strands in the thought of Machiavelli and others. In part 3 the focal distance is again changed. We are urged to reorder our perceptions and are seldom brought to consider a text closely. In the final chapter our perceptions are so swiftly shifted that we may be dazzled by proliferating moments (Machiavellian, Rousseauist), multiplying men (*politicus, rhetor, credens, mercator, creditor, and faber*), and the third dark apparition of the problem of modernity. At one point "commerce," previously a secular analogue of *fortuna* since it leads to luxury and therefore corruption, is identified with *virtù* because both are innovatory. (A is C; B is C; therefore A is B.)

Usually Pocock invites us to consider the possibilities inherent in a set of ideas. But sometimes one is puzzled by his account of a thinker. Is he telling us what Aristotle (pp. 66–80) meant, or what he could mean, or how some interpreters understood him? Is Pocock's Aristotle merely exotic, or minced up and reconstituted by his intellectual machinery? Occasionally one fears that the huge apparatus is running wild or being converted into a steam roller cracking nuts.

Most importantly Pocock explicitly raises the serious problem of the relation between thought and action. How much of men's politics is predetermined by the politics of their language? Some situations (soldiers' pay in 1647, the conflicts of the 1780s in the not yet United States) are submerged beneath the dialectic of ideas. Yet Pocock is no idealist.

The Machiavellian Moment thus gives a magisterial account of an important tradition and challenges us to consider a number of major problems. Even those who disagree with his views or quarrel with his interpretations cannot avoid joining me in applauding John Pocock's *virtù*.

M. M. GOLDSMITH
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PHYLLIS WALTER GOODHART GORDAN, translated from the Latin and annotated by. *Two Renaissance Book Hunters: The Letters of Poggius Bracciolini* to

Nicolaus de Niccolis. New York: Columbia University Press. 1974. Pp. x, 393. \$17.50.

This book is a labor of love. Only one who is steeped in the traditions and language of the Italian Renaissance and familiar with and tolerant of the arch and artful posturings of the Italian humanists, who respects their accomplishments and recognizes the innumerable *combinazioni*, could have produced it. It is not surprising to discover that its production covers a period of some forty years. This is not a work that could have been done in one, two, or five years, nor is it one that should be read from cover to cover in a few sittings.

Two Renaissance Book Hunters is a reference work. It is fully entitled to join its illustrious companions in Columbia's *Records of Civilization*. Phyllis Gordan may rest assured that generations of students will find her work useful and usable. It is, perhaps, appropriate that the book started out as an undergraduate term paper and became a lifelong obsession.

The one hundred letters of the prolific Poggius Bracciolini represent only a part of the work of the Florentine humanist. The selection translated here is a good one, exemplary of the enthusiasm and inspiration of the "second generation" of collectors of ancient manuscripts, as well as of their occasional pettiness and not infrequent naiveté. The translation is a major accomplishment because it has captured the interlinear thought of Bracciolini, wisely translating his idioms not literally, but with a splendid understanding of the niceties and *double entendre* of Tuscan literati.

Ninety-three of the letters are from Poggius to Niccolò Niccoli; one is a dedicatory letter to Francesco Ferrari; eight are to and from other collectors. These last, appearing separately in an appendix, provide fine firsthand information about discoveries of new manuscripts and new meanings assigned to old ones.

When the work was undertaken, complete or near-complete collections of Bracciolini's letters were rarer than the Gutenberg Bible. Now, thanks to modern technology, additional copies are available. Even so, the translation is one that will have many uses.

There are only two criticisms offered, and they are minor. The insistence on the use of the classical Latin versions of names can get out of hand after a while. One can become accustomed to Poggius rather than Poggio; to Nicolaus rather than Niccolò. But Cosmus and Laurentius de Medicis? And if this be so, why Jerome (p. 174) instead of Hieronymus? As they say in Tuscany, "Macchè!"

HERBERT L. OERTER
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DEBORAH HOWARD. *Jacopo Sansovino: Architecture and Patronage in Renaissance Venice*. New Haven: Yale University Press. 1975. Pp. xi, 194. \$18.50.

This richly illustrated and accurately researched study is a comprehensive survey of the artistic career of Jacopo Tatti, surnamed Il Sansovino (1486–1570), in Venice after 1527, the year in which he fled Rome. The subsequent forty-three years, from 1527 to 1570, constituted, for Sansovino, a most creative and remarkably active experience. The artist's architectural activities were favored by the fortuitous combination of various circumstances: peace, security, and economic stability. They were further enhanced by a concern on the part of the city's fathers and the Venetian aristocracy with pomp, ostentation, and innovation. The latter justified the undertaking of a city beautification plan.

In six concise but informative chapters the author has presented a perceptive and accurate account of the numerous artistic commissions that Sansovino executed for various Venetian patrons: namely the procurators of St. Mark's, the state, the Church, the *scuole* or charitable institutions, and the Venetian patriciate. Indefatigable, imaginative, and resourceful in proposing architectural plans that could best suit his patrons' economizing considerations or artistic tastes, Sansovino was responsible for the construction in Venice of many impressive structures—for example, the library, the mint, the *Loggetta*, the *Fabbriche Nuove di Rialto*, the Palazzo Corner, and the Church of S. Francesco della Vigna. By this array, he left an indelible imprint on Venice's secular and ecclesiastical architecture, earning for himself a reputation that can be matched only by Pietro Lombardo, Palladio, and Longhena.

The idea that a city should represent a work of art was first conceived in Renaissance Italy. Its first successful experiment—the resultant of papal munificence and Bernardo Rossellino's architectural genius—was carried out in the Tuscan hill town of Pienza. The city, however, that exemplified the idea of town planning at its best was sixteenth-century Venice. Among the talented men who strove for the realization of this concept, Sansovino, as *Proto dei Procuratori di Supra* (architect-in-chief to the Procurators of St. Mark's), acted as an inspiring catalyst.

Deborah Howard's basic argument is simple, logical, and convincing. In refuting the contentions of many scholars who have denied that Sansovino developed a personal style, she maintains that the reverse is true, although economic restrictions and other controlling factors did "almost completely obscure the architect's own inclinations." If it is true that Sansovino's style evades any classifica-

tion, then, on the other hand, an analysis of his architectural structures reveals the progression of a personal style.

Sansovino's style does indeed exhibit peculiar features, such as compliance with Vitruvian orthodoxy, structural elegance, and reliance on the Roman High Renaissance style. It reflects the influence of other master architects, such as Bramante, Raphael, and Sangallo. It also shows a penchant for some favorite motifs, such as the rusticated basement surmounted by coupled columns, the elegant corner solution, the blocked balustrade, the oval Attic windows, and the long consoles flanking the mezzanine windows.

The value of the study is enhanced by informative notes, a detailed index, and a number of rich plates that add a visual dimension to the narrative. A model of felicitous writing, this monograph is characterized by an absence of factual and typographical errors. In conclusion it suffices to observe that every thoughtful student or specialist will find this study an invaluable reference work. Furthermore, it is a stimulating and scholarly monograph that fills an extant lacuna concerning Sansovino in English-speaking countries.

THOMAS A. FABIANO
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FRANCO DE FELICE. *L'agricoltura in terra di Bari dal 1880 al 1914*. (Studi e ricerche di storia economica italiana nell'età del Risorgimento.) Milan: Banca Commerciale Italiana. 1971. Pp. 502.

ALDO DE MADDALENA. *Prezzi e mercedi a Milano dal 1701 al 1860*. Volume 1; volume 2, *Grafici*. (Studi e ricerche di storia economica italiana nell'età del Risorgimento.) Milan: Banca Commerciale Italiana. 1974. Pp. 455; 99 graphs.

These two well-documented studies in Italian economic history are important additions to the series of studies promoted by the Banca Commerciale Italiana of Milan and its late president, Raffaele Mattioli.

Franco De Felice's study fills an important gap in the literature and provides valuable historical material on the problem of underdevelopment in southern Italy. Agriculture in southern Italy suffered a long feudal inheritance. It was a backward, overpopulated sector with low productivity; it lacked capital, technology, and markets in the nineteenth century; and it was able to survive only with the intervention of the state. Eighty percent of those employed were agricultural day workers. The *meridionalisti*, as De Felice shows, did not have a comprehensive grasp of the problems of Italian agriculture in the 1880s—the real problem was not to protect agriculture in the south, but to trans-

form and modernize it through the introduction of new crops, fertilizers, agricultural schools and technical instruction, machinery, livestock raising, and so on. De Felice focuses on the relationship between economic backwardness and productive relations in agriculture. One of the main roots of southern underdevelopment, he points out, was the conflict between crop patterns and technology and the various forms of land tenure systems and national economic policies. This is in sharp contrast to the development of a modern capitalistic sector in the north.

Aldo De Maddalena has contributed a monumental study on prices and wages in Lombardy between 1701 and 1860 that will be fundamental for all future work on the history of the economy and society of northern Italy in the preunification period. His study presents annual price series for twenty-seven commodities, predominantly agricultural products and foodstuffs, but also prices of manufactured goods, industrial raw materials, colonial products, and services (rents) for a period of 159 years. The wage series covers, unfortunately, only two grades of labor (bricklayers and apprentice bricklayers). Price data have been compiled from primary sources, largely from the registers of the Archivio dell'Ospedale Maggiore and from the Archivio del Pio Albergo Trivulzio in Milan. The results are summarized in the form of annual arithmetic averages and index numbers for individual commodities. Moving averages have been constructed for commodity groups for analysis of long-term trends. The study contains fifty-eight tables and ninety-nine graphs.

Agricultural prices in Lombardy show a rising trend, allowing for notable fluctuations, between the 1730s and 1850s. Industrial prices are stable through the greater part of the eighteenth century—a period of economic depression in manufacturing, little population growth, and depreciating currency. Industrial prices (primarily textiles) begin to decline in the 1830s with tariff reform and cheap imports from abroad, but they rise in the 1850s with the beginning of large-scale industrialization in Lombardy. In general, prices rise faster than wages between 1701 and 1860, indicating a steady erosion of purchasing power among consumers and the wage-earning class. Wages maintain a stable level through the eighteenth century, rising modestly only during the early decades of the nineteenth century. The extreme rigidity of the wage index in the eighteenth century, however, suggests the possibility that nonmonetary factors (payments in kind, fixed contractual rates, and so forth) may have influenced the behavior of money wages. One can barely begin to do justice, however, to the wealth of detail and the complexity of the study in a short space. De Maddalena con-

cludes from this analysis of prices and wages that the secular process of capital accumulation in Lombardy was accomplished largely at the expense of the laboring class.

One hopes that with De Maddalena's work at hand, historians will turn to the study of long-term movements in production, consumption, demographic growth, and income distribution, which accompanied these fluctuations in prices, so that we may ultimately have a comprehensive profile of the social and economic structure of one of the most economically developed regions of Europe in the preindustrial period.

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PAUL CORNER. *Fascism in Ferrara, 1915-1925*. (Oxford Historical Monographs.) New York: Oxford University Press. 1975. Pp. xii, 300. \$25.75.

Paul Corner's monograph is a successful attempt to examine the circumstances that gave rise to the Fascist movement and permitted its survival in a key province of the lower Po Valley, Ferrara. The author rightly insists that it is important to study provincial Fascist organizations, for without his provincial bases, Mussolini probably could neither have come to power in 1922 nor remained in power during the protracted crisis that followed the assassination of Giacomo Matteotti in June 1924. Moreover, the provincial Fascist movements exerted much influence on the formation of the Fascist ideology. Corner warns his readers, however, that the experience of Ferrara is not to be taken as representative of that of most other provinces. Ferrara was clearly exceptional, simply because it was one of the first areas to become Fascist and did, to some extent, lead the movement rather than follow it.

For several crucial months in 1921 the province of Ferrara was the most vigorous element in the developing national Fascist movement. It was Ferrara that formed the spearhead of the rapid expansion of reactionary agrarian fascism that effectively rescued the town-based fascism of Mussolini from political extinction. Although the province never assumed quite the same degree of importance in later years, it nevertheless continued to be a Fascist stronghold. Furthermore, it was in Ferrara that one of the most prominent Fascist leaders, Italo Balbo, made his name and established his political base. Thus, many of the themes central to the problem of the rise and subsequent stabilization of fascism are found in Corner's study of this province. Certain of them relate to the methods and organization of fascism—for example, the systematic employment of violence by the *squadristi*;

the establishment of Fascist syndicates as a means of controlling the agricultural laborers; and the clarification of the structure and decision-making machinery of the provincial Fascist federation. Others concern the splits that emerged within the local Fascist movement, as well as the growing tendency for fascism to become completely intolerant of criticism, either from without or within. Such squabbles often reflected the diversity of social origin that existed between Fascists who came from the towns and those who came from the rural areas.

Corner's study concentrates chiefly, however, on the origins of the Fascist movement. Ferrara was remarkable for the speed with which fascism asserted its control. During the postwar years of 1919-20 the province had become one of the bastions of revolutionary socialism; yet it took only weeks for the entire socialist organization to be brought to submission. Why? Violence was, of course, part of the story; but the principal factor, according to Corner, was the economy of the province. Ferrara was notable at this time for its modern capitalist systems of agriculture; yet, side by side with these advanced forms of farming were other systems that had changed very little over the centuries. The tensions arising from this contact of old and new were a real factor in provincial life—especially for those small farmers, leaseholders, and sharecroppers who felt most threatened by the growing agricultural proletariat. Thus, the real struggle in Ferrara was not so much between Fascist *squadristi* and members of the socialist and Catholic leagues as it was between the hard-pressed small proprietors and what for them were the undesirable consequences of modernization in agriculture. Corner argues that it was these landowners who were the key to the provincial struggle. The *squadristi* were a necessary element in the maneuver against socialism, to be sure, but once that maneuver was over, they ceased to have much relevance to the local situation. Balbo, for example, rapidly became disinterested in politics and turned his attention to aeronautics.

Originally a doctoral dissertation prepared under the supervision of Christopher Seton-Watson at Oxford, Corner's monograph is based largely on material found in the Archivio Centrale dello Stato, particularly the archive of the Ministry of the Interior, as well as on local and national press sources, secondary works, and some interviews. The author has also profited from suggestions by Adrian Lyttelton, Stuart Woolf, Renzo De Felice, and Giuliano Procacci. Some sections of Corner's book are unfortunately cluttered by an alphabet soup of abbreviations of obscure organizations. A glossary would have been very helpful. One hopes that Corner's otherwise excellent book will inspire

English-language studies of other provinces that figured significantly in the rise of Italian fascism.

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APOSTOLOS E. VACALOPOULOS. *The Greek Nation, 1453-1669: The Cultural and Economic Background of Modern Greek Society*. Translated by IAN and PHANIA MOLES. New Brunswick, New Jersey: Rutgers University Press. 1976. Pp. xiv, 457. \$25.00.

The scope of this formidably learned book is accurately indicated by its full title. Apostolos Vacalopoulos is interested in the antecedents of the Greek nation as defined in the nineteenth century, and he distributes attention to legal, political, economic, religious, sociological, and geographical matters in successive chapters. The book assumes full familiarity with the course of events—political, military, and cultural-religious as well. Thus, for example, despite a chapter on religion and another on education, Vacalopoulos has absolutely nothing to say about the three-cornered propaganda struggle among Roman Catholics, Protestants, and Orthodox churchmen that dominated the diplomatic stage in Constantinople during the first decades of the seventeenth century. What attracted the author's attention instead was an enormous range of items recording steps along the way toward making Greeks more like the Greeks of, say, 1821 or perhaps 1920, before Hellenism collapsed in Asia Minor.

The main use such a book can have for American readers is as a source for bits of detailed information. Indeed, the massive annotation occupying more than one-third of the volume, drawing on nearly a dozen languages, will be for many the most valuable part of the work.

Where Vacalopoulos' scholarship fails is at a conceptual level. Retrojection of nationalist classification of human activity into the Ottoman past invites enormous confusion, because such ideas were alien to the society of the time. Vacalopoulos never explains the ambiguity between "Greek" and "Orthodox Christian" that lies buried in the language he uses. He furthermore assumes rather than says that Orthodoxy was unchanging and so simply does not mention the complex and delicate doctrinal issues that constituted the kernel of "Greek" intellectual history in the sixteenth and seventeenth centuries.

Such selectivity faithfully represents views that have an almost sacred status in modern Greece; yet to an outsider they seem to have blinders that obscure the very real effort Vacalopoulos has made to maintain a cool and objective tone in men-

tioning Turks, Bulgarians, and other traditional national rivals.

WILLIAM H. MCNEILL
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EVANGELOS KOFOS. *Greece and the Eastern Crisis, 1875-1878*. Foreword by W. N. MEDLICOTT. (Institute for Balkan Studies, Serial, number 148.) Thessaloniki: Institute for Balkan Studies. 1975. Pp. 283.

Kofos has given us a sample of microhistory at its best. He deals with the Eastern Crisis of 1875-78 when the so-called powder keg of Europe, the Balkans, went through a sort of controlled explosion—controlled thanks to the determination of the major powers to prevent the conflict from driving them into a direct armed confrontation with each other. The crisis was triggered by the Russian drive to establish a strong Bulgarian state extending from the Danube to the shores of the Aegean. When the Russian armies defeated the Turks in the spring of 1878, the sultan was forced to sign the Treaty of San Stefano, which created a huge Bulgarian state. Greece was faced with the danger of being relegated to a small area in the southern tip of the Balkan peninsula, a fate that she escaped but not because of the policies of her leaders. During those critical months Greece was practically leaderless. A coalition government that included all former prime ministers, under the leadership of the revolutionary hero Kanaris, then in his nineties, was formed in May 1877. Kanaris died in September, and for the next four months none of the participating leaders would accept a replacement for the prime minister's post. Greece escaped the worst mostly because of the intervention of the British, and to a lesser extent of the French and the Austrians, who were none too happy to have a strong Russian satellite so close to the Straits.

The highlights of those momentous years have been recounted numerous times by contemporaries and by more modern historians. Most of those writers studied the crisis in the context of international diplomacy, but little attention was paid to the role played by the nationalities in the area. Kofos' valuable book does not ignore, of course, the twists and turns of international diplomacy, but it also illumines the policies and actions of the Balkan nationalities, especially the Greeks. He analyzes both official Greek policy—or the lack of it—as well as the impact of the revolutionary ferment in Crete, Thessaly, Epirus, and Macedonia.

The book offers no startling revelations that would require an extensive revision of our knowledge or evaluation of the events of 1875-78. But Kofos, in clear prose, restructures in detailed relief

the behind-the-scenes exchanges, maneuvers, intrigues, misunderstandings, or cherished illusions that so often are the unseen building blocks of history. He does so by meticulously probing into the records, the diplomatic dispatches, the governmental papers, and the confidential assessments of those directly involved.

His findings should also be of interest to those concerned with today's political scene in Greece. Although the events discussed in this book occurred one hundred years ago, they offer valuable insights into the weaknesses—the lack of a coherent strategy, the internal contradictions and feuds—that even today afflict the making of Greek foreign policy.

D. GEORGE KOUSOULAS
Howard University

HUGH SETON-WATSON. *The "Sick Heart" of Modern Europe: The Problem of the Danubian Lands*. Seattle: University of Washington Press. 1975. Pp. xi, 76. \$4.95.

Hugh Seton-Watson's slim volume is made up of three lectures delivered at the University of Washington in Seattle in 1973. The author presents the problems of East Central Europe, to him the "sick heart" of Europe, in three phases. The first is that of the three declining old empires, Austria-Hungary, Germany, and Russia. The second phase covers the interwar period, "the interlude of small states," and the third that of "the new empire," the Soviet Union. All three lectures exhibit the author's well-known qualities at their best: the ability to summarize, simplify, and arrive at comprehensive deductions.

The volume reveals, however, the reverse side of these assets as well, such as the tendency to ride roughshod over facts that may stand in the way of well-formulated and seemingly convincing conclusions and a preference for oversimplifications. It appears problematic to compare on a par three empires of which two, Germany and Russia, continued to exist in changed form after 1918 while the third, the Habsburg monarchy, dissolved. The initial statement repeated later in the text that "all three empires, it may be argued, were replaced by barbarism," from which Seton-Watson only exempts Weimar culture and Masaryk's Czechoslovakia, while in several respects correct, is nevertheless excessive when expressed so categorically. This thesis definitely does not hold true, to mention a few instances, for the early stages of the Austrian Republic, for the first years of restored Poland as compared to the last ones in the best part of partitioned Poland, and for the Romanian administration of Transylvania as compared to the Hungarian. It is an oversimplification also, though a less serious one,

to state that the basis of "legitimacy" in the western, Cisleithanian part of the Habsburg Empire was simply *Kaisertreue*. In contradiction to this, the author subsequently recognizes a number of centripetal forces that supported the monarchy; here his analysis follows in substance that of Oscar Jászi. Furthermore, is it really necessary to establish a highly subjective scale of higher and lower levels of culture among the national groups in the Habsburg Empire that may antagonize many but will benefit hardly anybody? As a last example of the author's problematic oversimplifications, according to his account of the German-Polish crisis of the summer of 1939, Hitler believed he had made the Poles "a very generous offer. They need only give him some small bits of territories and then they could be his partners in conquering the Ukraine." Here the basic distinction between preservation of territorial integrity (plus the award of crumbs as bonus) and the loss of genuine independence and reduction to satellite status is missed. Hitler was obviously quite aware of it.

This very readable volume definitely contains many apt observations, some of which modify preceding generalizations. The basic philosophy is tenable as well. Yet, readers would benefit more if, in his future works, Seton-Watson would divert his great gifts from the sweeping and didactic to the more specific and factual.

ROBERT A. KANN
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REUBEN AINSZTEIN. *Jewish Resistance in Nazi-Occupied Eastern Europe: With a Historical Survey of the Jew as Fighter and Soldier in the Diaspora*. London: Paul Elek; distrib. by Barnes and Noble Books, New York. 1975. Pp. xxviii, 970. \$37.50.

Was Jewish resistance to the Nazis fact or fiction? A scholarly work describing the facts and analyzing the problems would satisfy a real need. Unfortunately, Reuben Ainsztein never quite makes clear what it is that he is discussing: resistance of Jews who may have felt as Russians, Frenchmen, and so on, or the resistance of individuals and groups arising out of some kind of Jewish identification or motivation, namely a specifically Jewish resistance. Nor does he define what he means by resistance; he simply lists underground activities and escapes, later called "elemental resistance," alongside armed actions. Yet he nowhere describes cultural, educational, economic, or religious underground actions taken in defiance of Nazi laws. His account of underground activities directly leading to armed resistance (in Warsaw, for instance) is even more inadequate. Worse, he never analyzes the problems, nor does he draw any conclusions from his facts. The result is a vast compilation of

facts that tend to become tedious because there is no conceptual thread running through them, beyond the desire to prove that Jews could fight. I doubt whether 970 pages were required to prove that.

The first 212 pages of the book are designed to disprove the charge that Jews in the Diaspora had no martial traditions; Ainsztein produces an array of facts covering nineteen centuries, completely devoid of any analysis and removed from context. Some facts are well documented, some are not; no attempt at differentiation is made. Ainsztein's antireligious bias prevents him from probing Jewish traditions for undercurrents that might prove his point. The results of his quest can only be described as chaotic.

There then follows a summary of the "final solution," some comments on "elemental resistance" through flight, and, unaccountably, the revolts in small-town ghettos (pp. 215-75). A major part of the book (pp. 279-460) describes Jewish partisans in Eastern European forests. Ainsztein deals with ghetto rebellions in parts 6 and 7 of the book (pp. 463-681), and then death-camp rebellions (pp. 685-816). A summary chapter merely adds Jewish armed action in two more places and does not sum up anything.

A vast amount of labor has produced an occasionally imposing array of facts, which are unfortunately overshadowed by the way Ainsztein uses his sources. Soviet claims, memoirs, and so on are accepted as true; anti-Semitism among Soviet partisans is hotly denied, despite overwhelming evidence to the contrary; and religious Jewry is denigrated. Ainsztein uses almost no archival sources, excepting some materials of the Jewish Historical Institute at Warsaw. He ignores the 30,000 testimonies at the Yad Vashem Archives in Jerusalem (except for one collective testimony about Sobibór), and he ignores other archives in London, New York, and Israel. Having limited himself to published sources, the author does not use Hebrew publications at all, except two old compilations and some incidental materials. Journals such as *Talkut Moreshet* or *Yad Vashem Studies* and literally hundreds of memoirs and books published in Hebrew are indispensable for a study of this subject, but Ainsztein has passed by them.

Despite the tremendous volume of facts, some major events simply do not appear. Armed attempts at rebellion in Częstochowa, Tarnów, and Bendin and at Kielce, Opatów, Pilica, and Tomaszów Lubelski, the attempted revolt of the Jewish police at Riga, the underground in Siauliai, organized escapes to the forests from Radzyń, Rzeszów, Chmielnik—the list could be extended—all are ignored, as are the rebellions in the camps at Kruszyzna and Krychów. The ac-

count of the Treblinka rebellion (pp. 714-42) is not very accurate or convincing, but in a footnote on page 916 the author quotes at length an account published in 1953 that changes the picture and that came to the author's notice too late to be included in the text. He bases his claim on page 221 that the Nazis discussed extermination of Jews and Slavs (*sic*) before the end of 1940 on an obscure publication of 1962 that avoids quoting its sources, but a check of Charles Wighton's book on Heydrich showed no record of any such discussion. There are numerous instances of this sort of thing.

The story of Kovno is an example of the author's treatment of his subject (pp. 699-701). He claims the Judenrat, whose chairman, Yochanan Elkes, becomes Dr. Alex in the book, was compliant. Ainsztein then contradicts this by saying that the Judenrat helped the armed underground. Had he consulted Zvi Bar-on and Dov Levin's *The Story of an Underground* (Jerusalem, 1962), among other sources, he would have realized that the Judenrat was far from compliant, that the Jews were misled by Soviet partisans into sending partisans to the Augustovo forests (which almost destroyed the underground), that Elkes tried to organize a mass escape from the ghetto, and so on.

Jewish armed resistance deserves better treatment and deeper analysis. Such an analysis would have to include a differentiated picture of Gentile attitudes, of objective and subjective difficulties, and of the way they were overcome in those instances where rebellious actions took place. Above all, armed resistance can hardly be taken out of the context of unarmed preparations, or unarmed resistance. The picture would have to include Slovakia, Yugoslavia, Hungary, and the West European countries to be complete. Ainsztein's book does not fill this need, despite the great effort and enormous good will that he has obviously invested.

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high school (*Viša škola*). For a time he was the principal contributor to Svetozar Miletić's *Žastava*. Jovanović translated and published several important works, and he was a member of *Srpsko učeno društvo*. As a representative of Serbia's Liberals, he helped organize in 1866 the *Ujedinjena omladina srpska* (*Omladina*), a nationalistic youth organization influenced by the German *Burschenschaft* and *Tugendbund*, the Greek *Philike Hetairia*, and the Italian *Carbonari*.

The author discusses at considerable length the political activities of Jovanović, his position on basic issues of the time, and the organization and proceedings of the six *Omladina* congresses. He also compares Jovanović's philosophy and political tactics with those of Svetozar Marković. Until the appearance of Svetozar Marković on the scene in 1870, Jovanović was the principal ideologue of the *Omladina*. The book does not go beyond the 1870s when Jovanović became a member of the government and, in the eyes of many, compromised his principles.

More than an assessment of Jovanović's life and thought, this book is a study of the *Omladina* movement as a whole, and as such it contributes a great deal toward a better understanding of the political developments and tendencies in Serbian history during the third quarter of the nineteenth century. In fact, the author might have done an injustice to himself by focusing his study on Jovanović, who might not deserve the attention he receives, rather than on the *Omladina*, which definitely does. Also, the surprise exhibited over Westernizing tendencies in Serbia is not warranted. The important thing is, however, that the author has given us an excellent picture of a highly complicated period in Serbian history. His study is based on an extensive list of primary sources and secondary materials.

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GALE STOKES. *Legitimacy through Liberalism: Vladimir Jovanović and the Transformation of Serbian Politics*. (Publications on Russia and Eastern Europe of the Institute for Comparative and Foreign Area Studies, number 5.) Seattle: University of Washington Press. 1975. Pp. xvi, 279. \$11.00.

As the title of this book indicates, this is a study of liberalism in nineteenth-century Serbia through the philosophy and activities of one of its principal exponents, Vladimir Jovanović, who was among several young men sent abroad for education by the Serbian government in 1839. A politician, publisher, journalist, and scholar, Jovanović was also one of the first members of the Liberal party and an instructor in political economy at Belgrade's

JOZO TOMASEVICH. *War and Revolution in Yugoslavia, 1941-1945: The Chetniks*. Stanford: Stanford University Press. 1975. Pp. x, 508. \$20.00.

MATTEO J. MILAZZO. *The Chetnik Movement & the Yugoslav Resistance*. Baltimore: Johns Hopkins University Press. 1975. Pp. ix, 208. \$12.00.

F. W. D. DEAKIN. *The Embattled Mountain*. New York: Oxford University Press. 1971. Pp. xiii, 284. \$9.50.

Interpreting the activities of the resistance movements in Yugoslavia in World War II, a many-sided civil as well as international struggle, was a matter of endless confusion and bitter controversy

at the time, not only among the embattled Yugoslavs but also for the outsiders who were playing for high stakes in this strategic piece of Balkan territory. History did not wait for historians to dispell the confusion and clarify the record; decisions were taken, campaigns fought, aid provided to one side or another. Tito's partisans won the civil war, while Mihailović and the Chetniks lost. But the legacy of the struggle remained, and the arguments over what had happened, and what might have happened, went on. They are likely to go on indefinitely, no matter how many sober scholars examine the evidence and present their reasoned conclusions. But it is a significant fact that, after thirty years, some competent and objective historians are doing just that.

Two American scholars, Jozo Tomasevich and Matteo Milazzo, have now given us admirable detailed studies of one of the parties in the civil war, the Chetniks. Working separately, they cover much the same ground and use many of the same sources, especially the extensive wartime records of the German and Italian occupation forces. (Although neither mentions the work of the other, they must have bumped into each other in the archives.) Milazzo's book is particularly good in describing the struggles taking place in areas annexed or occupied by Italy. Tomasevich's study, the first of a planned three-volume set that will cover the partisan movement and the quisling forces as well as the Chetniks, is broader in scope with background chapters on the prewar period and others on international diplomacy and the affairs of the Yugoslav government-in-exile.

On the issue that wracked all occupied nations, that of collaboration with the enemy, it became fairly clear during the war that the Chetniks were making deals in order to obtain arms (supplies dropped by the British were very meager), to carry on the fight with their domestic rivals, and to preserve their forces for the eventual rising when Western Allied armies would come to liberate Yugoslavia. Both books provide mountains of evidence that the collaboration was manifold, massive, and continuous; that Chetnik commanders made arrangements with the Germans, with the Italians, and even with the quisling Croatian regime in some instances; and that Mihailović himself, not just his lieutenants, deliberately chose that course. It flowed logically and almost inevitably from the fact that after the breakdown of the half-hearted efforts for Chetnik-Partisan cooperation in 1941, these rivals were in a war to the death to determine the country's future.

F. W. D. Deakin's *The Embattled Mountain* also covers the question of Chetnik collaboration. As one who was with the partisans through some of their most harrowing campaigns under heavy Ger-

man attack and who admired their fortitude and resolve, he could not be expected to be benevolent or neutral in his feelings toward the Chetniks, whose units were shooting at him and his comrades-in-arms. He was a keen and accurate observer, however, and his observations confirmed what earlier British missions (most of them sent in to make contact with Mihailović) had already concluded, though they had not always been able to get the word through to those responsible for high policy. Deakin's conclusions come through loud and clear, and his reports to his superiors and his extensive oral briefing of Winston Churchill in Cairo after returning from Yugoslavia in October 1943 had something to do with the decisive turn of British policy from Mihailović to Tito.

Deakin's book is a hybrid, as the author himself concedes. The first and last parts provide a gripping account of his own experiences when he was dropped by parachute in May 1943 into the cauldron of the fierce and bloody battle of the Sutjeska, and when he later took part in the events of the dramatic weeks following the Italian surrender. In between he describes the organization, policies, and personalities of the partisan movement, and he also reconstructs the story of the various British missions to the Yugoslav resistance groups. Here there is considerable new material, much of it based on conversations with Britons and Yugoslavs who were involved. The book is not the comprehensive study of the Yugoslav resistance that Deakin has declared his intention to write, but it remains a gem, a remarkable combination of personal memoir and historical inquiry written with spirit and style.

The story of the Chetniks, of course, did not end with the switch of Allied support to Tito in 1943. Mihailović was still hoping that events and possible American support would justify his strategy. He gained encouragement from the presence of an OSS mission under Colonel Robert McDowell, which remained with him until late 1944, long after the king and the government-in-exile, under British pressure, had dropped Mihailović and taken the road toward compromise with Tito. America's policy was not as clear as Britain's, but there was no real chance that Roosevelt would split with Churchill on Yugoslav policy by accepting the pro-Chetnik recommendations of McDowell. Tomasevich, who interviewed McDowell and looked at all the other evidence, covers this singular episode thoroughly and well.

It was the tragic destiny of Mihailović—a brave man, patriotic according to his own lights—that his base and his appeal were too narrow; he was essentially Serbian rather than Yugoslav in his loyalties. The Germans kept a firm grip on historic Serbia, his source of potential mass support, until

the Soviet army came in, and even at that late date he was still fighting the partisans instead of leading a rising against the Germans; and in the struggles waged in Bosnia, Croatia, and elsewhere the Serb population was decimated in Ustaši massacres and in the civil war (which was in large measure a war between Serbs). The Chetniks, as Tomasevich demonstrates again and again, could rally only a part of Yugoslavia's Serbs to their side and none of the other nationalities. Anticommunism was for them a genuine cause, but antifascism, the partisan watchword, was a stronger one for the bulk of the population. By fighting in the present, the partisans helped to shape the future, while the Chetniks made fatal compromises for a future that never came.

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ANDREW C. JANOS and WILLIAM B. SLOTTMAN, editors. *Revolution in Perspective: Essays on the Hungarian Soviet Republic of 1919*. (Russian and East European Studies.) Berkeley and Los Angeles: University of California Press. 1971. Pp. x, 185. \$10.00.

One of the essays in this volume is of classic stature. Written by Andrew Janos in an expressive English laced with Hungarianisms and carelessly proofread, the essay is based on extensive original research in Hungarian language sources. It undertakes, successfully in my opinion, the revision of the standard view of the history of *Ausgleich* Hungary as one of intransigent and feudal immobility. Rather, Janos argues, Hungary in the years 1867-1918 underwent a rapid, far-reaching, and centrally directed process of modernization.

The driving mechanism in Hungarian modernization consisted of two elements: a new state bureaucracy organized after 1867 and a political party (the Liberal) that without interruption managed to hold a majority in the lower house of parliament from 1875 to 1905. Both bureaucracy and party were made up primarily of persons of gentry origin. Through its domination of local government the bureaucracy controlled some 160 "rotten boroughs," located in the areas inhabited by the minority nationalities, so that the party had only to win some fifty seats in purely Hungarian districts, where elections were contested, in order to retain its majority.

Naturally, the party also looked after the interests of the bureaucracy, but the main concern of the new machine was that of national power. To this end the political machine rationalized the legal structure of the country, began the modernization of agriculture, improved the transportation net, encouraged industrial growth, and, above all, artificially depressed rural wages in order to provide for the accumulation of capital. Considering

the emergence of an industrial proletariat as a necessary evil, the gentry machine championed the emancipation of a rapidly increasing Jewish population, helping it become Hungary's first business class, in command alike of industry, banking, and commerce. The bureaucracy simultaneously swelled as a consequence of the effort to absorb the talented youth of the minority populations, which in this way were deprived of their own leadership; by 1918 roughly forty percent of ranking bureaucrats had non-Magyar names.

Modernization created its own opposition, however. The magnate class, which controlled fifty to seventy seats in the lower house and harbored reservations concerning both national state and market economy, attempted to undercut the new policy. A more far-reaching opposition developed among that portion of the ethnically Magyar gentry that could not be provided for in the administration. On the right, the gentry opposition stood for independence, the acquisition of a Balkan empire, and the achievement of European great-power status. On the left, the opposition gentry ended by collaborating with the Socialists, whose leadership in turn came from those elements of assimilated Jewry that could also find no place in the system. This prepared the way for the Károlyi *Gironde* of 1918-19. The workers, who provided the Socialist constituency, chose to concentrate on the fact that their living standards were low in comparison with those of Western Europe, rather than to remember that they were better off than other Hungarians. The peasantry, whose living standard under *Ausgleich* declined in absolute terms, was sufficiently affected by the growing volume of communication to express its simmering frustration in the bloody disorders of the Stormy Corner. Even the state bureaucracy found that its salaries had become the largest single item in the national budget. Modernization failed because the economic base was not broad enough to permit the Liberal machine to pay off all strategic groups.

The Janos essay takes up the first third of this tiny volume. Useful and competent chapters directly concerned with the Hungarian Soviet Republic are provided by Peter Kenez, Keith Hitchens. William B. Slottman, Richard Lowenthal, and Janos himself.

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MARIUSZ KULCZYKOWSKI. *Andrychowski ośrodek płócienniczy w XVIII i XIX wieku* [The Textile Center of Andrychów in the 18th and 19th Centuries]. (Polska Akademia Nauk—Oddział w Krakowie. Prace Komisji Nauk Historycznych, number 31.) Cracow: Zakład Narodowy imienia Ossolińskich, Wydawnictwo Polskiej Akademii Nauk. 1972. Pp. 238. Zł. 45.

Kulczykowski's monograph is a study in the field of economic and social history. The author deals with the textile center that consists of seven villages surrounding the manor of Andrychow, which is located forty miles southwest of Cracow. The work chronologically spans the period from approximately 1750 to 1830, and it involves a population of twelve thousand at any given time.

In the introductory chapter the author offers a concise review of pertinent literature. He pieced his work together on the basis of information from archives in Warsaw, Cracow, and Lvov, which is now in the Soviet Union. He extracted a great deal of data from court books, from birth, baptismal, death, and matrimonial records, as well as from private collections and other less conspicuous sources. In this manner, Kulczykowski shows a new method of scholarly investigation. He also conducted a careful study of the topography of the area in terms of the drainage systems, the quality of soil, the type of forests—factors facilitating the production of linen.

The Andrychow weavers were peasants, bound to the manor by law, and were mostly illiterate. Kulczykowski shows that the whole social and economic life of the seven villages was centered upon the manufacture of linen. Having grown sufficient quantities of foodstuffs, the farmers cultivated flax, the staple raw material in linen production. Every household was in some way involved in this industry. The Andrychow textile center was unique in the author's opinion because the peasant-weavers controlled the entire apparatus of production, including the auxiliary crafts and the marketing of their product. This was in contrast to analogous centers in France, Prussia, and Russia, where outside factors intervened in the process. As regards marketing, the author proves, to his own amazement, that the practically illiterate peasant-mercers from Andrychow successfully vended their linen personally in distant places like Constantinople, Moscow, Warsaw, and the Balkans.

By reference to specific cases, Kulczykowski is able to bring out the real people of Andrychow: the small entrepreneurs, often indebted, with families living and working under the same roof. The thirty-three charts help the reader to understand the author's thesis, although illustrations of the equipment used by the weavers would also have proved useful. But to end on a positive note, the book illuminates the diverse relations between the peasants and the privileged nobility of Poland.

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EMANUEL HALICZ. *Partisan Warfare in 19th Century Poland: The Development of a Concept*. Translated by JANE FRASER. (Odense University Studies of His-

tory and Social Sciences, number 25.) Odense, Denmark: Odense University Press. 1975. Pp. 220. Dkr. 70.

After the partitions of their country, Polish patriots pondered over the dilemma of how a defenseless nation could recover its independence. What kind of an insurrection was feasible and most likely to succeed? Should it be directed against all the partitioning powers or solely against Russia, which held most Polish territories? Could final victory be won only through major battles or could guerrilla warfare also destroy the enemy?

In his interesting book, Emanuel Halicz explores nineteenth-century Polish military thinking together with its political and socioeconomic implications. The author, an emigré Polish historian, has written among others a monograph on the duchy of Warsaw and studies in military-political history. His present work makes a contribution to a field largely neglected by Western historians; given the relevance of partisan warfare for the present it might also interest nonspecialists.

Halicz provides his frame of reference in the introduction in which he also defines such terms as small-scale warfare, partisan warfare, and people's war. He proceeds then to a critical review of the polemical writings by Polish military theorists from the Kosciuszko-inspired pamphlet *Can the Poles Fight Their Way to Independence?* to the revolutionary vision of Kamieński. This indubitably rich body of thought has a utopian overtone, and one is tempted to suggest as a subtitle to the volume "from the myth of the scythe to the myth of people's war." Halicz ties Polish military thinking to Italian writings on partisan warfare and to general contemporary European experiences. Perhaps his most important and telling point is that advocacy of partisan warfare is not a satisfactory criterion for determining one's political ideology. Such a view is largely at odds with official present-day Polish historiography.

The translation is good, although there are occasional slips; perhaps the translator has stayed too close to the original Polish text. A nonspecialist may find the numerous details and the length of some of the quotations somewhat tedious. He will be gratified, however, by the good illustrations and maps. It is a pity that the book has no bibliography, but all in all it is a valuable study that enriches the meager body of Western-language publications concerning Poland in the nineteenth century.

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JAN KOZIK. *Między reakcją a rewolucją: Studia z dziejów ukraińskiego ruchu norodowego w Galicji w*

lalach 1848-1849 [Between Reaction and Revolution: A Study of the Ukrainian National Movement in Galicia in 1848-1849]. (Zeszyty naukowe Uniwersytetu Jagiellońskiego, 381, Prace historyczne, number 52.) Summary in English. Cracow: Uniwersytet Jagielloński. Pp. 236. Zł. 36.

There are technical and ideological hurdles confronting anyone writing on the complex events in Galicia in 1848-49. Indeed, the apparent specialized nature of the subject would militate against the need for a full discussion of the complexities of such labels as progressive and conservative, as well as a discussion of Engels' misjudgment of the events. Yet the role of the Ukrainians in Galicia remains important for understanding the vacillating policies of the Austrian government and the complex interaction of nationalism and socialism in Eastern Europe.

Jan Kozik clears the ideological hurdle by basing his analysis on his sources, not upon any preconceived predilections. He removes the technical hurdle, namely a paucity of sources, by his discovery of such primary materials as the long-lost minutes of the *Holovna Rus'ka Rada*, the major Ukrainian organization that emerged from the upheavals of 1848. He thereby renders a welcome service to students of the history of the area as well as of Ukrainian historiography. Moreover, Kozik makes full use of all other sources, including the contemporary press, which has not been readily available to earlier writers. Finally, he has made exhaustive use of secondary works and monographs, so that his book provides scholars not only with a discussion of the subject, but also with a full bibliography.

One can argue with Kozik's criticism of the pro-Austrian policies of the *Holovna Rus'ka Rada* and with his analysis of some of the policies of the Poles. He fails to point out that in Galicia "governmental policy" was often that of the local Polish administration and did not necessarily reflect the views of Vienna. These, however, are professional disagreements between specialists, which in no way detract from the overall excellence of the work.

The book is useful reading for anyone interested in the nationality policy of the Habsburg realm, in the Polish national movement, and in the formation of modern Ukrainian political and cultural organizations.

MARTHA BOHACHEVSKY-CHOMIAK
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I. S. IAZHBOROVSKAIA. *Ideinoe razvitiie pol'skogo revoliutsionnogo rabochego dvizheniia (konets XIX-pervaia chetvert' XX v.)*. [The Ideological Development of the Polish Revolutionary Workers' Movement

(The End of the Nineteenth-Beginning of the Twentieth Centuries)]. Moscow: Izdatel'stvo "Nauka." 1973. Pp. 413. 1 r. 60 k.

This latest contribution of I. S. Iazhborovskaia to the historical literature on Polish Marxism is an exhaustively researched account of the development of the Polish Social Democratic movement from the founding of the party "Social Democracy of the Kingdom of Poland" (SDKP) in the spring of 1893 to the August 1923 Second Congress of the Communist Workers party of Poland, when a Polish party "of a new type" (Leninist) was established. In her study, the author draws heavily from the underground and exile press of the Polish movement as well as from documents of the SDKPiL and letters recently published in Poland and in the Soviet Union of prominent social democrats. In addition, Iazhborovskaia cites interesting portions of previously unpublished letters and notes of Feliks Dzierżyński, Cesaryna Wojnarowska, and Adolph Warszawski (Warski) located in the Central Party Archives of the Institute of Marxism-Leninism in Moscow.

The title of the book is misleading; the author does not discuss the revolutionary workers' movement. Instead, she concentrates on the leadership of the "left-wing" of Polish Social Democracy: the SDKPiL (the Lithuanian component was added in 1899), the PPS-left (founded in 1906), and Ludwik Kulczycki's PPS-"Proletariat" (1900-07). According to Iazhborovskaia, the political and ideological maturation of these parties depended on the proper understanding of the dynamics of the Polish working class, the "social-psychology" of the Polish "national-liberation movement," the vanguard position of the Russian revolutionary movement (the Bolsheviks), and the revolutionary potential of the Polish peasantry. Owing primarily to the distorting effects of the struggle against Pilsudski and the chauvinistic "social-patriotism" of the PPS, no Polish party sufficiently understood all the forces in Polish society. Despite their "short-sightedness" and "mistakes," Iazhborovskaia concludes, these parties still contributed to the progressively more "correct" (Leninist) application of Marxism to the problems of Polish class and national consciousness.

Nowhere in Soviet historiography is the cult of Lenin and its devastating effect on history writing so pervasive as in the literature on the Social Democratic movement. Iazhborovskaia's work is no exception, despite the valuable information she provides the Western reader. In an otherwise relatively balanced picture of the intellectual development of Rosa Luxemburg (especially on the national question), for example, the author misrepresents the important series of polemics be-

tween Luxemburg and Lenin on the organizational question. Their differences on this issue prior to 1905 are minimized. Luxemburg's famous critique of the Russian Revolution of 1917—the antibolshevism of which is over emphasized by both Western and Soviet historians—is simply omitted. Even Luxemburg's and Lenin's genuine friendship and the SDKPiL leader's enthusiastic response to bolshevism in the years immediately following the 1905 revolution are buried in the rhetoric of the cult of Lenin.

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JANUSZ JĘDRZEJEWICZ. *W służbie idei: Fragmenty pamiętnika i pism* [In the Service of an Idea: Memoirs and Writings]. London: Oficyna Poetów i Malarzy; distrib. by Piłsudski Institute of America, New York. 1972. Pp. 355. \$7.00.

The publication of a new primary source is always a significant event in the world of scholarship. These memoirs and writings by Janusz Jędrzejewicz (1885–1951)—the Polish minister of education from 1931 to 1934 and the premier in 1933–34—were compiled, edited, and annotated by the author's brother Wacław, who succeeded him as minister of education and who was in his own right an important figure in Polish interwar politics. Part 1 consists mainly of the author's recollections of his early personal and professional life and of his public service in the Piłsudski government. For the most part they were written during his exile in Palestine after 1941 and his subsequent second exile in London. Part 2, a collection of Jędrzejewicz's addresses and speeches, provides a moral and theoretical explanation of Piłsudski's political philosophy. Though marked by a "great man" approach toward Piłsudski, Jędrzejewicz's account of the interwar political milieu in Poland is critical and frank. Jędrzejewicz emerges as a political moderate concerned with practical and concrete ends rather than with ideological or factional issues that dominated his associates.

The "idea" Jędrzejewicz served was, in the narrow sense, the wholesale restructuring of Polish education, which he carried out in 1932 and 1933. In the wider sense, Jędrzejewicz's "idea" was—as it was for Piłsudski—no less than the political, moral, and religious renovation of Polish life based on patriotism, loyalty, public service, and traditional principles. Jędrzejewicz's reforms created a uniform and logical multitrack system that reflected his personal conservative values and those of the regime. The political implications of his program, especially its elitist and statist character, drew opposition from both left and right extremists as well as from the Union of Teachers. The financial problems of the government in the

1930s reduced the effectiveness of Jędrzejewicz's impressive and ambitious plan.

Owing to their fragmentary nature, the memoirs are of uneven interest or value and do not yield a composite view of the period or the personalities. Contrary to my expectations, the section dealing with Jędrzejewicz's public role as minister and premier was not particularly informative. While it is always valuable to have a minister's own explanation of his policies, this account does not add substantially to existing information. The recollections pertaining to Jędrzejewicz's premiership are too incomplete to be of much use to scholars. The major interest of part 1 is Jędrzejewicz's dramatic account of the political maneuvering in Poland after the death of Piłsudski and his sensitive portrait of Walery Sławek. The close associate and heir apparent of Piłsudski, Sławek's influence waned after the death of the marshal; rebuffed and isolated, he ended his own life in 1939. Jędrzejewicz's profound understanding of Sławek's personality and political ideas, plus his insightful treatment of political machinations, contribute substantially to our appreciation of the internal turmoil in Poland on the eve of the Nazi invasion.

In addition to the fragmentary and uneven quality of the memoirs, other problems serve to limit their value and utility. The chronology in Jędrzejewicz's texts, for example, is not always easy to follow; the editor might well have supplied the relevant dates in brackets. Moreover, the silhouettes of personalities—with the exception of the profiles of Piłsudski, Sławek, and Śmigły-Rydz—are not as extensive or complete as the reader is led to believe. At times, Jędrzejewicz merely compiles anecdotes. As a whole, however, the memoirs are stylistically well written, astute, and forthright. Jędrzejewicz clearly explains his own positions and does not dissemble in appraising his associates in the Piłsudski government. Several points come through strongly in the memoirs: the economic and financial troubles of the period, the interplay between leftist and conservative ideologies, and the chronic factionalism that enervated the Polish Republic.

KENNETH F. LEWALSKI
Rhode Island College

EDWARD D. WYNOT, JR. *Polish Politics in Transition: The Camp of National Unity and the Struggle for Power, 1935–1939*. Athens: University of Georgia Press. 1974. Pp. xvi, 294. \$12.50.

When, after more than a hundred years of foreign rule, the Polish state was re-established in 1918, it was faced with a host of apparently insurmountable problems. The country was underdeveloped, the Great Depression further com-

pounded its economic problems, approximately one-third of the population of Poland consisted of national minorities, and, last but not least, it was faced by powerful and hostile neighbors both to the east and the west. By the late 1930s, with the international situation rapidly deteriorating in Europe, the external threat acquired alarming dimensions and at the same time the country found itself in the throes of a serious domestic political crisis.

After the death in 1935 of Marshal Piłsudski—the man who had played a key role in restoring Poland's independence and who had ruled the country as virtual dictator since 1926—political struggle developed simultaneously on two levels. First, there was the opposition to the continuation of the Piłsudskiites' dictatorship by political parties ranging from the extreme left to the extreme right. At the same time there was the internal rivalry, often no less bitter, among the various groups of Piłsudskiites themselves.

Edward Wynot's study outlines the activities of the different opposition parties and national minority groups, but it primarily describes the struggles among the Piłsudskiites. He traces step by step the rise of the group led by Marshal Śmigły-Ridz over the rival factions led by Colonel Ślawek and President Mościcki. Wynot describes the structure and activities of the Camp of National Unity, the mass political movement supporting Śmigły-Ridz, and he carefully analyzes the camp's unmistakable evolution toward totalitarianism.

Polish Politics in Transition is one of the best studies of the dramatic events in Poland in 1935–39. It is well researched, clearly organized, and carefully balanced in its judgments. As such it represents a valuable contribution to scholarship of interwar Polish history.

ADAM BROMKE
McMaster University

CHRISTOPH KLESSMAN. *Die Selbstbehauptung einer Nation: Nationalsozialistische Kulturpolitik und polnische Widerstandsbewegung im Generalgouvernement, 1939–1945*. (Studien zur modernen Geschichte, number 5.) [Düsseldorf:] Bertelsmann Universitätsverlag. 1971. Pp. 277. DM 34.

This study consists of two parts. The first and longer section is devoted to the conquerors; the second to the conquered. The first part illuminates Nazi intentions of enslaving the Poles. Culturally, the Nazi design was to deprive the Polish people of all but rudimentary education; like slaves, the Poles were left without cultural and esthetic assets and without an intellectual elite. Klessmann am-

ply demonstrates this thesis in his discussion of the Nazi-planned schools, the status of art during the occupation, the rigid control of the press by the German authorities, and the Nazi destruction of those institutions that had served to preserve the Polish heritage.

As the author shows, however, the Nazis were not wholly successful in implementing their designs. Competition and animosity among the leaders of the General Government and the civil, military, and SS authorities hindered many plans. Administrative chaos, changes in Nazi procedures and thinking, and the opposition of the Polish people also impeded the carrying out of the Nazi blueprint.

The last part of this volume focuses upon three elements among the Polish underground: the educational structure, the press, and the literati. Pre-1918 conspiratorial considerations guided the illegal schools. Polish educators in the underground—many of them leaning to the left—attempted to achieve during World War II what the Old Regime had denied them: the democratization of education. Academia acted under diverse conditions, and the almost semilegal institutions paired with the secret ones. At the same time, writing, in the press and elsewhere, followed strict partisan lines and conveyed ideas along the entire political spectrum. Although Janus-faced, the original, thought-provoking output was somehow meager, as Klessmann indicates.

This scholarly work, balancing description with analysis, evokes associations, and the author alludes to a few. He describes the tradition of resistance to oppression among the Polish people, and he notes the differences between the residents of Warsaw, who were more experienced in conspiracy, and the residents of Cracow, who were relatively unseasoned in underground activities. (No doubt, a more systematic comparison of Poland's sections from the times of partition would be enlightening.) Furthermore, Klessmann records the similarity of the Polish and Czech experiences under the Nazis (he could add the Slovenes' as well), but unfortunately without elaboration. Poland contained, too, a ready-made testing group, the Jews, although Klessmann only points to the frequency with which the Polish Jewish community was studied. Yet, one feels compelled to ask what caused the Polish Jews to react in so similar a fashion to the Polish Gentiles in their resistance (including the cultural and intellectual aspects) to their mutual captors.

Klessmann's scholarship is a welcome addition to the literature on wartime Poland and on anti-Nazi resistance.

YESHAYAHU JELINEK
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ANN M. KLEIMOLA. *Justice in Medieval Russia: Muscovite Judgment Charters* (Pravye Gramoty) of the Fifteenth and Sixteenth Centuries. (Transactions of the American Philosophical Society, New Series, volume 65, part 6.) Philadelphia: the Society. 1975. Pp. 93. \$5.00.

This monograph, a revised Ph.D. dissertation produced in Horace Dewey's workshop on late medieval-early modern Russian law at the University of Michigan, is based on a meticulous and definitive analysis of about 156 published judgment charters. These documents, which summarize the cases, were awarded to the victorious litigants to show that they had won court suits, should the matter be contested in the future. (About 30 of these judgment charters are translated with extensive commentary as part of Dewey's and Kleimola's *Russian Private Law. XIV-XVII Centuries. An Anthology of Documents* [1973].)

Ann Kleimola, currently at the University of Nebraska, is to be congratulated for her mastery of the Middle Russian legal and vernacular language of these documents. More interestingly, she has progressed beyond the philological stage to write a first-class piece of history, one that uses current secondary materials. The judgment charters show the legal life of Muscovy in the fifteenth century, when justice had become triadic (the court supervised the process, but did not run an inquisition; the litigants themselves were responsible for making a case move along). Written records were replacing the earlier oral stage, but the state had not yet developed adequate record-keeping facilities to retain all verdicts. When the state bureaucracy reached that level around the beginning of the seventeenth century, according to Kleimola it ceased issuing judgment charters.

Using each judgment charter an average of six times while analyzing all aspects of Muscovite justice they portray (officialdom; litigants and their claims; trial procedures—testimony of witnesses, documentary evidence, duels and oaths; and instances), the author concludes that there was a high degree of legality in Muscovy. Decisions were rational, predictable, according to written law and custom, and even intelligible to Americans four centuries later. Justice was not necessarily class-oriented, so that a peasant could win in litigation with a prince. Corruption was at most of minor significance. The work, which also shows the high degree of Muscovite centralization, is a major contribution that will be of interest to students of legal, social, and Russian history.

RICHARD HELLIE
University of Chicago

R. G. SKRYNNIKOV. *Perepiska Groznogo i Kurbskogo: paradoksy Edvarda Kinana* [The Correspondence of

Groznyi and Kurbskii: The Paradoxes of Edward Keenan]. (Akademiia Nauk SSSR, Institut Russkoi Literatury.) Leningrad: Izdatel'stvo "Nauka." 1973. Pp. 136. 83 k.

The correspondence between Tsar Ivan IV Groznyi of Russia and Prince A. M. Kurbskii, and Kurbskii's history of Ivan's reign, have been considered the most original literature and important historical documents of sixteenth-century Moscow. Kurbskii was Ivan's military commander in the Livonian War. He defected to Lithuania in 1564 after which he purportedly wrote Ivan from Wolmar, Livonia. By 1579, he allegedly wrote four more times, and Ivan replied to him twice. Although there has never been a critical edition of the correspondence or of the "history," they have appeared in many editions and translations and have been cited extensively by historians. It is not, then, surprising that when Edward Keenan suggested that the entire correspondence and the "history" were forgeries (*The Kurbskii-Groznyi Apocrypha* [1971]), it raised a scholarly furor. (See the extended reviews of C. Halperin, A. Kappeler, and I. Auerbach, *Jahrbücher für Geschichte Osteuropas*, 22 [1974]: 161-213.) Keenan's discovery that a passage of the "Complaint" of the Lithuanian Orthodox monk Isaiah dated to 1566 had been copied into the Kurbskii letter of 1564 was the starting point of his argument against authenticity. Keenan further argued that Prince C. I. Shakhovskoi in the 1620s was progenitor of the correspondence. He then traced a logical textual development of additions and later editions throughout the seventeenth century.

R. G. Skrynnikov, the author of three books and many important articles on Ivan IV, now has written a sweeping rejection of Keenan's thesis which reaffirms the traditional dating and authorship. In the process Skrynnikov offers fresh evidence and analysis of the manuscript tradition of the correspondence and of related documents, of the conditions under which Kurbskii wrote the first letter and Ivan his long answer, of the likelihood of Shakhovskoi's authorship, and of the content and language of the letters. He does this against a framework of contemporary and later sources on the reign of Ivan IV (Kappeler, and G. Orchard, *Canadian-American Slavic Studies*, 16 [1974]: 448-59, discuss Skrynnikov's arguments in detail). Skrynnikov's reconstruction of the manuscript tradition differs fundamentally from that of Keenan and is more convincing. His reconstruction of the events of Kurbskii's defection and of the "Wolmar convoy" of letters, within which Kurbskii's first letter appeared, also is persuasive. The new textual evidence includes analysis of the manuscript containing Isaiah's "Complaint" by which Skrynnikov

shows that the "Complaint" probably was written before 1564. Skrynnikov also makes a case for Isaiah's links to Orthodox emigrés in Wolmar to explain how Kurbskii obtained the document. Finally, Skrynnikov shows that the content of the correspondence correlates well with other sixteenth-century literature. Especially important is his discovery of a reference to letters of Kurbskii and Ivan in the "Opis' tsarskogo arkhiva," a document of unquestioned sixteenth-century origin. Much in Skrynnikov's analysis remains problematical, but the case he makes is strong. The burden of proof remains with Keenan and others who might continue to question the authenticity of the correspondence, and it is a heavy burden indeed.

DAVID B. MILLER
Roosevelt University

O. N. VILKOV, editor. *Goroda Sibiri (Ekonomika, upravlenie, i kul'tura gorodov Sibiri v dosovetskii period)* [The Cities of Siberia (The Economy, Management, and Culture of Siberian Cities in the Pre-Soviet Period)]. (Akademiia Nauk SSSR, Sibirskoe Otdelenie, Institut Istorii, Filologii i Filosofii.) Novosibirsk: Izdatel'stvo "Nauka." 1974. Pp. 297.

G. P. BASHARIN. *Nekotorye voprosy istoriografii vkhozhdeniia Sibiri v sostav Rossii* [Some Questions on the Historiography of Siberia's Entry into the Russian State]. Iakutsk: Iakutskoe Knizhnoe Izdatel'stvo. 1971. Pp. 134.

A. N. KOPYLOV. *Ocherki kul'turnoi zhizni Sibiri XVII-nachale XIX v.* [Essays on the Cultural Life of Siberia from the 17th to the Beginning of the 19th Century]. (Akademiia Nauk SSSR, Sibirskoe Otdelenie, Institut Istorii, Filologii i Filosofii.) Novosibirsk: Izdatel'stvo "Nauka." 1974. Pp. 251.

Siberia's historical relationship to European Russia remains an important issue in Soviet historiography. These three works on Siberia's "feudal" past (1580-1850) agree upon the ultimately positive, progressive results of Russian conquest. Otherwise, they have little in common. Narrow in focus and strictly factual is the anthology, *The Cities of Siberia*, containing fourteen relatively unrelated, vignette-style essays. They discuss aspects of social life, architecture, industry, and administration in selected towns or areas. The reader must pose broader questions, such as: What did these towns have in common? What military role did they originally play, with their defense forts and paramilitary personnel (*voevody, sluzhilye liudi*, and Cossacks)? When and how did industrial capitalism and a bourgeoisie emerge? The editor, however, making no claims to overall synthesis, merely dedicates the book "to the important, but weakly elaborated problem of the Siberian town in our historiography."

Kopylov's work, *Essays on the Cultural Life of Siberia*, is of major historiographical interest. The author presents excellent surveys of education, architecture, painting, and theatrical entertainment (literature is excluded). Regarding Siberian culture as "part of overall Russian culture," he points out common developments as well as regional variants from the Russian norm. More a province than a colony of Russian culture, Siberia was in step with developments in European Russia in the areas of literacy, technical and public education, town planning, and theater. However, in the introduction of baroque and classical architectural styles and in the emergence of secular painting and artists, Siberia experienced a cultural lag.

State, church, and eventually urban public initiative comprised the most active forces after 1700 in advancing cultural innovations, usually along Western lines. State motivations were chiefly educational (to acquire a body of literate, trained officials at the local level) and often arbitrary. Without a gentry, in Siberia the church played a larger role in education, art, and theater than it did in Russia proper. Also atypical were the contributions of exiles and scientific expeditions, especially in painting. Kopylov concludes that the "official ruling culture" was more influenced by a "democratic" input in Siberia than Russia. Although he asserts that native Siberian ornamentation entered into art and architecture, neither text nor illustrations substantiate this. Claims of a distinct "Siberian baroque" style are likewise unconvincing. Despite minor flaws, Kopylov's study is exemplary in its research, organization, and thoroughness.

Broadest in conception, and least satisfying of the three works, is Basharin's *Some Questions on the Historiography of Siberia's Entry into the Russian State*. The book lacks integration and amounts to an exercise in "history as it should have been" and "should have been written." Historiography is used as a base for lambasting "falsifiers" of Siberian history (at home and abroad, recent and past) and for interspersing indictments of Nazi, Japanese, and Western imperialism. Following the majority line among Soviet historians, Basharin staunchly contends that Iakutia, Siberia, and other outlying areas entered "voluntarily" and peacefully into the Russian territorial complex—and not by conquest or forced annexation. In contrast to Kopylov and the anthology contributors, Basharin stresses "correct" historiography and opinion, giving little attention to the documentary record.

Of the three works, Basharin's raises the most provocative questions concerning the nature of any territorial absorption, the relationship between central authority and new territory, attitudes of

masses and elites, natives and settlers. Analogies with the American "winning of the West" often come to mind. Though all three studies unconditionally paint tsarism in dark colors, the state itself is shown as administrative unifier, cultural pacesetter, defender of Russian territory, and upholder of "Russia's historical mission" (Basharin, p. 70).

STEPHEN WATROUS
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S. E. TOLYBEKOV. *Kochevoe obshchestvo kazakhov v XVII-nachale XX veka: Politiko-ekonomicheskii analiz* [The Nomadic Society of Kazakhs in the Seventeenth to the Beginning of the Twentieth Century: A Political-Economic Analysis]. Alma Ata: Izdatel'stvo "Nauka," Kazakhskoi SSR. 1971. Pp. 632.

This study of the Kazakhs in the three centuries before the 1917 Revolution serves three purposes for Western historians: first, since it is a substantially revised and enlarged version of a 1959 book published by Tolybekov, *Obshchestvenno-ekonomicheskii stroi Kazakhov v XVII-XIX vekakh*, it shows the professional development over a decade of the "dean" of Central Asian historians; second, it provides an inside view of the state of Soviet historical and sociological science; and, third, it gives the most current interpretation held by Soviet historians of the effect of annexation and revolution upon the Kazakh people. It is sad that a fourth purpose, providing important historical and sociological knowledge about the Kazakhs, is not one of Tolybekov's goals.

More than twice as long as his first effort, the book under review shows both methodological and substantive maturation but almost no development of a conceptual nature. While Tolybekov earlier had been content to base his more surprising statements about Kazakh society on a few carefully selected texts of Marx and Lenin, in this volume the author takes great pains to expand his source material. He has examined archival materials, has read many more prerevolutionary authors, and, save for the notable lack of any non-Russian or non-Soviet accounts, has used virtually all relevant materials. Yet the introductions and conclusions to every topic considered are exactly the same as before. Not a single supposition or conclusion has changed. This is a remarkable feat in an area subject to such intense debate and about which so much has been written in the last decade. Tolybekov has expended all of his efforts at providing more documentation of both a theoretical and substantive nature for his earlier conclusions.

The work is representative of the type of problem that Soviet historians and sociologists are

studying. It concentrates on the benefits of Russian and Soviet modernization on the Kazakhs rather than on the results of modernization in a traditional society. Theoretical and methodological input is aimed at the justification, not analysis, of events in Kazakhstan. Tolybekov provides evidence of a considerable amount of debate within Soviet scholarship on this topic. Harshly criticizing the works of A. Erenov and I. Zlatkin, who wrote on feudal relationships among the Kazakhs, Tolybekov shows that there is not unanimity on the conclusions drawn from the evidence, just unanimity on the correct topics and approaches to pursue.

Finally, the reader learns that not only did the Kazakh people greatly benefit from having joined the Russian state, but that the unification was the result of a natural development. The Kazakhs before the mid-nineteenth century did not have a society, a state, or a culture of their own. They "could not" have had these, for their society was a good example of a "semi-nomadic" construction that, in Tolybekov's view, exists in a no man's land between a feudal nomadic society and a patriarchal settled one. The rulers of such a society draw their power from the nomadic base; the people exist in a settled condition. Thus the semi-nomadic society contains such social and political inner contradictions that it is meaningless to discuss state or culture in one. An example of the extent to which Tolybekov is willing to go to prove his case is that in his first book he included a Chinese fourth-century poem, purportedly from nomadic portions of the state. In his second book he repeats this poem (p. 52) but deletes two lines that speak of "motherland." Here he uses this to support his case for the nomadic Kazakhs' lack of thought on nation or homeland.

In his introduction Tolybekov states that his task is "using Marxist-Leninist methods, to clarify the main questions of the socio-economic structure of the nomadic economy of the Kazakhs . . . , to analyze the facts of the history of the Kazakh people, and to examine the benefits to the Kazakh people of their annexation to Russia." A task that includes the third goal must of necessity ignore the second. Tolybekov does.

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DAVID L. RANSEL. *The Politics of Catherine II: The Panin Party*. New Haven: Yale University Press. 1975. Pp. x, 327. \$17.50.

David L. Ransel's monograph is an important addition to the growing list of revisionist writings on the reign of Catherine II. Its subject is the relationship between Catherine's government and one of

the networks of clan ties and patronage that united and divided her statesmen. Ransel sees these networks, which he calls "parties," as being critically important to the workings of Catherine's government, for it was through them that the monarch ruled, first aligning herself with one group, then controlling and counterbalancing it, and eventually replacing it with another more suited to her changing tastes and purposes.

Within this general framework Ransel focuses his attention on the "party" led by Nikita Panin. Refuting Sacke and others who saw in Catherine's statements and actions of the 1760s the results of a struggle between the empress and an aristocratic opposition led by Panin, Ransel shows that the Panin group was in fact predominant throughout most of that decade and collaborated enthusiastically and loyally with Catherine. Identifying good government with themselves and their own political views, the members of Panin's group developed their criticisms of Catherine's regime in the 1770s as they lost influence first to the Orlovs and then to Potemkin, until finally Nikita Panin himself was dismissed in 1781. Their critique of the Potemkin era in the 1770s and '80s established the fundamental position for those who would view Catherine negatively, but it was misunderstood and misused both by her successor, the Emperor Paul, and by historians when they misapplied that critique to the whole of her reign, including the early years, when the Panin group itself had been in favor.

The great problem for both the author and the readers of this book is the absence of any work as good as Ransel's on Teplov, Viazemskii, the Orlovs, the Chernyshevs, and others. Because of that, Ransel has trouble fitting some of the participants into the bipolar system of politics he envisions. At one point he describes Viazemskii as "a man attached to neither of the leading parties," but elsewhere calls him Orlov's "creature." Similarly, although he is vague on the political relationship of Teplov and Shakhovskoi to Panin, Ransel uses some of their actions and writings (but not others) to indicate Panin's position on certain issues. Ultimately, Ransel's focus on Nikita Panin blurs and distorts his view of other statesmen, including Catherine herself.

Nevertheless, Ransel's book is more convincing than any of its predecessors in depicting the politics of Catherine's court, and it contributes significantly to our understanding of her reign.

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FRANCIS LEY. *Alexandre I^{er} et sa Sainte-Alliance (1811-1825)*. Preface by PIERRE PASCAL. Postscript

by GEORGES BIDAUT. Paris: Librairie Fischbacher. 1975. Pp. 328.

The title of this excellent study is precise. Beginning with a careful account of the "*cheminement spirituel*" of Tsar Alexander I under the impact of various religious and mystical groups, Francis Ley traces in great detail the fortunes of that strangest of diplomatic documents, the Treaty of Holy Alliance, from its drafting by the Tsar in 1815 to his death in 1825. In a concluding chapter, "*L'Agonie de la Sainte-Alliance*," Ley shows how Alexander's genuine hopes for a new age in diplomacy and religion dissolved under the conservative pressures of Metternich abroad and of reactionary leaders within Russia.

When this reviewer undertook some forty years ago a study of Madame de Krüdener—the name generally associated with Alexander and the Holy Alliance—he soon found that many important documents used, albeit imprecisely, by the Genevan Charles Eynard in his basic two-volume biography (1849) were no longer to be found. In 1959, through the well-known Paris bookseller, Raymond Clavreuil, he met Francis Ley, a Paris banker and a direct fifth-generation descendant of Madame de Krüdener. In the salon of the Ley villa at Asnières were the missing documents, a veritable "Krüdener archive." Using this material Ley published his *Madame de Krüdener et son temps* (1961), a work crowned by the French Academy. Now, with other unpublished material and an exhaustive use of the printed sources in French, German, English, and Russian, Ley gives us a splendidly written and meticulously documented account of this unusual chapter in European history. His bibliography is indispensable.

Ley has not attempted to give a full diplomatic history of the period. He keeps Alexander in the central position, demonstrating the impact upon him of a large number of foreign figures such as Jung-Stilling, Franz von Baader, the Herrnhuters, and the English Quakers, as well as Russians such as Golitsyn, Kochelev, and Labzine. By 1815, all this had become a veritable bombardment directed, one must feel, to a man already convinced that he was acting under the immediate guidance of God. Yet with the revolutionary outbreaks of 1820, Alexander came to associate some of the "mystics" with a new revolutionary temper which he abhorred. Hence he turned against them and died a tragic figure.

Ley's story, in outline not new, has a solidity which earlier accounts lacked. He has done for Alexander what the psychohistorians might wish to have done, but he has worked by means of massive documentation and legitimate interpretation. It is difficult to see how this important

contribution to religious, political, and social history could have been better presented.

ERNEST JOHN KNAPTON
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J. L. BLACK. *Nicholas Karamzin and Russian Society in the Nineteenth Century: A Study in Russian Political and Historical Thought*. Toronto: University of Toronto Press. 1975. Pp. xvi, 264. \$17.50.

The study of Russian historians in the English-speaking world has been surprisingly neglected, especially in view of their importance for shaping views on the evolution of the Russian state and society. J. L. Black's book is a welcome addition to the limited Western analysis of Russian historians.

The book is not narrowly historiographical and considers Karamzin in the broad context of Russian political and historical thought. The author's major purpose is to demonstrate the "social and political nature" of the contribution made by Karamzin's writings to the image held by Russians of themselves and of their past. In light of this goal, the best single chapter is chapter 5 ("The *History* and Russian Society in the Nineteenth Century"), for here the author is most concrete in specifying the contribution of Karamzin's writings. Text-books, historical novels, scholars' lectures and publications, and the salon conversations of the literati reflected the wide distribution, use, and impact of the *History*.

Given its singular success with the Russian reading public, the *History* became significant as a symbol of Russian nationalism. This subsequently led to the development of "Karamzinism," wherein Karamzin became an icon to his supporters and was identified with political and social views occasionally not his own. Black's view of Karamzin is essentially that of an eighteenth-century figure committed to the rationalizing effect of enlightenment for both ruler and ruled, but who reacted in a patriotically conservative manner to influences and events growing out of the French Revolution. Eventually Karamzin's *History* became an obstacle to the further development of Russian historical scholarship, owing to its use by both liberals and conservatives as a touchstone for conservative views on Russia and its political future.

Black successfully explicates Karamzin's historical themes and illustrates the extent of their acceptance, but his achievement stimulates the wish that it might be possible to delineate the extent and depth of Karamzin's persuasiveness in nineteenth-century Russian society with greater precision. Perhaps as Western and Soviet scholars devote more attention to the study of nineteenth-century Russian historians, there will be less likeli-

hood of judging that these matters are "incalculable," as Black assumes.

Based on printed sources, the book contains a selective bibliography emphasizing recent books and articles. Published with care in an attractive format, the book should be in all good collections on Russian history in both undergraduate and graduate libraries.

CARL W. REDDEL
United States Air Force Academy

RICHARD G. ROBBINS, JR. *Famine in Russia, 1891-1892: The Imperial Government Responds to a Crisis*. (Studies of the Russian Institute, Columbia University.) New York: Columbia University Press. 1975. Pp. xiv, 262. \$12.50.

Richard G. Robbins has discovered a compact body of archival material, especially in the papers of the Economic Department of the Ministry of Interior and the Special Committee on Famine Relief, which chronicles the Russian government's reaction to the great famine of 1891-92, and which details its efforts at famine relief. He has worked through these previously unstudied papers carefully to produce a model historical monograph. True, the topic itself is narrowly focused. But it is meticulously researched, the narrative is carefully constructed, and the conclusions are solidly based.

It is the administrative question—the efforts of the tsarist government to deal with the scourge of famine in the late nineteenth century—which concerns Robbins. A discussion of the manner in which state policies evolved and were implemented in response to the famine of 1891-92 constitutes the major portion of his work. The author's careful analysis of documents in Soviet archives effectively disputes many of the myths which have emerged during the past three-quarters of a century concerning the manner in which the imperial government dealt with the famine. Certainly, he shows clearly that the Russian statesmen were deeply concerned with the famine, and that their efforts to meet the crisis can hardly be characterized as incompetent or completely ineffectual. "The [Russian] government succeeded in mounting one of the largest relief campaigns in Russian history" (p. 168), Robbins writes, and his detailed discussion of the government's efforts on that score more than proves his point.

This immensity of the Russian government's relief campaign cannot be doubted; more to the point in assessing its impact is the question of its effectiveness. Certainly, as Robbins indicates, the efforts of the tsar and his advisers were not without shortcomings. More important, however, Robbins proves that despite "serious institutional and political obstacles, . . . government assistance averted

the very real threat of mass starvation, held the death rate within acceptable limits, and prevented a total economic collapse in the stricken region" (p. 173). Yet despite the government's considerable success in dealing with the immediate crisis, the famine of 1891-92 was a critical watershed in Russia's history. The debate over the famine had politicized Russian life, and, as Robbins argues, "too many questions had been posed by the famine; too many aspirations had been raised by the regime's own programs for combatting the crisis" (p. 182). The result, he maintains, was that "the government's attempt to return Russia to the calm it had known during the *zlatisha* of the 1880s was doomed to fail" (p. 182).

In conclusion, Robbins' study considerably broadens our knowledge of how the imperial government functioned at the end of the nineteenth century. Equally important, his study adds to our understanding of how absolutist (and underdeveloped) states seek to deal with grave social and economic crises.

W. BRUCE LINCOLN
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BERND BONWETSCH. *Kriegsallianz und Wirtschaftsinteressen: Russland in den Wirtschaftsplänen Englands und Frankreichs, 1914-1917*. (Studien zur modernen Geschichte, number 10.) [Gütersloh:] Bertelsmann Universitätsverlag. 1973. Pp. 256. DM 29.

In this published dissertation, Bernd Bonwetsch contributes substantially to our understanding of British and French economic goals vis-à-vis their erstwhile Russian ally during World War I. In the process the author also helps, though not explicitly, to put the issue of German war aims into a comparative perspective. There was, Bonwetsch makes clear, no lack of Anglo-French ambitions toward Russia. Both powers had designs for markets, raw materials, and commercial advantages; what was lacking was an ability to translate those goals into reality.

The study's conclusions are quickly stated. Britain and France wanted to replace Germany as the major supplier of goods to Russia. British private interests pressed for concessions but with inconsistent government support; in France, by contrast, the government led the effort to secure long-term arrangements, especially postwar supplies of raw materials and preferential tariff treatment. In these endeavors London and Paris were driven by a realization that the American colossus might soon pre-empt their opportunity to supplant Germany. Despite minor concessions, obviously derived from the war situation, neither Britain nor France succeeded in its schemes. Their failure stemmed in part from Russia's own determination

to control its postwar economic destiny, in part from the progressive deterioration of the Anglo-French economic position as the war continued.

These findings suggest a Russian government with far more freedom of action than either Soviet historians or the late G. F. Hallgarten would concede to the tsarist regime. Yet Bonwetsch's case would have been helped, and certainly made clearer, had he indicated the total amount of economic exchanges—including war loans, the sale of strategic goods, and deferred payments—that took place among the Triple Entente countries. For background on these matters, René Girault's *Emprunts russes et investissements français en Russie, 1887-1914* (Paris, 1973) is indispensable. Further, Bonwetsch would have buttressed his position had he consulted more private collections, especially those of senior British officials. And the entire work would have benefited from a conclusion that actually concluded. These caveats notwithstanding, the author has provided a useful guide to a hitherto *terra incognita*—inter-Allied relations during World War I, a terrain where it was almost as dangerous to be a friend as a foe.

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NORMAN STONE. *The Eastern Front, 1914-1917*. New York: Charles Scribner's Sons. 1976. Pp. 348. \$15.00.

Norman Stone argues in this book that the Russian defeat in the First World War cannot be explained by economic backwardness alone. He shows that the shortage of munitions, while real, was exaggerated by some generals to cover up their own mistakes. The main cause of the shell shortage of 1915 was that the Russians, just like everyone else, had expected a short war. By 1916 they had managed to make up the deficiencies. According to Stone the country suffered not so much from the lack of economic development per se as from the feverish attempts to overcome it. Modernization exacerbated the social tensions that ultimately brought down the empire. This thesis is well taken and a useful corrective to those who would accept unicausal, simplistic explanations of Russia's defeat.

The great value of the book, however, is not its novel thesis but the author's ability to present military history. Stone knows a great deal about strategy and tactics; he is familiar with questions of military organization and has an impressively thorough knowledge of the armies of the Central Powers. As a result, he is able to put the Russian effort in comparative perspective. In his best chapter he describes the Brusilov offensive and is able

to pinpoint the achievements of the Russian general as well as the errors of the Austrians.

In spite of its virtues this is an extremely irritating book. Stone is a historian in the tradition of A. J. P. Taylor: he believes his main task is to debunk legends and disprove fabrications. (He uses the words "legend" and "fabrication" with distressing frequency.) Obviously it is useful to question commonly accepted generalizations, but the Taylor approach has two dangers: the historian is tempted to set up straw men in order to knock them down, and in being overanxious to contradict prevailing notions he may cease to be judicious. In this book Stone often makes both errors.

For example, there is no need to "disprove the legend" that Russian agriculture at the time of the First World War was dominated by a few large estates. No serious historian holds such a view. Similarly the author is knocking on open doors when he essays to prove that agriculture was not damaged by the war. Michael T. Florinsky, in his important book, *The End of the Russian Empire* (1931), made the same point. (This title, incidentally, does not appear in Stone's thin and unsatisfactory bibliography.)

The author usually champions the unconventional point of view: he takes Falkenhayn's side against Ludendorff's, and Sukhomlinov's against everyone else. He believes that the *zemgor* and the war industry committees made only a small contribution to the war effort and that the Russian army did not fall apart in 1917. These judgments are suspect because Stone obviously does not have the same firm grasp of Russian history as he has of military matters. For instance, it is wrong to say that "Lenin could lay some claim to patrician status" when in fact his grandfather was a serf. It is untrue that a poor man could not attend the Academy of the General Staff because of the high cost, for not only was the academy free, but the officers received a salary while attending it. It is far too simple to divide the prewar officer corps into two groups: patrician and "preaetorian" (whatever that means). The confident assertions that "the Red Army in 1918 established a partnership with the people and therefore there was a rush of volunteers to join it" and that "the Bolsheviks attracted what was the best in the Russian officer corps" are simply false. If Stone was correct it would be hard to understand why the Reds in the spring of 1918 could not suppress the White Volunteer Army that had only three thousand participants. The author's belief that the tsars wanted to use the peasants against their masters betrays great ignorance of Russian social history.

This book should be read, but with caution.

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LEOPOLD H. HAIMSON, editor. *The Mensheviks: From the Revolution of 1917 to the Second World War*. Translated by GERTRUDE VAKAR. (Hoover Institution Publications, number 117.) Chicago: University of Chicago Press. 1975. Pp. xxiii, 476. \$22.50.

This volume is the result of seminars that involved surviving Mensheviks and some American and European scholars. The five Mensheviks, who contributed essays based on documentary evidence and recollections, concentrated only on events personally known to them. The four-part product is not a comprehensive history of menshevism from 1917 to 1939, nor even a chronologically or thematically consistent volume. The greater part of the book concerns the pre-Stalinist Soviet period with only slight coverage of the party in emigration. The work is quite valuable for its bibliographical information, for its factual detail, and, most importantly, for what it reveals about the Menshevik movement and its influence on twentieth-century Russia.

Though the essayists maintain that objective circumstances more than ideology dictated Menshevik policy in 1917, the book clearly reveals the Mensheviks' blind commitment to doctrine thereafter. Most important was the theory of the bourgeois character of the Russian Revolution—the primacy of economics, which according to their laws of historical development made the collapse of the Bolshevik regime inevitable. Some Mensheviks even saw the New Economic Policy as a confirmation of the traditional concept of bourgeois revolution, even though the NEP witnessed an onslaught of systematic persecution of the party and by 1923 saw the Mensheviks driven underground. As for 1917, the theory of bourgeois revolution was slightly hazy on the question of coalition, which bedeviled the Mensheviks between March and November. The Menshevik Geneva Conference of April–May 1905 had breached the theory with the point about the "partial and episodic" participation of Social Democracy in governmental organs created by the revolution (hardly unequivocal prohibition as some scholars write). Therefore, though justified in their skepticism concerning the primacy of theory in 1917, Leo Lande and Boris Sapir do not buttress their argument with an analysis of the period between March and June 1917 to demonstrate that menshevism indeed formulated policy on the basis of objective circumstances and not according to theory. The essayists' failure to treat this crucial period in the history of menshevism is a major weakness of the book. However, the memoirs of I. G. Tseretelli reveal that he, as the pervading force in menshevism's apogee, tried to have his cake and eat it, too; that is, Tseretelli and his associates tried to prevent civil war by joining a coalition as individuals, not

as leaders of their party (at best an imperfect compromise between circumstances and theory).

While circumstances and theory shaped the Menshevik position on state power, the consequences of their views, as democrats, derived entirely from theory. The Menshevik F. I. Dan has noted that the paradox of socialism versus democracy led menshevism to emphasize democracy more than socialism after 1905, while bolshevism sought socialism at the expense of democracy. As these essays reveal, not only did their democratic commitment limit their field of action, but their ability to act as well. As democrats, they relied solely on the art of persuasion and agitation to accomplish their goals—shunning any suggestion of coercion. The essayists show that the Mensheviks decided after November 1917 to combat the Bolsheviks—but peacefully. According to these essays, however, circumstances, not theory, decided the Mensheviks' fate. The Menshevik strategy of an anti-Bolshevik sympathy among the workers failed because the party could not foresee Lenin's surrender to Germany, the willingness of Imperial Germany to recognize and support his regime, and the terrorism that was to outrage the world. Menshevik strategy was not confined to snatching slogans from the Bolsheviks but of snatching the Bolsheviks out of Lenin's hands. Even when menshevism finally proved unequal to the contest, the movement's saving grace was its commitment to democracy and human decency that did not degenerate into totalitarianism, of which Lenin was the architect.

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ROMAN KUPCHINSKY, compiler. *Natsional'nyi vopros v SSSR: Sbornik dokumentov* [The Nationalities Question in the USSR: A Collection of Documents]. (Obshchestvenno-Politicheskaya Biblioteka, number 42, Dokumenty, number 13.) Munich: Suchasnist. 1975. Pp. 440. Cloth \$9.50, paper \$7.95.

This anthology constitutes the first attempt to compile recent materials, which are devoted exclusively to the nationalities question, originating in the Soviet Union, the majority of which fall into the category of *samizdat*. The first six chapters deal with the Ukrainians, the Baltic nations (Latvians, Estonians, and Lithuanians), Crimean Tatars, Belorussians, Jews, and Georgians; the final chapter includes documents on the Armenians, Meskhethians, and Germans, as well as two collective declarations in defense of national rights signed by representatives of the Baltic states, the Ukraine, and the Caucasian region.

The largest single group of documents deals with the Ukrainian national movement and includes, among others, such important materials as

the recommendations of the Republican Conference on Problems of the Culture of the Ukrainian Language (1963) to party and state authorities, the petitions and works of the well-known dissidents Valentyn Moroz, Sviatoslav Karavansky, and Ivan Dziuba, and Mykhailo Braichevsky's historical essay "Annexation or Reunification?" Documentation of the national movements of the Jews, Crimean Tatars, and the Baltic nations is not as well represented, and I feel that although the anthology is quite bulky (440 pp.), in view of their significance, additional materials relating to these groups should have been included. This is particularly true in the Jewish case. Finally, another aspect of the nationalities question in the USSR that might well have been taken into consideration in the preparation of such a volume is the position of various Russian nationalist (and in some cases chauvinist) and neo-Slavophile groups vis-à-vis the non-Russian nationalities.

It is clear that Kupchinsky and the Suchasnist publishers are to be congratulated for bringing us such a fine collection of documents on the current Soviet scene. In view of the continually growing Western interest in the nationalities question in the USSR, it is hoped that we shall soon see an English-language edition of this useful and important anthology.

ROMAN SOLCHANYK
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GEORGE ALEXANDER LENSEN. *The Damned Inheritance: The Soviet Union and the Manchurian Crises, 1924-1935*. Tallahassee, Fla.: Diplomatic Press. 1974. Pp. xi, 533. \$19.80.

The inheritance to which the title of this book refers is the Chinese Eastern Railway, with its diplomatic, strategic, and economic importance. It was bequeathed to the Soviet Union by Tsarist Russia. For various reasons the Soviet Union found it inadvisable to act on the spirit of the Karakhan Manifesto and surrender its rights to the railroad. As a result, it was obliged to defend them by force against a Chinese challenge in 1929 and then sell them, under duress, to Japan in 1935.

Lensen's account is based primarily on the foreign relations documents recently published by the Soviet Ministry of Foreign Affairs, the Japanese Foreign Ministry's secret *History of Negotiations between Japan and the Soviet Union* (declassified and republished in 1969), and British Foreign Office archives opened in the mid-1960s. His major thesis is that the brief Russian sally into Manchuria was a strictly limited operation in response to extreme provocation and that the Soviet Union's policies after 1931 amounted to an orderly retreat in the face of a Japanese takeover, which it was powerless to prevent. Such a nonaggressive policy, Lensen

argues, should dispose of the contention that Japanese expansion in Manchuria was to some extent justified by the Soviet menace, for no real threat existed. (As a corollary Lensen points out that the Russian incursion in 1929 was so different from the Japanese conquest of Manchuria that failure of the powers to support China in the former instance can scarcely be judged the fatal precedent that led to the Pacific War.)

While the corollary seems reasonable, the major thesis is less compelling. Diplomats' reports and exchanges provide too narrow a range of evidence for an understanding of the reasons why a given policy was pursued. Russian military weakness vis-à-vis Japan in Manchuria in 1931 is no index of the strategic threat that she might have been expected to pose within a few years. Japanese perceptions of and attitudes toward Russia need to be integrated into an overall analysis of Japanese foreign policy in a more systematic fashion than is attempted in this study, particularly in view of the extreme tension between the two countries in 1931-34 that Lensen documents (not to mention Soviet efforts in Mongolia and China proper). It should also be noted that Lensen's treatment of the Russo-Chinese dispute of 1929, while providing a useful corrective to the anti-Russian interpretation, does not do full justice to the Chinese.

Still, the diplomatic record of these controversies remains an indispensable part of the story, and Lensen has provided an extremely valuable and detailed introduction to it.

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JOHN ERICKSON. *The Road to Stalingrad*. Volume 1, *Stalin's War with Germany*. New York: Harper & Row. 1975. Pp. x, 594. \$25.00.

In 1962, John Erickson wrote *The Soviet High Command: A Military-Political History, 1918-1941*. Thirteen years later, despite the appearance of much new material and the publication of several similar studies, his book remains the definitive work on the subject and is likely to remain so. *The Road to Stalingrad*, although not quite up to the same Olympian standards, is destined to receive the same recognition.

In view of the sheer size of the project, it might have been better to follow the Russian example of devoting one volume to each year of the war. Nonetheless, Erickson has written the best narrative to date of the first seventeen months of the greatest armed struggle in history. He traces in minute detail the history of Hitler's preparation for operation "Barbarossa," the paralysis and confusion of the Soviet government and military in the early

days of the war, the Soviet counterattacks, the devastating defeats in the summer of 1942, and, finally, the first phase of the Battle of Stalingrad. He presents several possible reasons for the unpreparedness of the Red Army in 1941 and, even more perplexing, for the failure of Soviet intelligence and armed forces in the summer of 1942. He tends to blame Soviet military doctrine for these catastrophic defeats, and he shows that Stalin was a meddlesome bully and not at all an exceptional military leader. His views on the effect of the purges on the performance of the Red Army, however, are ambiguous and even contradictory (pp. 6-7, 20).

Most writers depend primarily on Western sources; Erickson has an encyclopedic knowledge of Soviet sources as well, and he uses them with great discretion. His preface to the study is an excellent guide on how to use Soviet sources and is essential reading for those who want to make use of Soviet historical writings. It is inexplicable why he does not use General Grigorenko's *Der sowjetische Zusammenbruch*. This is the harshest criticism of Stalin's military leadership, in which Grigorenko persuasively argues that in 1941 the Red Army was both quantitatively and qualitatively superior to the Wehrmacht. Also, in his references Erickson omits a fair number of important sources, such as the 1969 edition of *Kievskii Krasnoznamennyi*, which remains the most detailed Soviet description of the campaign on the southwest front. Despite these omissions, his extensive use of Soviet sources is one of the strongest points of the book. It is doubtful that any significant Soviet material is going to be released in the near future. Since the ascendance of the Brezhnev-Kosygin regime, omission and falsification as well as glorification of Stalin's role have been the rule in writings about the military events of 1940-42.

The book is not without faults. Erickson has decided to dispense with footnotes and instead to rely on a bibliographical essay for each chapter, a practice that I find unsatisfactory. There are no maps, not even copies of the excellent Russian ones, and there are no tables of command and lists of units, information that is easily obtainable. There are a fair number of minor factual errors, giving the book the appearance of a "rush" job. Finally, the index, obviously done by the publisher, leaves something to be desired.

These criticisms should in no way detract from the monumental work of Erickson, who remains our foremost scholar of Russian military history. We can be sure that if better military histories of the USSR are written, then Erickson will write them.

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Z. BASIN'SKII *et al.*, editors. *Dokumenty i materialy po istorii sovetsko-pol'skikh otnoshenii*. Volume 7, 1939-1943 gg. Moscow: Izdatel'stvo "Nauka." 1973. Pp. 509.

As might have been expected of an official documentary collection edited jointly by Soviet and Polish historians and dealing with the stormy relationship of the two governments during World War II, the present volume does not tell us much about this relationship that we did not already know. More than two-thirds of the 304 documents printed in the volume have already been published elsewhere, while of the new ones, only a half dozen or so add significantly to our knowledge. (Among the new documents the ratio of Soviet to Polish sources is on the order of six to one.)

If the new collection is compared with that published by the Polish exiles in London, which covers approximately the same period (*Documents on Polish-Soviet Relations*, vol. 1, 1939-43 [London, 1961]), important gaps emerge. Basin'skii and the other editors provide no documents concerning the German-Soviet negotiations pursuant to the fourth partition of Poland in 1939. They permit only indirect reference to the forcible deportation from the newly annexed territories of up to 1.5 million Polish citizens. The repeated inquiries of the London government as to the whereabouts of the 8,300 Polish officers taken prisoner by the Red Army in 1939—namely, questions raised prior to the discovery of approximately 5,000 bodies in the Katyn Forest during April 1943—find no place in this book. Even the only conversation between Sikorski and Stalin (on December 3, 1941) does not appear in this volume, evidently because Sikorski asked about his officers.

Furthermore, the editors regularly suppress evidence of Soviet unwillingness or inability to supply the Polish divisions, formed in the USSR from among refugees and former prisoners, with Soviet standard equipment and rations; of the substantial number of Poles that Anders was not permitted to recruit; and of the persistent efforts of the London government to secure the evacuation of elements of the refugee population. Such lacunae must reflect more accurately the tensions that currently underlie Soviet-Polish relations than they do the events of World War II.

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O. B. BORISOV and B. T. KOLOSKOV. *Soviet-Chinese Relations, 1945-1970*. Edited with an introduction by VLADIMIR PETROV. Bloomington: Indiana University Press. 1975. Pp. xviii, 364. \$12.50.

Writing under pseudonyms in order not to impair their future dealings with the Chinese, two high-

ranking Soviet specialists on China have penned a comprehensive account of Sino-Soviet relations since the occupation of Manchuria by the USSR in the final stages of the Pacific War to 1970 (the book was published in the Soviet Union in 1971), as perceived through Russian eyes. They detail the assistance provided by the USSR to the Chinese Communists (1945-49) and the People's Republic of China (1949-52), the cooperation between the two neighboring powers (1953-59), the gradual deterioration in Sino-Soviet relations in the 1960s, and the anti-Soviet character of the Cultural Revolution. They ascribe the roots of the conflict to Mao's "cult of personality," his opposition to "peaceful coexistence" with the United States, his ingratitude for Russian assistance, and his unwillingness to coordinate his policies with Moscow.

Unlike many American authors who depict the split as a manifestation of historical enmity between Russia and China, the Soviet writers portray it as an aberration, produced by one faction in Peking. In presenting the policy of Moscow in a most benevolent light and blaming Maoist chauvinists for the Sino-Soviet dispute, they add new grist to the mill of controversy. At the same time, however, they stress that the conflict is neither desirable nor natural and that the Soviet Union stands prepared to terminate it.

The frustration that the Russians have experienced in their relations with the Chinese is genuine. It was shared at one time or another by Japanese, English, and American supporters of various Chinese governments. These powers were likewise alienated by the lack of expected gratitude on the part of the Chinese for the aid extended to them. Deep as the rift between Peking and Moscow may be, as recorded in the book and proclaimed by the present Chinese leadership, it would be perilous for American policy makers to count on its indefinite continuation.

The book is an important contribution to the history of Sino-Soviet relations. It was translated by computer and verified by David Chavchavadze. The translation was heavily edited by Vladimir Petrov of the Institute for Sino-Soviet Studies of George Washington University (who also supplied an introductory essay) and polished (with redundancies deleted) by Sally G. Bunting. The result is a tightly knit, readable account, more easily digested than the Russian original.

GEORGE ALEXANDER LENSEN
Florida State University

GEORGE LENCZOWSKI. *Soviet Advances in the Middle East*. (U.S. Interests in the Middle East Series. Foreign Affairs Study 2.) Washington: American Enterprise Institute for Public Policy Research. 1972. Pp. 176. \$4.00.

The Arab defeat in June 1967, followed by the protracted Israeli presence in the Arab lands, brought about a massive deterioration of the United States position in the Arab orbit. With the sharp decline of American political influence in the late 1960s and early 1970s came the loss of economic predominance, particularly in most of the Arab oil-producing states. These developments prompted the American Enterprise Institute to initiate a series of studies on American interests in the Middle East, under the editorship of George Lenczowski. The volume under review, written by Lenczowski, is the seventh in this series. It is devoted to the general theme of the recent progressive Soviet penetration of the Middle East.

After a brief introduction setting forth the doctrinal underpinnings of Soviet foreign policy, the author carefully analyzes the historic Soviet quest for expansion in the key states of the region—Iran, Turkey, Egypt, and Iraq. The chapter on Iran examines briefly the turbulent phase of Soviet-Iranian relations during the 1940s and early 1950s, with particular reference to oil concessions and the Azerbaijan problem. Lenczowski follows this with an account of the shift of Soviet policy from unsuccessful subversion to the establishment of normal economic relationships during the 1960s. The chapter on Turkey contains an excellent review of Soviet-Turkish relations during and after World War I, including the Moscow-Ankara rapprochement and the consequent destruction of the Armenian Republic, the Straits question, and the Kurdish problem. The author analyzes the growing dissonance in Soviet-Turkish relations as these concerned Soviet strategic and territorial demands after World War II. He devotes the final section to Turkey's NATO role, the Cyprus issue, and the normalization of Soviet relations with Turkey.

Lenczowski's most ambitious chapters focus on the expanding Soviet role in Arab lands. He identifies the factors facilitating Soviet penetration: the rise of Arab nationalism, the establishment of Israel, and the Western imperial presence. He describes the methods used to implement it—arms deliveries, cultural offensives, technical assistance, and economic aid. The convergence of Soviet and Arab interests is detailed, focusing on the development of close ties between the USSR and Nasserite, radical regimes espousing pan-Arabism and Arab socialism. The chapter on Egypt contains valuable sections on the role of Egyptian leftist groups and the evolution of Soviet economic and political influence until the early 1970s.

Nowhere in the Arab world was indigenous communism stronger than in Iraq. The author points out that the Iraqi Communist party was unique in attempting to capture power in an Arab country. Coupled with the deeply factionalized

state of Iraqi society, this produced a "checkered pattern" of relations with the Soviet Union. Indeed, so unstable and paranoic had been the Iraqi regime that systematic Soviet attempts to cultivate in Iraq a reliable ally were repeatedly aborted.

The concluding chapters include a detailed estimate of the Soviet military contribution to Arab armed forces and the consequent Soviet strategic "presence" in the area. Despite the general success of Soviet efforts, the author identifies certain obstacles operating in the Arab areas. These include the uncompromising nature of Arab nationalism, which defied Soviet attempts to exercise greater political control, and the deep admiration of most Arabs toward the West and its superior technology. Lenczowski concludes by returning to his main theme that the Arab-Israeli conflict is primarily responsible for the decline of the position of the United States among the Arabs—one filled by the Soviets.

The chief weakness of this competent analysis is that it is dated because of the rapid changes in Middle Eastern politics.

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I. M. MUMINOV *et al.* *Samarkand Tarikhi* [History of Samarkand]. In two volumes. Tashkent: Izdatel'stvo "Uzbekiston SSR FAN Nashrieti." 1971. Pp. 474; 481.

This two-volume work in both Russian- and Uzbek-language editions is a useful addition to the growing body of materials concerning the local and regional histories of the Soviet Union. Samarkand and its vicinity, long an important area of western Central Asia, are presented in a multifaceted manner from earliest times to the present with discussions of social, economic, cultural, and political history. The general theme of the work is the relationship of all the peoples of Samarkand and its environs, regardless of their ethnicity, to a deep local heritage rather than to a broader Islamic or general Turkic-Iranian background. It stresses that this local heritage and potential after many long centuries finally achieved full development in the Soviet period.

The first volume, especially in its treatment of the ancient and medieval periods, places substantial reliance on the works of Barthold and Vambery. These sources are supplemented by materials from native histories and the results of Soviet period archeological work. Both volumes feature lengthy bibliographies of great utility to the student and scholar in the West. The first volume has the sources conveniently divided into those from the Soviet and pre-Soviet periods. The

first volume contains little new information on political developments, but the inclusion of social, cultural, and economic history into the same work adds a new dimension. Emphasis is on the Islamic period, with most attention given to the Timurid era. Of somewhat unique interest are three chapters discussing the development of irrigation systems in this river valley and oasis area during the ancient, medieval, and early modern periods. The second volume, dealing with the Soviet period, emphasizes social and economic themes. It is perhaps the more valuable of the two for the student and scholar, because it draws heavily on local materials, both published and unpublished, that are for the most part unavailable outside the Soviet Union.

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NEAR EAST

J. G. MACQUEEN. *The Hittites and Their Contemporaries in Asia Minor*. (Ancient People and Places, volume 83.) Boulder, Colo.: Westview Press. 1976. Pp. 206. \$18.00.

As volume 83 in the useful Ancient Peoples and Places series, this book presents a nontechnical, up-to-date introduction to the history and culture of the Hittites. Footnotes are sparse, but a select bibliography and sixty-one line drawings of plans and objects provide a measure of documentation. The first third of the book is a sketch of the history of the Hittites and their neighbors in Anatolia during the second half of the second millennium B.C. J. G. Macqueen states his method of historical reconstruction: "Events fit into a general pattern of economic necessity, and the efforts of the various states can be seen to be directed towards something more permanent than the arbitrary ambitions of individual sovereigns" (p. 41). For the author the determining factor in the political history of Anatolia was the presence of large deposits of copper ore (at Ergani Maden, for example), which were carefully secured, exploited, and traded in distant quarters of the ancient world. Since tin was not available in Anatolia, the routes by which it was brought to metalworking centers were jealously guarded. This intriguing thesis lacks only the support of archeological evidence for metalworking installations and slag heaps. One wishes for more detailed documentation for this far-reaching interpretation of the course of political events in Anatolian history than has been possible within the limits of a volume of this series.

The latter two-thirds of the book deal with topics of cultural interest: daily life, warfare and

defense, society, religion, art, and literature. This section follows the rubric of the synthesis made by Götze in 1933, but it fills in the picture by using the wealth of new material that has appeared since. Macqueen summarizes his story of the Hittites as follows: "Their rediscovery has revealed to us a people who, if they did not possess the genius and originality to change the course of world history, at least showed a talent for political and military organization, and a capacity to utilize their resources, which enabled them to gain, and retain for several hundred years, a leading position in the Middle Eastern world" (p. 150). Although work in progress may change details in the story, it is not likely to alter materially this appraisal.

JAMES B. PRITCHARD
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LEWIS V. THOMAS. *A Study of Naima*. Edited by NORMAN ITZKOWITZ. (New York University Studies in Near Eastern Civilization, number 4.) New York: New York University Press. 1972. Pp. xii, 163. \$12.50.

Mustafa Naima was an official historian of the Ottoman Empire. His history, composed between 1697 and 1704, covers events during the reigns of eight sultans (Ibrahim is curiously omitted from the list on page 128) from A.H. 1000 (1591-92) to A.H. 1070 (1659-60). Thomas' study, based on Naima's history, is also a study of Naima himself. In a broader sense, it is a study of various Ottoman institutions and attitudes of the seventeenth and eighteenth centuries.

One will not find here a précis of Naima's chronicle or a critique of its adequacy or accuracy as history. Instead, Thomas has written about Naima's life, about his ideas and attitudes, especially as expressed in the two prefaces of his work, and about his use of sources and his critical methods. The whole book is in fact a sort of Ottoman commentary on aspects of Ottoman life and thought. The fact that Naima was born in Aleppo of a Janissary father leads Thomas, for example, to introduce sections of explanation on Aleppo officialdom and its relations with Arab tribes and on Janissary units in a provincial station. His study of Naima's prefaces leads Thomas to comment on Naima's ideas on the virtues of a strong ruler, on advice to viziers, on factions, and so forth. The commentary is often brilliant. The text and notes are crammed with information, including occasional translated passages from Naima's text.

Lewis Thomas, before his untimely death in 1965, was America's leading teacher of Ottoman history. This study was his doctoral dissertation. A former student, Norman Itzkowitz, is the editor.

It is good to have the study, already influential in manuscript, finally in print.

RODERIC H. DAVISON
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ILKAY SUNAR. *State and Society in the Politics of Turkey's Development*. (Ankara University Faculty of Political Science Publication, number 377.) Ankara: Ankara Üniversitesi Basımevi. 1974. Pp. ix, 196.

This stimulating Berkeley dissertation examines the linkage between the traditional Ottoman political structure, the state, and the emergence of a new entity, the civil society, during the last days of empire.

In the first chapter İlkey Sunar points to the unique nature of the Ottoman Empire and the inappropriateness of likening it to medieval Europe, insofar as the former was based on patrimonial rule and the latter on a clear-cut distinction between state and society inherent in the feudal order.

Chapter 2, in the discussion of the political and economic state of the Ottoman Empire, offers the reader an intelligent analysis of some of the deleterious forces at work that militated against the economic development of the empire; for example, the close integration of the empire with Europe's economic and political structures; the patrimonial nature of the empire resulting in a regulated rather than a free economy; the tensions between the Ottoman state and nascent society "created" by bureaucrats; the emergence of patronage as a mechanism for economic development as an adjunct to the patrimonial order; and so forth. For the historian, this detailed chapter is the most rewarding. The third chapter is devoted to political events since 1923; as such, it covers familiar ground.

Of great interest to the author is the role of the middle class in the Republic of Turkey, a topic that occupies the lengthy fourth chapter. To be sure, Sunar argues, there has been economic development since 1923, but it has been lopsided, limited, and of a questionable nature owing to the differential distribution of the fruits of this development.

Analysis of events in Turkey since its founding as a republic leads the author to conclude in his final chapter that what is now taking place in Turkey is a "politics of mobilization," that is, the urban proletariat and the peasantry have made an effort to change the situation so that they, too, will be the beneficiaries of economic growth.

This well-written and well-researched monograph is full of stimulating insights and observations. It is hoped that Sunar will, however, further

develop some of his seminal theses and theories more fully in later publications.

This work successfully integrates the disciplines of history, sociology, political science, economics, and public administration in forcefully expounding the author's perspectives. Historians might wish that Sunar had devoted a larger portion of his study to history. However, this is a minor criticism of an otherwise outstanding work.

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DICKRAN H. BOYAJIAN. *Armenia: The Case for a Forgotten Genocide*. Westwood, N. J.: Educational Book Crafters. 1972. Pp. xiii, 498. \$15.00.

The focus of this volume is the extermination of over half of the Armenian people by Ottoman Turkey during World War I. As a volunteer in the French Army fighting against the Turks, the author witnessed the final phase of the deportations and atrocities that befell the Armenians. Yet his book is not an eye-witness account; rather, it is a historical-legal brief presenting the Armenian case in quest of justice. Indeed, Boyajian's methodology is that of a trained lawyer—which he is—rather than the craft of a historian. He is basically motivated by an Armenian's deep sense of outrage not only against Turkey but against the world for having forgotten the plight of his people. Six decades after the loss of two million souls, the Armenians remain uncompensated materially or territorially despite the promises made by the Allies during and after the Great War. Thus the author's stated purpose is dual: to awaken the conscience of the world and to present the historic case for the establishment of an "Armenian homeland" similar to the Jewish state of Israel.

The first chapter documents the governmental planning behind the massacres by presenting the translations of several official telegraphic communications that contain specific orders of deportation and extermination. In chapter 2 the author details the progressive decline of the Armenians' position in the Ottoman Empire, including the massacres of 1895-96 and 1909 as seen through the eyes of American and European missionaries, diplomatic officials, and travelers. This is followed by the weakest part of the book—an account of 3,000 years of Armenian history. Next, the story of the massacres of 1915 is presented in some detail by drawing from official German, French, British, and American sources. Significantly, few Germans, except Count Wolff-Metternich, attempted to stop the Armenian massacres, despite their pervasive military presence in the territory of their Ottoman ally. Ironically, it was a Jewish-American, Ambassador

Morgenthau, who unsuccessfully pressed the Young Turkish leaders to stop the atrocities.

The remaining portions of the book deal with the aftermath of the holocaust, including the desperate efforts of the surviving Armenians to set up a homeland under an American mandate and the failure of the Allies to help sustain the Armenian Republic, which eventually collapsed in 1920 in the face of the developing Soviet-Turkish rapprochement. A massive appendix—consisting of additional documentation on the massacres, statements by world leaders concerning the Armenian question, as well as relevant treaties, conventions, and maps—ends the volume.

Clearly, Boyajian's study is the result of a prodigious amount of work, and despite its unorthodox, legalistic format it represents a strong validation of Armenia's tragedy. His use of the Jewish holocaust as a point of departure is enlightening and apparently sound, although a comprehensive analysis of comparative genocide has not yet been attempted. Nor can Boyajian's volume be considered the definitive work on the Armenian massacres; indeed, such a major effort cannot be undertaken as long as the relevant sections of the Ottoman Archives of Istanbul remain closed to foreign scholars.

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HARRY N. HOWARD. *Turkey, the Straits and U.S. Policy*. Baltimore: Johns Hopkins University Press. Published in cooperation with the Middle East Institute. 1975. Pp. xii, 337. \$14.50.

Almost half a century ago Harry N. Howard began to write about Turkish-American relations. While pursuing careers in the State Department and as a university professor he published an impressive series of articles and monographs. More than half of this new book deals with the period before World War II when the United States was politically disinterested in the Middle East, except for the World War I interlude. Howard's account, spanning the period from the first Turkish-American treaty in 1830 until 1939, is useful, especially for the detail it provides on the Straits Conventions of Lausanne (1923) and Montreux (1936). But Howard and others have already covered this ground well.

The chapters dealing with the Straits issue since 1939 are more important. Howard's long account of World War II is more than an examination of the Straits question; it provides interesting detail on Turkey's nonbelligerent role, as well as Grand Alliance discussions on possible Turkish entrance into the conflict. Cold war historians will find Howard's analysis of Stalin's role interesting;

while officially favoring Turkey's active participation in the war, Stalin was much more interested in the establishment of a second front and possible Turkish concessions to the Soviet Union in the Straits and elsewhere. These objectives would be more difficult to pursue after the war if Turkey assumed belligerent status.

Howard makes clear why a Soviet-Turkish crisis after the war was almost inevitable. Pursuing its historic objectives in the Straits area from a more powerful base, the Soviet Union in 1945 and 1946 demanded substantial revision of the Montreux Convention—to include Soviet bases in the area and joint Turkish-Russian administration of the strategic waterway. The United States and England backed Turkey's nervous but resolute determination to withstand Soviet pressure. As Howard points out, Truman chose to get tough on this issue. Truman told Secretary of State James Byrnes that "unless Russia is faced with an iron fist and strong language another war is in the making." The promulgation of the Truman Doctrine in 1947 further illustrated the president's commitment to Turkey.

This study is effectively documented with original and secondary materials. A useful appendix provides texts of several key documents, comparison of various Straits conventions, and statistical tables on commercial shipping in the Straits since World War I. An annotated bibliography is also provided. Howard's important work is a fitting climax to the career of a pioneer scholar of United States-Middle Eastern relations. Another book on the subject of the Straits will be unnecessary for many years.

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R. J. GAVIN. *Aden under British Rule, 1839-1967*. New York: Barnes and Noble. 1975. Pp. x, 472. \$35.00.

LAWRENCE R. PRATT. *East of Malta, West of Suez: Britain's Mediterranean Crisis, 1936-1939*. (Published in cooperation with the London School of Economics and Political Science.) New York: Cambridge University Press. 1975. Pp. xiii, 215. \$17.95.

In his introduction to *The View From Steamer Point* (1964) Charles Johnston stated that R. J. Gavin's full-length study of the British in Aden would soon be in print. Now, eleven years later, *Aden under British Rule, 1839-1967* appears, the first work detailing British presence in Aden and the hinterland—the Yemen and the Hahdramawt—during 128 years of British rule. At present the capital and main port of the People's Republic of Southern Yemen, Aden was until 1967 the most important part of England's life line to India. This study

describes British policy-makers' activities pertaining to Aden and their relations with sultans, sheiks, emirs—friends, allies, and enemies—and it describes an evolving economy in what was first a refueling port and then a free port and center for a land and sea-borne trade, which included coffee, gums, hides, skins, and incense from the shores of South Arabia and the Red Sea. The latter part of the book traces the development of Aden's modern industrial society, including treatment of the formation of trade unions; the growth of educational, medical, and social welfare institutions; for formation of the Aden Federation; the revolution in Yemen and the nationalist struggle to take over Aden; and the ultimate British withdrawal.

Gavin explains Aden's external affairs as they related to Egypt. With the opening of the Suez Canal in 1869, however, foreign affairs begin to be slighted in the text, but at times Italian aspirations and "meddlings" come to the fore. An important segment of the latter, with its emphasis on the Mediterranean, can be read in Lawrence R. Pratt's *East of Malta, West of Suez*, an analytical and interpretive study. Its theme is an explanation of the crisis involving Mussolini that came at the time of the Italian Abyssinian invasion and enveloped an overextended empire, insufficient revenue, and inability to defend the empire. The crisis was magnified by the weakness of the League of Nations, Hitler's machinations, the Spanish Civil War, French insecurity, the lack of policy in American foreign affairs, and the inability of British statesmen to agree on how to contain Mussolini because of his unpredictability or "mad-dog tactics." Since policy centering on the Mediterranean cannot be separated from Europe, the Balkans, or even the Far East, Pratt delineates a second theme, appeasement, and exonerates Hoare in the original negotiations with Laval and Chamberlain, the two scapegoats for most statesmen. Gavin suggests that appeasement provided valuable time for reinforcement of defenses at Singapore, for building additional ships, and for rearmament at home.

Based on extensive primary sources, Gavin's and Pratt's studies show competency and clarity of thought. Gavin, in his bibliography, misspells Charles Johnston's name, and he makes no mention of Johnston's participation in the Aden Federation. Pratt superficially places his topic in its historical setting and pays too much attention to appeasement. Both authors present excellent psychological portraits of the men involved, discussing their dealings, shortcomings, and loyalties. The importance of the Mediterranean, the Red Sea, and Aden to Britain and the empire is stressed. In his conclusion Gavin explains how the British hurriedly left Aden, thus symbolically ending the final chapter on the British life line to India. Pratt

also indicates in his work that English indecision in the Mediterranean from 1936 to 1939 only foretold the reality of the situation.

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BEN ZION WACHOLDER. *Eupolemus: A Study of Judaeo-Greek Literature*. (Monographs of the Hebrew Union College, number 3.) Cincinnati: Hebrew Union College Press, for the Jewish Institute of Religion; distrib. by Ktav Publishing House, New York. 1974. Pp. xi, 332. \$15.00.

Eupolemus is reputed to be the first Graeco-Jewish historian known to us. There are a number of problems, however, attendant to this second-century B.C. diplomat and member of a Jerusalemite priestly family. His work survives only in fragments preserved in the writings of the Church fathers, Eusebius and Clement of Alexandria, whose source was Alexander Polyhistor. Complicating the matter is that part of Eupolemus' history is said by some, including Ben Zion Wacholder, to have been written by a historian they call Pseudo-Eupolemus, identified as a Samaritan.

The six surviving fragments of Eupolemus' history cover the period from Moses to his own day. Wacholder sees Eupolemus' history as a continuation of the work of the Biblical Chronicler who allowed himself complete freedom in elaborating Jewish history to indicate the great value of Judaism and to enhance the position of certain Jewish heroes. Thus, in the first fragment we read that "Moses was the first wise man, the first who imparted the alphabet to the Jews; the Phoenicians received it from the Jews, and the Greeks from the Phoenicians; also laws were first written by Moses for the Jews."

Eupolemus' goal apparently was to promote Judaism for those Jews who were profoundly influenced by Greek culture by indicating that Jewish culture was the oldest in the world and that it was Jews who really taught others, rather than vice versa. Somewhat later, Philo of Alexandria attempted to do the same by noting that Plato learned from the Bible and the teachings of Moses. What we have then, Wacholder concludes, is an essentially new history of the Jews, fusing biblical with Hellenistic historiography, in response to the needs of a generation torn between the attractions of Hellenism and their loyalty to Judaism.

While much of Wacholder's analysis is influenced by the work of J. Frudenthal, his major contributions are the detailed linguistic analysis of the fragments and the use he makes of them in exploring the nature of Graeco-Jewish literature which flourished in Palestine in the second century B.C. This literary tradition, seen as following the

style of the Book of Chronicles, continued in the histories of Josephus and Justus of Tiberias. This is an impressive study of problematic material, offering important insights into a period of great cultural flux.

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EDGAR O'BALLANCE. *Arab Guerilla Power, 1967-1972*. Hamden, Conn.: Archon Books. 1973. Pp. 246. \$10.50.

That old phenomenon, irregular warfare, and the much more recent, but still old, word "guerilla" [sic] seem to have taken on a strange fascination in recent years. Many appear to believe that the phenomenon itself did not occur before the emergence of recent theories of "people's war." Palestinian Arabs have engaged in some fairly impressive acts of this kind, in 1936-39 especially, and in 1950-56; but only the post-1967 actions have attracted much attention. O'Ballance follows the majority. His theme is the adoption of Maoist theory by the Palestinians and their fortunes in its application. The book relates how a variety of underground Palestinian organizations, most notably al-Fath (O'Ballance uses the more common "Fatah") were formed after 1956 and gained popularity and strength after 1967 through their terrorist acts in Israel and through the appeal of their doctrines. The Palestinian guerrillas soon found themselves in collision with the governments of Lebanon and Jordan, which opposed them because their activities stimulated Israeli retaliations and, in the case of Jordan, because the Palestinians tended toward internal revolution. The result was the bitter war between the Palestinians and the Jordanian monarchy in 1970-71. Following King Hussein's victory, the guerrillas entered a period of decline. The net result was the failure of the Palestinians "to carry out revolutionary guerilla warfare on the Algerian and Viet Cong patterns inside the Israeli occupied territories" (p. 228).

The book is based on Western public sources (and documentation is sparse), but this limitation is one shared by all who seek to study the military side of this topic. More serious is O'Ballance's unfamiliarity with Arab history and politics. The occasional shots at earlier periods miss the mark, and the account of the political side of the Palestinian movement suffers from a few errors and some confusion. The author incorrectly explains "Fatah" and does not give a reliable account of the Palestinian organizations. (He uses two English names for a single organization.) Finally, he fails to make a systematic analysis of the available data relative to the effectiveness of Palestinian guerrilla activities. In these respects his work is inferior to

The Politics of Palestinian Nationalism by William B. Quandt, Fuad Jabber, and Ann Mosely Lesch (1973). But O'Ballance's book meets the need for a detailed, coherent narrative of Palestinian military activities, a narrative constructed by fair-minded and skillful use of the available sources.

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EDGAR O'BALLANCE. *The Electronic War in the Middle East, 1968-70*. [Hamden, Conn.:] Archon Books. 1974. Pp. 148. \$8.00.

Edgar O'Ballance discusses what he contends was history's first all-out electronic war and in the process assists the reader in developing a more accurate perspective regarding the events which led up to that conflict. Of particular significance is O'Ballance's outline of the role of the superpowers vis-à-vis their client states—those that conducted the hostilities as the United States and Soviet Union supplied necessary equipment and key manpower.

In terms of military affairs, O'Ballance rightly points out that the "electronic summer" of 1970 changed the pattern of future warfare in the Arab-Israeli conflict. The role of sophisticated air defenses, electronic countermeasures (ECM), and even electronic counter-countermeasures (ECCM) was to have a powerful impact on subsequent military preparations by the two warring sides. The author highlights the fact that the advent of such weaponry increased the need for more specialized technical training. Thus, while the number of available personnel may have become less important, the quality of manpower has become a paramount concern.

One particularly important message is implicit throughout the book—the folly of assuming that if one side becomes militarily superior, this imbalance of power somehow will prevent warfare from erupting. Such a situation usually results in a change of tactics, as in the case of Cairo's reaction to the destruction of its air force by Tel Aviv in the massive Israeli pre-emptive strike in June 1967. Rendered defenseless against deep-penetration raids, the Egyptians turned to the Soviet Union for an effective air defense system. The United States then attempted to assist its client state (Israel) to maintain superiority by providing it with highly sophisticated ECM to counter Egypt's Russian-built and Russian-manned defenses. And the cycle continued.

The author aptly concludes his study by stating that "electronics in warfare have come to stay and their degree of involvement will only be limited by the wealth, resources and technical capability of

the country concerned, or its ability, like that of Egypt and Israel, to persuade larger nations to supply them with military means."

Although O'Ballance wrote the book prior to the outbreak of hostilities in October 1973, his assessments for the future were not proven inaccurate by the war, and his book should be considered required reading for those interested in military history, the Arab-Israeli conflict, or arms races.

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AFRICA

CHARLES WENDELL. *The Evolution of the Egyptian National Image: From Its Origins to Ahmad Lutfi al-Sayyid*. (Published under the auspices of the Near Eastern Center, University of California, Los Angeles.) Berkeley and Los Angeles: University of California Press. 1972. Pp. xvii, 329. \$17.50.

This study traces the historical evolution of the nationalist idea in Arab political life from its inception in the Umma Muhammadiyya, or the international community of believers, through its Islamic reformist phase. It ends with the ideas of secular nationalism as expounded in the journalistic writings of the liberal Egyptian thinker, Ahmad Lutfi al-Sayyid (1872-1963).

Charles Wendell's erudite exposition and lively narrative convincingly demonstrate the intellectual inadequacies of traditionally based and religiously inspired political belief systems that gradually emerged in the eighteenth and nineteenth centuries as indigenous responses to the crushing dominance of European secularism. Wendell analyzes the historical process of challenge and change in the specific context of the Egyptian experience, viewing it simply as a microcosm of an intellectual phenomenon affecting the whole of the Arab East. From the period of Napoleon's invasion of Egypt in 1798 until the eve of World War I the focus of Arab nationalist aspirations successively narrows from the universalist Islamic state to the European-imposed, self-contained, and autonomous nation-state.

The clearest and most eloquent definition and defense of a liberal, rationalist view of the Egyptian *umma* or nation-state is nowhere better articulated than in the writings of Lutfi al-Sayyid. Although neither an original thinker nor a philosopher, among a generation of educated Egyptians he was known as "failasuf al-jil," philosopher of the generation. His contribution remains in the manner in which he expressed European ideas and sought their incorporation into a uniquely defined Egyptian nationalist image that rejected purely pan-Ottoman, pan-Arab, and pan-

Islamic political formulations. Indeed, what distinguished Lutfi from his intellectual predecessors was his advocacy of European legal and political concepts of citizenship contained in a territorially defined nation-state.

For Wendell, Lutfi's liberalism, while genuine, was misplaced and overly idealistic. Moreover, it borrowed too heavily from Europe ever to achieve lasting acceptance among a tradition-bound and profoundly Islamic mass public. European-inspired ideas of liberal, secular nationalism were thus doomed to inevitable extinction in an area where religious zeal and political extremism historically prevailed. Clearly the postrevolutionary period of Nasserite rule would give support to such a view. However, Sadat's Egypt appears to be invoking the spirit, if not the letter, of many of the liberal nationalist ideas that Lutfi so energetically put forth in the pages of *Al-Jarida*, which he edited from 1907 to 1914. If in fact this trend continues, it may confirm and give contemporary relevance to Lutfi's essential belief in the distinctive Egyptian view of its political self—a free, liberal spirit, culturally aligned to the West, but respecting its twin inheritances from the Arab and Pharaonic pasts.

In conjunction with the earlier pathbreaking studies of Jamal Mohammed Ahmed, Nadav Saffran, and Albert Hourani, Wendell's meticulously researched and balanced presentation provides another splendid addition to the growing literature in English on the intellectual geneses of Arab and Egyptian political and social thought in the modern period.

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ALEXANDER SCHÜLCH. *Ägypten den Ägyptern! Die politische und gesellschaftliche Krise der Jahre 1878-1882 in Ägypten*. Foreword by ALBERT HOURANI. (Beiträge zur Kolonial- und Überseegeschichte, number 9.) [Zurich:] Atlantis. 1972. Pp. 396. 48 S. fr.

"Egypt to the Egyptians!" This, the Monroe Doctrine in Egypt, was the slogan of a movement there that preceded and led to, but was then strengthened by, the British invasion of that country in July 1882. Yet, the 'Urābī movement, named for its most influential promoter and spokesman, had neither narrowly defined chauvinistic nor racist goals. It did not intend to question the authority of the sultan in Istanbul, who was still the nominal Egyptian sovereign, nor to establish an Egyptian national state, nor even to overthrow the Egyptian khedive, a fact the khedive himself did not realize. Rather, the movement's first objective was the abolition of European financial and political control over Egypt, which was the result of Egypt's £91 million indebtedness in 1876. It also aimed at

the abolition of privileges, especially in the army, of the old Turko-Circassian ruling class, which had regained much terrain since Muhammad 'Alī's blow against it in 1811. When the English army occupied Egypt, it did so for strategic and financial interests and also for the khedive, who was reinstated. That there were influential Egyptian personalities on the invaders' side shows that the 'Urābī movement, in its fight against the foreign aggressors, was not able to unite the leading groups of Egypt.

Alexander Schölch describes and analyzes the Egyptian political and social crisis of 1878–82 as well as the development of the 'Urābī movement. Schölch spent years working in European and Near Eastern libraries and archives before presenting this study for his doctoral dissertation at Heidelberg University in 1972. He introduces his work with an analysis of Egyptian society, emphasizing the leading regional and national group under Ismā'īl (1863–79). He then describes the overthrow of the social order by means of foreign intervention, the new politically influential groups after the overthrow, and, finally, the termination of change in Egypt caused by the English occupation.

Schölch's exciting and readable book offers a magnificent case study of nineteenth-century European imperialism: growing indebtedness, for whatever reasons, "necessitated" foreign financial control. It was the supervisors' evaluation of the political and social changes that finally caused military intervention. The people who profited most in the end from this development were the European merchants and bankers.

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TADEUSZ LEWICKI, with the assistance of MARION JOHNSON. *West African Food in the Middle Ages, According to Arabic Sources*. New York: Cambridge University Press. 1974. Pp. xv, 262. \$23.50.

This, the English version of a study that appeared in Polish in 1963, is a welcome addition to the literature. What we know of the precolonial economic history of the forty-odd tropical African countries has heretofore been limited to the last four and a half centuries. With evidence from little-used Arabic sources, Lewicki pushes back what we know about West African economic history another five centuries.

Although the scholarship is generally solid, the book changes very little our understanding of West African economic history. For the most part it merely confirms earlier speculations, and, even so, it is limited in geographical coverage largely to the Sahara Desert and its southern fringes. We still

know little about economic change in the forest and savanna belts of West Africa—now by far the most important areas in terms of economic activities—prior to the nineteenth century.

The main contribution of the book, then, is that it confirms what we would have expected from previous evidence. We now have confirmation that the millets and sorghums, which today account for over eighty percent of all calories consumed in the desert fringe portion of the area studied, were also the dominant staples at least a thousand years ago, and that persons living in the area were familiar with a majority of the less-important crops now found there for at least five hundred years.

Unfortunately, the study tells us little about other aspects of the economy. There is one reference to use of irrigation for producing wheat in the eleventh century (p. 39), but otherwise almost no information on techniques of production, the organization of labor, marketing, or transport systems. Nevertheless, it is good to have the Arabic sources finally searched and the findings made available in one of the world's major languages.

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JOHN D. HARGREAVES. *West Africa Partitioned*. Volume 1, *The Loaded Pause, 1885–1889*. Madison: University of Wisconsin Press. 1975. Pp. xiv, 273. \$17.50.

In what is explicitly the first of three volumes, John D. Hargreaves gives us a detailed historical narrative covering five crucial years of French, British, and German involvement in West Africa. He addresses the "dramatically sudden transition" to a European imperial role in the area from Senegal to Cameroun. Hargreaves provides some answers to the question of why this transition took place "despite the absence of sustained or broad-based pressures for the establishment of empire in Africa."

Readers of Hargreaves' *Prelude to Partition* (1963) will expect detailed scholarship, narrative control, and generalizations framed with knowledge of wider theoretical controversy. They will not be disappointed with this volume, although Hargreaves leaves the ideological and bibliographical dimensions of imperialism in West Africa for the third volume of the trilogy. Volume 1 opens with a summary of European-African relations in 1885. It proceeds with discussions of French inhibitions and initiatives concerning Samori, the Senegambia, and the Kong; the anomalies of the regime set up in the Oil Rivers region by the Foreign Office; the complexities involving Yoruba, Dahomeans, the Aja, and Lagosians on the Slave Coast; and the Sierra Leone hinterland. It ends with a recapitula-

tion of the 1885–89 period. Hargreaves notes particularly the confusion of purpose among metropolitan groups and the great diversity of interests of African participants.

In concluding, the author suggests that the increasing European involvement of these years was not brought about by metropolitan plan. There was not then an imperial design. In each case "some effective driving force can be identified within the frontiers of existing colonial societies." Practitioners became spokesmen; merchants, missionaries, local officials, and African leaders sought increased contact and wider opportunities. The "inner logic of frontier relationships" extended European commitments and made governments wary of the others' "agents." The desire to forestall future, mostly illusory, economic benefit that would fall to others had some persuasive effect, but in essence it was local pull, not imperial push, that brought Europeans and Africans into inextricable relationships. Even by 1889 Europeans did not think of political dominion as the only appropriate solution for West African problems.

Hargreaves has set the stage. We await with high expectations the curtain's rise on African and European decisions during the final decade of the nineteenth century.

PROSSER GIFFORD
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PHILIP D. CURTIN. *Economic Change in Precolonial Africa*. Volume 1, *Senegambia in the Era of the Slave Trade*; volume 2, *Supplementary Evidence*. Madison: University of Wisconsin Press. 1975. Pp. xxix, 363; xi, 150. \$15.00, \$15.00.

Philip D. Curtin refers to this two-volume study as "an experiment in a kind of history sometimes called interdisciplinary" (p. xix). The result of the experiment is a seminal work of historical, economic anthropology. Using theories and concepts borrowed from anthropology, economics, geography, and sociology, Curtin reconstructs the process of economic historical change in a West African region known as Senegambia from approximately the middle of the sixteenth century to about 1850. If the book is an experiment in historical methodology, it also is a study of how a fringe economy of the Western world functioned before it fell under colonial domination. Curtin's conclusions challenge many sacred beliefs concerning the function of "primitive" economies. His model of articulating and changing economies should replace Karl Polanyi's earlier substantivist model published in *Dahomey and the Slave Trade: An Analysis of an Archaic Economy* (1966).

Curtin reconstructs his data on two levels, which provide new perspectives. On one level he views

Senegambian historical economic phenomena from a universal perspective, relating events to "the process of change in human society generally"; on the other he reconstructs history from a precolonial Senegambian orientation. Accordingly, Curtin eschews the Scylla of "a perspective . . . dictated from America," including its ethnocentrism, and the Charybdis of focusing on great men, great ideas, and great events. Such treatment makes this not a study of "savage Africa," "static Africa," "primitive Africa," or a "waiting-to-be-civilized-Africa," but an imaginative recovery of the broader patterns of precolonial Senegambian life, which emphasizes economic and commercial pursuits, including the Atlantic slave trade.

Curtin divides the subject into eight chapters forming a continuous whole. The first chapter delineates regional political perspectives, the next three deal with Senegambian and European trade. Here the treatment is refreshing, for instead of the traditional historian's one-dimensional study of "European trade on the West African coast," two-dimensional, interdependent, Afro-European trading diasporas emerge with interlocking points at coastal and interior commercial enclaves. Curtin justifies his Senegambian perspective by arguing that without West African trade diasporas of itinerant and sedentary merchants, who established and maintained interior trade routes and networks, the commercial scope of corresponding European trading diasporas, including that of the Atlantic slave trade, would have been severely limited.

The African-oriented fifth, sixth, and seventh chapters provide accounts of historical change in precolonial Senegambian economies. Curtin discusses raw materials and commodity production for internal and external market exchanges as well as currencies, prices, price fluctuations, and the introduction of new currencies in the mid-eighteenth century. Here he challenges the myth that precolonial African resource exchange was effected through barter. After discussing existing concepts of "primitive" and "modern" money, he concludes that "anything can be money as long as the people who use it accept it as a recognized medium of exchange and standard of value" (p. 235). Finally, he examines the process of competition—yes, competition—in indigenous West African trade.

Curtin loves his work, and it shows. Some, like Lamin Sanneh ("The Origins of Clericalism in West African Islam," *Journal of African History*, 17 [1976]: 49–72), criticize—perhaps with justification—Curtin's "liberal" interpretations of data. However, when faced with a seminal study that generates a new model for inquiries into precolonial African economics while justifying an interdisciplinary methodology, one must cast aside

chiding and scolding and express appreciation to the author.

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DAVID ROBINSON. *Chiefs and Clerics: Abdul Bokar Kan and Futa Toro, 1853-1891*. (Oxford Studies in African Affairs.) New York: Oxford University Press. 1975. Pp. xi, 239. \$19.50.

In the 1860s as Futa Toro—a section of the middle valley of the Senegal River 240 miles long and 10 miles wide—disintegrated into political chaos, a young war leader named Abdul Bokar Kan began a bid for political power at a provincial level. For the next thirty years his career was bounded by the three major realities of Futa political life: the ambitions of local Futa leaders and their neighbors, the manifestations of Islamic reform, and the expansion of French interests in the Senegambia.

David Robinson's purpose in this book is to challenge the assumption that Futa Toro was a "passive receptacle of external initiatives" caught between two expansive empires. In seeking to show how Abdul Bokar Kan established a measure of autonomy, the author demonstrates a range of complex interactions between Europeans and Africans during the late nineteenth century.

Chiefs and Clerics is strongest in its chronology of the politics of Senegal's narrow central valley during these forty years. It is a meticulous account of who joined whom and who fought where as the slow growth of the colonial bacillus progressively destroyed the options open to Futa's inhabitants. It is a thoroughly documented account, and the footnotes often provide reading as vivid as the well-written, crisply directed text.

In Abdul Bokar Kan, David Robinson has picked a protagonist largely ignored in earlier histories of the Senegambia. A provincial warrior who was never able to solve the essential problems of the region or to create new institutions, Kan survived by playing a lone hand in the turbulent precolonial period. He is a difficult subject to portray from the often enigmatic oral traditions and colonial reports. Had he been a stronger personality, Robinson's task would have been easier. He might have been able to draw the book more fully out of the realm of local history had he developed an analytic theme. One could wish, as well, for a discussion of the social climate and institutions out of which this historical chronology emerged and within which Abdul Bokar Kan acted out his drama.

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JOHN GRACE. *Domestic Slavery in West Africa: With Particular Reference to the Sierra Leone Protectorate,*

1896-1927. New York: Barnes and Noble. 1975. Pp. 294. \$23.50.

Domestic slavery was not abolished in Sierra Leone until 1926. It is ironic that the colony founded as a base in the crusade against the slave trade was the last place in the British empire where slavery was legal. John Grace's book is a study of the reasons for this bizarre epilogue to Britain's abolitionist crusade.

The intellectual basis for British reluctance to abolish slavery in Sierra Leone was what Grace calls "conservationism"—the argument that it was either immoral or unwise for a colonial power to tamper with the customs of the colonized. Conservationism was generally a justification for avoiding socially disruptive policies that would raise the cost of administration. Grace argues that two problems lay behind the failure to abolish: the need to shore up the power of the chiefs, who were major slaveholders, and the fear that abolition would lead to a decline in agricultural production. When abolition finally came, it was forced from London because Britain feared embarrassment in the League of Nations.

Though generally well researched and well argued, Grace's book has several shortcomings. The most important is that the author does not seem to have done any significant interviewing of former slaves or slaveholders in Sierra Leone. This results in a failure to get behind the façade of law and official rhetoric into concrete social and economic structures—in particular, the slave's position in the household. Grace does not analyze in any detail the slave's labor obligations, his rights to land, or his rights to his offspring. Having failed to study household relations, he does not recognize changes in social relationships that undoubtedly occurred.

Grace echoes the "conservationists" in telling us that African domestic slavery was not a harsh institution, but this oft-stated remark seems to be in contradiction to one of the major themes of his book, the constant problems caused by runaway slaves. Sierra Leonean slavery may not have been as harsh as slavery in the fields of Jamaica or the Attic silver mines, but the slaves do not seem to have liked it. Yet, when abolition finally came, most slaves stayed where they were. Why? Had changes already taken place or did abolition force changes that made their social position more acceptable? Or did they have few options? This is social history and these are the questions Grace does not answer.

MARTIN A. KLEIN
University of Toronto

S. O. BIOBAKU, editor. *Sources of Yoruba History*. (Oxford Studies in African Affairs.) New York: Oxford University Press. 1973. Pp. vi, 268. \$11.25.

The Yoruba of southwestern Nigeria have been better served by historians than any other ethnic group in tropical Africa. They have been the object of a historical research scheme of which this volume is a by-product.

The reconstruction of the precolonial history of most tropical African societies depends largely, sometimes wholly, on nondocumentary sources, so that a volume which sets out to assess "the various categories of source material available for the reconstruction of Yoruba history" should prove useful to those interested in precolonial African history. Unfortunately, the treatment here of the various sources—traditional history, oral literature, ceremonies, archeology, art, language, sociopolitical structure, warfare and weapons—is variable. Some contributors, perhaps because of lack of editorial directive, have concerned themselves more with a description of the sources than with what these can tell the historian. This is particularly true of the otherwise excellent studies by Robert Smith, "Yoruba Warfare and Weapons," and Dennis Williams, "Art in Metal." This latter consequently compares unfavorably with Carroll's companion piece, "Art in Wood," that addresses itself directly to the historical evidence provided by Yoruba carving.

Law's chapters on contemporary written sources and traditional history are masterly studies of the problems inherent in using both types of sources. He argues cogently that written sources are not necessarily more reliable than oral sources, a point that is conventionally accepted, but rarely as effectively illustrated.

Willett competently describes the service archeology has rendered Yoruba history, while Peter Lloyd examines what historians can learn from comparisons of the widely differing sociopolitical structures found in Yorubaland.

Of the three chapters covering the prolific oral literature of the Yoruba, only Delano's gives a really satisfactory treatment of it as a historical source. Adetugbo's admirable essay on language shows how its study can help steer the historian through the technicalities of linguistics to an understanding of the limitations and potentialities of this source.

Ade Obayemi, a Yoruba historian-cum-archeologist, has recently provided an outstanding demonstration in the volume edited by J. F. Ade Ajayi and Michael Crowder (*History of West Africa* [1976]) of how the various sources covered by this volume can successfully be combined to reconstruct early Yoruba history. It is sad, therefore, that one of the Yoruba historians associated with the Yoruba Historical Research Scheme—the editor himself, or I. A. Akinjogbin—did not conclude the present volume with a chapter on the problems

the historian faces in using sources from disciplines in which he has no formal training.

MICHAEL CROWDER
University of Lagos

PATRICK COLE. *Modern and Traditional Elites in the Politics of Lagos*. (African Studies Series, number 12.) New York: Cambridge University Press. 1975. Pp. xi, 297. \$17.50.

Precolonial elites in Africa did not disappear under the weight of alien rule; they did not become mere pawns of the Europeans, nor were they driven by atavistic impulse when they opposed them. Rather, they continued to calculate and to pursue their economic and political interests, adjusting to the new context of the colonial state. Other elites, created by colonialism and content to work with the imperialist as long as they gained power and wealth from the skills they learned, challenged traditional chiefs and kings at the beginning of British rule. European attempts in the 1890s to seize land controlled by chiefs in Lagos and then to weaken the chiefs' positions as middlemen-traders within the new order coincided with white civil servants' efforts to prevent the enthusiastic African scramble up the colonial ladder and with the churches' growing racism. The result from the last decade of the nineteenth century through World War II was the wedding of the modernizers' skills with the traditionalists' legitimacy into an opposition movement that Patrick Cole labels the first Nigerian nationalism.

By delving deeply into public and private archives, this Nigerian historian has come up with a detailed analysis of the Oluwa land case (1913–26), the first victory of traditional landholders that also brought Herbert Macauley, the educated grandson of an African bishop, to prominence. By examining the Eleko Affair (1920–31), a dispute over the *Oba*-ship (kingship) of Lagos, he shows how the newly mobilized traditional and modern elites formed a political movement and then a party. Thus, Cole brings to light new information about the Ilu Committee and the Nigerian National Democratic party (NNDP) and thereby joins G. Wesley Johnson and others in challenging the idea that politics in Africa began only after World War II.

Although the author proves that politics existed in Lagos, his evidence that it was Nigerian nationalism is incomplete. Two slim references to the NCNC and Action Group that led Nigeria to independence are insufficient to prove the connection between prewar articulation of elite interest and postwar sovereignty. And when he accuses scholars such as Coleman, Apter, and Young of allegedly ignoring the roots of nationalism, is he not

committing a similar offense by not taking into account changes going on outside of Lagos among non-Yoruba peoples?

The book bears strong traces of the dissertation—86 pages of rich footnotes compared with 191 pages of text—but polemics intrude. To dismiss Henry Carr, who opposed Macauley, as “Malcolm X’s House Negro” without mentioning his contribution to the modernization of Nigerian education is much more than an editorial blunder. Lacking balance and attention to nuance, the book is nonetheless a useful study of African elite behavior.

BRIAN WEINSTEIN
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T. O. RANGER and JOHN WELLER, editors. *Themes in the Christian History of Central Africa*. Berkeley and Los Angeles: University of California Press. 1975. Pp. xvi, 285. \$16.50.

This book presents deep but disjointed studies of a variety of themes grouped in three parts. Part 1 deals with the interaction between Christianity and Central African religions (though the latter are never defined). Consequently we get descriptive sketches of cultic movements, notably the M’Bona Cult, the Nyau Societies, the Mwana Lesa movement, and “heathen practices.” This superficial concentration on cults gives an unsatisfactory notion of religious interaction, since Christianity has interacted with African religions on many more and deeper levels. Part 2 describes Christianity and colonial society, focusing on the achievements of the Livingstonia Mission, the Epworth Mission, and three elitist Church leaders (A. S. Cripps, J. L. Membe, and A. May). Part 3 presents Christianity and contemporary society in Zimbabwe (Rhodesia). The book closes with two very useful indexes, general and thematic.

Themes in the Christian History of Central Africa has many of the normal scholarly qualities, and it is clear, readable, and informative. However, it presents only case histories, as if history were just a mosaic of special, bounded events. It assumes that other historical methods should “seek a more adequate balance,” and it brings attention to “the need for ‘inward-looking’ history.”

Of the fifteen essays and introductions, only two are written by Africans, and yet the book claims to present a “Christian history of Central Africa.” Thus, it is largely an outsider’s history and therefore an affront to African peoples and scholars. Can the foreigner, who also has been a colonizer, really describe and interpret meaningfully the history of those he colonized? One need only read the two essays by African contributors to see that

they describe a different historical environment—a living environment in which one hears the voices of the people and not the passing whistling of benevolent foreigners. Even if this book is academically sound, it is not “ideologically” convincing. At best, then, it is only an appetizer; it cannot fully satisfy the reader, despite the claims the editors make about it. And because it cannot, one must put an inevitable question mark upon its credibility and usefulness.

JOHN MBITI
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EDOUARD BUSTIN. *Lunda under Belgian Rule: The Politics of Ethnicity*. Cambridge, Mass.: Harvard University Press. 1975. Pp. xiv, 303. \$11.00.

With this important and probably definitive volume, Edouard Bustin proves that a scholar in political science need not be a prisoner of jargon or of theory and can have much to contribute to historical scholarship. Bustin produces a careful, two-sided study; it is both a modern history (mainly 1900–65) of the Lunda kingship, an important traditional African kingdom in southwest Zaïre (formerly the Belgian Congo), and a precise case study of the nature and impact of colonial rule on a largely rural section of Central Africa. Both topics are naturally connected, and the author succeeds in illuminating them while keeping in perspective African history and politics.

The first two chapters discuss the Lunda state’s murky origins and its history up to 1891 when the Belgians began to consolidate their hold. The heart of the fully documented study is in the next five chapters, which deal with the evolution of the weakened Lunda state and its relationship with the Belgian administration up to 1960, and with the independence of the country as the Republic of Congo (Leopoldville). The next-to-last chapter briefly discusses the first five years of independence, when the Lunda state became involved in a civil war, sparked by the secession of the Katanga province under the extraordinary local politician, Moïse Tshombe.

Bustin’s base of historical documentation is impressively thorough and varied: personal interviewing in the field, anthropological and historical scholarship, relevant provincial archives in Zaïre, and colonial archives in Belgium. The marrying of archival and field work is most felicitous.

This study is particularly valuable for its focus upon a neglected topic and period: rural conditions during the interwar period (1919–39). It will also interest those concerned with its discussion of ethnic feeling and African nationalism outside of the better-studied urban areas of Zaïre. The most

interesting and original chapters were chapter 6 ("Out of the Depression and through the War; The Bureaucratizing of Chieftaincy"); chapter 7 ("From Tribalism to Ethnicity: The Growth of Lunda and Cokwe National Sentiments"), and chapter 8 ("Decolonization, Secession, Reunification: The Lunda and Cokwe through the Congo Crisis"). African historians will find here some familiar themes, especially the ethnic conflicts and the artificiality of the old colonial frontiers, which were adopted wholesale by the new African states.

DOUGLAS L. WHEELER
University of New Hampshire

ANDREW D. ROBERTS. *A History of the Bemba: Political Growth and Change in North-eastern Zambia before 1900*. Madison: University of Wisconsin Press. 1973. Pp. xxxiv, 420. \$15.00.

Concentrating on the period prior to 1900, Andrew Roberts contributes a useful and often probing history of the Bemba peoples. He bases his study on extensive field work in Zambia during 1964 and 1965, skillfully blending oral traditional and linguistic, anthropological, and historical sources.

Roberts is best at describing the flow of Bemba history. Anyone who has ever struggled with the problems of African prehistory will appreciate his painstaking reconstruction of the chronology of the area. However, his analysis of the Bemba political system and its evolution, while quite properly redressing the previous misperceptions concerning the centralized character of its organization, is somewhat sketchy and in places unmindful of existing works dealing generally with traditional political systems. One comes away with a less than clear-cut understanding of the internal dynamics of the system, even though there is considerable detail concerning its operation.

Numerous charts, maps, and illustrations enhance the work; the footnote form (at the bottom of each page) will delight the interested reader. The book will be of interest primarily to those concerned with the history of Zambia and the formation of African political units.

CHRISTIAN POTHOLM
Bowdoin College

GODFREY MURIUKI. *A History of the Kikuyu, 1500-1900*. New York: Oxford University Press. 1974. Pp. viii, 190. \$10.50.

Within the last decade an increasing number of historians have produced studies of uncensitized East African societies. Of these, Godfrey Muriuki's treatment of the Kikuyu must be considered among the best. A Kikuyu himself, Muriuki presents deep insight into this somewhat exemplary

Bantu stateless society. In doing so he has produced something more valuable than East African scholars eager to know more about the Kikuyu would expect. His book is a model of historical analysis of structural and institutional development, especially pertinent to descriptions of the evolution and functions of social structure.

Many readers will be particularly interested in the author's efforts to dispel a number of tenacious misconceptions about Kikuyu relations with their neighbors. For instance, the Maasai and Kikuyu were not traditional enemies. Where they had mutual contact, they lived in a state of symbiosis. Trouble between them developed only when economic hardship forced the Maasai onto Kikuyu land and when the Imperial British East Africa Company began to take sides. The author also shows that the Europeans did not initiate the land alienation which caused the Kikuyu such grief; they did, however, intensely exacerbate it. This observation is a good example of the author's objective consciousness.

The detail and intricacy of the description of migrations and settlement of the many subgroups of the Kikuyu composite is quite impressive. The thoughtfulness and depth of comprehension of the impact of the age-set system (*riika*) on Kikuyu society is where the value of this study as a model becomes most obvious.

Little detracts from the high quality of this book, but, in the spirit of balance, it should be pointed out that the author has failed to emphasize the important distinction between the pastoral Maasai and Iloikop, a point that has always been crucial in Maasai-Bantu relations. Also, I have found Muriuki's explanation of the origin of circumcision practice and the age-set system rather confusing and lacking in solid evidence.

It should be obvious, however, that this is a very useful book. I look forward to more high-quality scholarship from the author.

CHARLES F. HOLMES
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EDWARD A. ALPERS. *Ivory and Slaves: Changing Patterns of International Trade in East Central Africa to the Later Nineteenth Century*. Berkeley and Los Angeles: University of California Press. 1975. Pp. xviii, 296. \$13.50.

As defined here, East Central Africa stretches from the coast of northern Mozambique and southern Tanzania inland to, and somewhat beyond, Lake Nyasa. While Alpers offers some revisions to accepted interpretations of the Portuguese impact on the East African coast in the sixteenth and seventeenth centuries, he concentrates on the eighteenth

century. A fascinating account emerges of the changing success of the Portuguese at Mozambique Island—relative to Kilwa, four hundred miles north on the Swahili coast, which Portugal lost in 1698—in attracting the overland ivory caravans, especially those of the Yao. Kilwa eventually became the preferred terminus, not because it was closer than Mozambique to the main sources of ivory, but because traders there offered better prices. Even at Mozambique, the Portuguese colonists found it difficult to compete with Indian merchants who had made themselves indispensable to the conduct of trade in general. With the effective opening of the East African slave trade in the late eighteenth century, at first to meet local demands in the Indian Ocean but during the “abolition” period to supplement supplies to the New World as well, Mozambique found a new line of business. Here again, though, Kilwa and the Swahili coast were not outdone. By tracing the slave trade into the nineteenth century, Alpers is able to bring to bear a more complex and exciting historiography.

But Alpers' most remarkable achievement is his original research on the eighteenth century in a hitherto unknown region. Whereas historians have studied the coast—though less well for the eighteenth than for preceding centuries—and the lower Zambezi Valley, they have totally neglected the hinterlands of Mozambique and Kilwa. The extreme rarity of firsthand, written sources before the mid-nineteenth century, the “stateless” nature of most societies of this region, and the perverse configuration of colonial boundaries have all conspired to discourage historical treatment and fieldwork. Though Alpers has not done the fieldwork and oral research, he provides the context for others equipped to embark upon it. His own primary work has been in archival sources, notably those in Lisbon, relating to Mozambique Island and the coast in general. What these suggest about the interior he correlates with more recently published ethnographic materials.

The book is based on Alpers' 1966 London University doctoral thesis. Since then he has rejected some of the then-fashionable assumptions on local initiative and development in response to international trade, and he has reframed the work in terms of underdevelopment. This is interesting, though he occasionally stresses the obvious. As he admits, he is limited by the available evidence. But once the theme of underdevelopment loses its luster to more advanced theories, one hopes that the basic materials Alpers presents here will prove sufficient for further testing.

J. E. G. SUTTON
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J. FORBES MUNRO. *Colonial Rule and the Kamba: Social Change in the Kenya Highlands, 1889-1939*. (Oxford Studies in African Affairs.) London: Oxford University Press. 1975. Pp. vi, 276. \$19.50.

ROBERT L. TIGNOR. *The Colonial Transformation of Kenya: The Kamba, Kikuyu, and Maasai from 1900 to 1939*. Princeton: Princeton University Press. 1976. Pp. 372. \$25.00.

The area of the Central Highlands of Kenya is an important one in the study of the impact of British colonialism on Africa. Unlike many parts of West and Central Africa or the interlacustrine area of East Africa, the colonial system in the central highlands confronted three politically decentralized societies—the Kamba, Kikuyu, and pastoral Maasai—who had experienced few Europeanizing influences prior to the twentieth century. Moreover, a relatively small but vigorous settler population during the colonial era produced a scheme of dualistic economic development unlike the situation in many other parts of Africa. Until the publication of these two new books, however, little scholarly attention had been paid to the colonial transformation of this area. Robert L. Tignor and J. Forbes Munro have rectified the situation, providing us with a wealth of information and some valuable new interpretations concerning the colonial impact on the peoples of the central highlands and their response to it.

Of the two books, Tignor's is the more ambitious. Through careful analysis of many interacting political, economic, and social factors present in each society, he is largely successful in attempting a comparative study of the adjustments and reaction of the three societies to the colonial presence. While his approach is undeniably comparative, there is a certain primary focus on the Kikuyu in many areas of the book. This is understandable, however, as he identifies the Kikuyu as the most receptive to, and therefore the most intimately involved with, the various processes of colonial transformation under investigation.

Munro's focus is more microcosmic. In fact, his title is rather misleading, for it is not the Kamba as a whole upon whom his investigations center, but rather only those of the Machakos district. His basic approach is an interesting one. Avoiding the more typical focus on a transition from a traditional to a modern society during the colonial era, Munro instead employs a “mobilization concept” in developing his central themes. He stresses that this approach not only avoids important terminological problems, but also “illuminates aspects of the colonial situation which can be too easily ignored in the search for tradition and modernity” (p. 248). While Tignor's examination is well conceived and developed, Munro provides an even

more thoughtful and, in some respects, more analytical examination of the colonial system and the nature of the African response to it.

Each writer bases his interpretations on an impressive array of data, drawn mainly from Kenya and from other archival sources, but also relying to some degree on oral materials. Because many of the same source materials were employed, and because each book examines the same theme in the same area and over the same time period, it is curious that more duplication of information does not occur. In fact, this happens only occasionally, for example, in their respective discussions on the female circumcision controversy (Munro, chapter 8; Tignor, chapter 15) and of the Kamba reaction to the forced destocking of the 1930s (Munro, chapter 11; Tignor, chapter 15). Munro's more specific focus usually insures that his treatment of a given Kamba situation is more detailed than Tignor's, although in a few instances Tignor somewhat surprisingly produces the more thorough examination, as with his discussion of the AIM and its activities among the Kamba (chapter 6 in each book).

Similarly, there are few substantial discrepancies in interpretation. The more significant include the respective investigations of Kamba resistance to wage-earning—Tignor emphasizes the lack of strong collaborative chiefs and an initial absence of severe land hunger (p. 108), while Munro emphasizes the ability to meet cash requirements through their own domestic economy (p. 83 ff.)—and Munro's assertion that Kamba chiefs frequently were succeeded by relatives (p. 70), which Tignor contradicts (p. 60).

While each book has individual strengths, they share a common weakness. Tignor's treatment of colonial chiefs (chapter 3), the labor situation (chapters 5 and 7), and education, with its very impressively researched background information on missionary groups (chapters 6 and 9), are all particularly successful. Likewise, Munro's studies of Kamba chiefs and councils (chapter 4), the build-up of land pressures (chapter 10), and his careful analysis of the process of colonial mobilization of Kamba resources (especially chapter 5) are exceptionally effective. However, in contrast to their careful treatment of the colonial period, the two authors are less successful in their handling of precolonial situations. Tignor, for example, does not appear to have a clear understanding, in some respects, of time-based class systems. One is also dismayed by his implied suggestion (p. 58) that evolution from a decentralized gerontocracy to a government by hereditary chiefs is in some way a natural political process. Similarly, Munro naively presupposes a single origin for the Kamba (p. 8), thereby ignoring a host of recent studies showing

that East African societies have evolved from a complex process of interaction and assimilation between different peoples over considerable periods of time.

Nevertheless, this shared weakness does not seriously detract from the very positive contribution these two books make to the historiography of East Africa. Indeed, the books often tend to complement each other, with Tignor's Kikuyu emphasis and Munro's Machakos Kamba focus providing valuable new insight into those two societies and establishing a sound basis for a very detailed comparative study of their respective reactions to British colonialism. What is required now is that someone produce an equally detailed and thorough examination of the response of the pastoral Maasai.

JOHN LAMPHEAR
DePaul University

ZEWDE GABRE-SELLASSIE. *Yohannes IV of Ethiopia: A Political Biography*. (Oxford Studies in African Affairs.) London: Oxford University Press. 1975. Pp. xiii, 334. \$24.74.

Anyone reading Zewde Gabre-Sellassie's fine political biography of his great-great grandfather, Emperor Yohannes IV (1872-89) is likely to conclude that the monarch was Ethiopia's greatest nineteenth-century politician and the architect of the modern Ethiopian empire. My only reservation about these evaluations is that I claimed them for Menilek II (1889-1913) in *The Life and Times of Menilek II: Ethiopia, 1844-1913* (1975), published a few months before the author's book. Uncannily, we both have used many of the same sources and basically agree with each other on most significant events. Our different analyses must derive from the contrasting locales of our subjects; not only are our focal points different, but we also might disagree about what Ethiopian empire each man unified.

Gabre-Sellassie strongly argues that it took a man like his ancestor—who had an ascetic and even disposition, a profound understanding of internal Ethiopian politics, and the flexibility of personality that would allow him to work with ambitious and fractious subrulers—to solidify the Solomonic crown's authority over traditional "Abyssinia." I concur heartily in this assessment, since I know, better than most, what a difficult man Menilek could be. If Yohannes' major achievement was to resurrect and consolidate patterns of authority in the north, then it was Menilek's deed to expand the state twofold, to its present size, and to incorporate large numbers of non-Semitic speakers, non-Christians, and nonagriculturalists. If Zewde wishes to see the beginnings of

this expansion as part of Yohannes' policy of strengthening his vassals wherever possible, as long as they retained their allegiance to him, so be it. The point is not worth arguing about, even if Menilek undertook his policy of territorial aggrandizement in part to obtain the finances necessary to purchase weapons so that he might topple Yohannes from the imperial throne, and even if most of the expansion occurred after Yohannes' death in 1889. Thus the author and I are both right: Yohannes unified the traditional Solomonic state, and Menilek unified the present Ethiopian empire.

Zewde's book is a vital contribution to the historiography of Ethiopia. Since 1960 Ethiopianists have written a series of good books and articles based upon materials drawn from European sources and Ethiopian documents, both oral and written. The author's research has been thorough, and his bibliography shows the richness of available materials. His use of the hitherto unavailable and little-known Heruy Amharic-language manuscript, "The History of Ethiopia," provides the scholarly world with some interesting insights into and fascinating documentation about the relationship between Yohannes and Menilek, which, by the way, could be used just as easily to derive analyses greatly at variance with the author's constructions. We can be very grateful, therefore, that the author has included several large excerpts in the appendixes. One might inquire, however, why he did not use Nadaw's readily available and valuable Amharic manuscript, "Ras Gobana's History," while he cited others not so important. If space permitted, I probably would have commented upon several small blunders and quibbled over some analytical vagueness that in several cases blurred important issues. I also would have complimented the author concerning his ideas on political assimilation and upon his astute analysis of Menilek's diplomatic relations with Egypt and Italy. Finally, although the author has described his book as a political biography, it is more a study of Ethiopia's unification, revitalization, and national survival in the late nineteenth century, and a good one at that.

HAROLD MARCUS

Michigan State University

HARRY A. GAILY. *Sir Donald Cameron: Colonial Governor*. Edited by PETER DUIGNAN and LEWIS GANN. (Hoover Colonial Studies; Hoover Institution Publications, number 139.) Stanford: Hoover Institution Press, Stanford University. 1974. Pp. xvii, 181.

The subject of this biography, Governor Donald Cameron, left no diaries. No collection of his private papers exists, and the memoirs that he wrote

were rather reserved and disorganized. Therefore, the materials available for his study are confined essentially to administrative records. With one disappointing exception, the Tanganyika archives, these are the records Harry A. Gaily uses to reconstruct Cameron's life.

Gaily recounts Cameron's services in Tanganyika from 1925 to 1931 and in Nigeria from 1931 to 1935. As governor of Tanganyika, Cameron's one great achievement was to resist plans for "closer union" with the nearby British colonies, especially Kenya, that were aimed at depriving Africans of their land and establishing the kind of settler colony which existed in Kenya. Gaily gives Cameron credit also for his policies toward "native authority." At times the author concedes that these policies were unrealistic, bringing about the imposition of "traditional chiefs" where none had previously existed, but on the whole he judges these policies as having been successful. A reading of J. Gus Liebenow, *Colonial Rule and Political Development in Tanzania: The Case of Makonde* (1971), might have underscored the shortcomings of Cameron's policies. Gaily's account of Cameron's activities in Nigeria confirms previous research on the shortcomings of British policy, especially in regard to their application in the country's northern districts.

While Cameron was an important individual who helped shape the future of the territories he ruled, the lack of records for a critical evaluation hardly makes him an ideal candidate for a biography.

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LEONARD THOMPSON. *Survival in Two Worlds: Moshoe-shoe of Lesotho, 1786-1870*. London: Oxford University Press. 1975. Pp. xx, 389. Cloth \$24.00, paper \$7.00.

In their introduction to the *Oxford History of South Africa* Leonard Thompson and Monica Wilson argue that "the central theme of South African history is interaction between peoples of diverse origins, languages, technologies, ideologies, and social systems, meeting on South African soil." In his present study Thompson emphasizes this theme in recounting the life of an African ruler whose career spanned the Early Iron Age to the Industrial Revolution. As a young man, Moshoe-shoe knew nothing of horses, firearms, or steam power. When he died, diamonds had been discovered in South Africa, railway building had started, and breech-loading rifles were changing the mode of warfare just as complex mining machinery was

altering the art of extracting ores from the ground. The young warrior was familiar only with a society organized in small principalities. The mature statesman had to cope with a situation where African overlords became progressively less capable of maintaining their sovereignty against their white neighbors—Boer and Briton—and where Great Britain seemed bound to dominate South Africa.

Moshoeshoe was the sovereign of a small country, but he was one of the great monarchs of history, comparable to such early medieval state builders as Alfred the Great. Moshoeshoe indeed created the Sotho nation. He defended it against a host of enemies—Boer trekkers, Kora raiders, British fighting men—and he skillfully managed to play his opponents against one another. In maintaining a precarious autonomy for his country the king had to make concessions of a kind found unpalatable by African nationalists of a subsequent generation. He benefited from the new lore brought to his country by Western missionaries. He submitted his country to British protection, thereby opting for the lesser of two evils. Great Britain had no direct interest in exploiting the Basotho, whereas the Orange Free State would gladly have deprived them of their remaining fields and grazing grounds.

In Leonard Thompson the king has found a worthy biographer who provides the first detailed account of the monarch's life in English. The author's work represents a considerable achievement. He skillfully uses information from a broad range of sources: archival, printed, and oral. He also makes good use of anthropological accounts, thereby avoiding the dangers of an approach excessively wedded to one particular discipline, a limitation from which much of South African history has suffered considerably in the past. Above all, Thompson writes in a clear and workmanlike fashion; though he is thoroughly familiar with the anthropological literature, he avoids its jargon. Neither does he indulge in the romanticism that in recent years has affected so much historical writing regarding precolonial Africa. The author gives a sympathetic account of precolonial societies, but he remains aware of their weaknesses. Overpopulation, overstocking, famines, and internal dissensions marked by witchcraft accusations and internecine war were not occasioned by colonial rule, but they were already familiar to the Sotho people of Moshoeshoe's time.

The value of this work has been enhanced by detailed footnotes (notes that appear at the foot of each page), by notes on informants and sources, and by well-chosen illustrations. The author might have omitted his somewhat superficial epilogue, which does not do justice to the study as a whole. Nevertheless, this is the standard work on the sub-

ject. It is not likely to be superseded for a long time.

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*Hoover Institution and
Stanford University*

MARIAN ROBERTSON. *Diamond Fever: South African Diamond History, 1866-9, from Primary Sources*. New York: Oxford University Press. 1974. Pp. 250. \$22.00.

Scholars dealing with the development of South Africa's diamond industry usually concentrate upon the years after 1870 when the gems became a significant factor in South Africa's economic, social, and intradiplomatic history. Marian Robertson, a feature writer for Radio South Africa, now deals with the first years of the diamond discoveries. She corrects and supplements previous accounts of the Cape government's response and its prior interests in Griqualand West, the site of the major discoveries, which it annexed in 1871. She identifies the government's attitude after the discovery of the first diamond in 1867 as one of "wait and see," an understandable position considering the earlier false hopes of finding profitable mineral deposits, and she notes the development of free-wheeling searching, buying, and selling of diamonds during these years of official nonintervention.

The book, however, is an awkward presentation of her findings. It is much more a stringing together of excerpts from primary sources than a coherent narrative likely to be read by its intended popular audience in South Africa. The text is difficult to follow, even for a fellow researcher wishing to determine the author's internal conclusions. A lack of sufficient maps, a degree of repetition in poorly divided chapters, and a narrowness of approach contrast sharply with the scope and diligence of the author's researches. Her commitment to testing and using primary sources is to be commended, and her lengthy extracts from the papers of Richard Southey (the Cape's colonial secretary at the time) as well as her assemblage of other contemporary sources make sections of the volume and its bibliography worth noting for those working on this period. Since so much needs to be done on both the nature of the mining industry and its relationship to other processes and features of South African history in the last third of the nineteenth century, it is with disappointment that one must give this volume such low marks.

LESLIE CLEMENT DULY
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Lincoln*

ANTHONY J. DACHS, editor. *Papers of John Mackenzie*. Johannesburg: Witwatersrand University Press, for African Studies Institute. 1975. Pp. xvi, 282.

Lord Milner, in a characteristic piece of overstatement, once noted that the Reverend John Mackenzie "changed the history of South Africa." This claim will not withstand close scrutiny, yet there is a kernel of truth hidden in Milner's exaggeration. As a missionary who labored for four decades among the peoples of Bechuanaland (modern Botswana) and as a vocal proponent of an enlightened form of British imperial rule, Mackenzie exerted considerable influence in the South African interior during a critical phase of the region's history. This book draws on Mackenzie's extant papers, notably the large collection now housed at the University of the Witwatersrand, in an attempt to delineate the nature of that influence in three distinct areas.

The author sees Mackenzie as a student of African peoples and societies, as a servant of Christianity, and as an imperialist who functioned as a British deputy commissioner in Bechuanaland during the 1880s. This thematic approach, illustrative of three important facets of Mackenzie's career, is a departure from the practice normally followed in volumes of this type. Yet the method employed by Dachs, a capable young Rhodesian historian, merits praise. To be sure, one misses the comprehensive biographical insight that might have been derived from a general cross-section of Mackenzie's papers arranged chronologically, but the editor's choice of material sensibly and sensitively reflects both the missionary and his writings. The coverage is also certain to please all who stress an interdisciplinary approach to the African past. Here is grist for the mills of the social anthropologist, the student of missionary enterprise, those interested in the interaction of strikingly different cultures, and the imperial historian.

The revelation of the wide research potential of the Mackenzie papers is, in fact, the work's single most important feature. Carefully edited and annotated, this volume marks an auspicious beginning for a projected series based on the Witwatersrand University Library's archives. Incidentally, it also builds a persuasive case for a full biography of Mackenzie. That written by Mackenzie's son shortly after his father's death lacks both perspective and impartiality, while Anthony Sillery's recent work suffers from his not having used the more than 3,000 items upon which the present volume is based. Perhaps Dachs will use his background preparation and the abilities displayed in this edited work to produce a complete portrait.

JAMES A. CASADA
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ASIA AND THE EAST

WM. THEODORE DE BARY *et al.*, editors. *The Unfolding of Neo-Confucianism*. (Studies in Oriental Culture, number 10, Conference on Seventeenth-Century Chinese Thought.) New York: Columbia University Press. 1975. Pp. xiv, 593. Cloth \$20.00, paper \$9.00.

In *The Unfolding of Neo-Confucianism* thirteen scholars have produced a work of immeasurable importance for the entire field of modern Chinese intellectual history. Much more than a sequel to the editor's previous volume, *Self and Society in Ming Thought* (AHR, 76 [1971]: 1205), the present book goes beyond the mere introduction of a new perspective to attempt a major revision of the prevailing interpretation of intellectual stagnation in late imperial China. Although he wishes "not to stigmatize [the prevailing view] as a tissue of modern myths and prejudices, superimposed on a misunderstood past," Wm. Theodore de Bary believes that the scholarship represented here will serve to correct our contemporary vision of a China dependent on Western values, forced to remove the dead hand of tradition before it could emerge from an orthodox and culture-bound darkness into the enlightened modern age. Whether or not de Bary's own vision is accurate, the book should have its intended effect. The Neo-Confucian tradition appears as creative and as critical in the seventeenth century as in the eleventh, and the book's collective thesis, that spiritual enlightenment, moral renewal, and the critical methods of "evidential investigation" (*k'ao-cheng*) were mutually supportive and historically linked, is sustained.

Three of the articles, by Wing-tsit Chan, de Bary, and Tang Chun-i, to whom the work is appropriately dedicated, are in fact sequels to their authors' contributions in the earlier volume and provide the interpretation linking the two. Together with Araki Kengo's and Edward Ch'ien's, they argue persuasively that the concept of moral mind was central to the development of Neo-Confucian thought, the seemingly divisive issues relating to ontology, epistemology, practicality, and religiosity notwithstanding. Chan's article, which concludes the volume, points out the emphasis on critical method and systematic doubt in an early eighteenth-century document previously assumed to represent a denial of these basic Neo-Confucian concerns because of its reassertion of state orthodoxy.

If the presentation of a consistent unfolding of ideas based on a faith in personal and social renewal is the strength of this book, the paucity of social or political analysis is its very large weakness. William Atwell's lively article on the political

and literary movement known as Fu She concludes that the group was primarily motivated by renaissance ideals. Kristin Yu Greenblatt also suggests that Buddhist moral cultivation appealed to literati interests and led to a florescence of lay Buddhism. Richard Lynn's excellent analysis of poetry as a form of self-cultivation and other such ground-breaking essays all help to humanize our image of the orthodox mind. But none asks why the creative spirit of renewal was never realized in reform, but channeled into orthodoxy, classical research, and other concerns which de Bary calls "totally relevant to the immediate needs of the state, society, and culture that supported them." For the sake of historical truth, as distinct from philosophical or religious, this question must also be answered before the revision is complete. In the meantime scholars can only applaud this substantial contribution to the history of ideas.

JERRY P. DENNERLINE
Pomona College

JOHN E. WILLS, JR. *Pepper, Guns, and Parleys: The Dutch East India Company and China, 1622-1681*. (Harvard East Asia Series 75.) Cambridge, Mass.: Harvard University Press. 1974. Pp. xv, 232. \$12.00.

Wills' painstaking but lively study, which is based upon thorough research in the Dutch archives and in published Chinese sources, begins in 1662 with the expulsion of the Dutch from their base on Taiwan by the corsair Zheng Cheng-gong (Coxinga). During the next two decades, while the sea lord's heirs ruled that island, the Dutch sporadically combined with the new Qing dynasty on the Chinese mainland, first to wreak vengeance upon Zheng, and then to gain trading privileges from their own allies. The naval forces of this "uncertain alliance" on several occasions jointly attacked the corsairs, and the Dutch received a biennial trade agreement at Fuzhou; but the "accumulated irritations" of their dealings resulted in the revocation of Dutch privileges in 1666 and the growing conviction on the part of the company's representatives in Batavia that trade with China was a hopeless venture.

During the Three Feudatories' Revolt (1673-81) the Dutch had a second chance. The Qing dynasty was again interested in the company's naval support against Zheng and sent envoys to Batavia in 1679 to offer trading privileges in exchange for military aid. These "farcical" negotiations ended in an impasse. The suspicious Dutch insisted upon guarantees that the envoys were not empowered to give, and the expedition never materialized. By 1681 the Dutch had abandoned their trading post in Fuzhou and thereafter mainly traded with China through intermediaries.

The bulk of Wills' analysis stresses the cultural obstacles to understanding between the Dutch and Chinese, each with their own "diplomatic traditions." Wills tells us, however, that the "irritations" in the 1660s and 1670s were less important causes of the "broken dialogue" than other basic developments that took place at the very end of, or after, the period he has studied. Their victories in Java in 1681 turned the Dutch away from China, while the Kangxi emperor's opening of legal trade in 1684-85 meant Qing abandonment of fickle military alliances for long-term commercial monopolies. There is thus a discrepancy between Wills' assertion that this study of Sino-Dutch relations is about the "emergence of a new political and commercial order in relations between China and the west" in the 1680s and his actual emphasis in the main body of the text upon failures of diplomatic communication before that new order actually emerged. Nevertheless, *Pepper, Guns, and Parleys* corrects the mistaken impression that tributary diplomacy always stemmed from the Chinese sense of cultural superiority, and Wills considerably enriches our knowledge of seventeenth-century Asian history by exposing two decades of the Dutch experience in China.

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ROBERT B. OXNAM. *Ruling from Horseback: Manchu Politics in the Oboi Regency, 1661-1669*. Chicago: University of Chicago Press. 1975. Pp. viii, 250. \$12.50.

The author of this work suggests, with a bow to the late Mary Wright, that the Oboi regency be labeled the "last stand of Manchu conservatism." In the regency period (1661-69), China and the fortunes of the Ch'ing dynasty were at a crossroads. The Sinification policies of early Manchu leaders were subjected to re-evaluation by the regents. And, as the author effectively demonstrates, a nativist reaction flourished as the regents sought "to create an order in which the Manchu system, Manchu officials, and Manchu ideas held undisputed control over Ch'ing China." They succeeded briefly in holding back the tide of Sinification, but when the K'ang-hsi emperor seized power from Oboi in 1669, he built a more harmonious and effective relationship with the Chinese.

Relying largely on official historical and biographical collections, Robert B. Oxnam examines the politics and policies of the Oboi regency against this backdrop of Sinification versus Manchu dominance. The author traces the origin of the regency to the early Manchu wars of conquest and court intrigue, building up an image of the regents

as steeped in Manchu concerns and values. After their appointment in 1661 as regents for the young K'ang-hsi, they began to display their Manchu face to the empire.

In both the metropolitan and provincial bureaucracies, Manchu and Chinese bannermen were elevated over the Chinese official elite, and Manchu-oriented institutions took precedence over their Chinese counterparts. From the Chinese provincial elite, Oxnam argues, the regents demanded absolute obedience and recognition of Manchu superiority. Unable to develop an ideology that could replace Chinese norms of control, they resorted to force in confronting southeastern gentry over tax evasion and Ming loyalism. In their military and foreign policies, however, the regents were strangely passive. According to Oxnam, the regents were faced with military and economic exhaustion after the wars of conquest and felt it was reckless to undertake any new initiatives. They did undertake institutional innovations, however; the most notable example was their governance and defense of Manchuria. Oxnam concludes his work by carefully detailing the shifting balance of power from Oboi to the imperial faction around K'ang-hsi.

Ruling from Horseback is a welcome addition to the growing list of works analyzing the dynamics of alien rule in early Ch'ing China. Oxnam's study provides a more sophisticated view of the Sinification process and points to the important role of K'ang-hsi as the final consolidator of Ch'ing rule.

LAWRENCE D. KESSLER
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PEI HUANG. *Autocracy at Work: A Study of the Yung-cheng Period, 1723-1735*. (East Asian Studies.) Bloomington: Indiana University Press for the International Affairs Center. 1975. Pp. xii, 500. \$15.00.

The reign of the Yung-cheng emperor, spanning the years 1723 to 1735, is of central importance in the study of Chinese autocracy. Building on a number of innovations in the spheres of bureaucratic organization and communications that had been developed by his father (the K'ang-hsi emperor, who ruled from 1661 to 1722), Yung-cheng strengthened imperial power to an extent perhaps unique in his country's history. This power was rational and bureaucratic—far from the tyrannical whims that typified some Chinese rulers before him; though because Yung-cheng was also harsh, and severely mistreated or killed some of his brothers after his accession to the throne, his institutional achievements have been obscured by

dramatic but unsubstantiated charges that he was a usurper.

The first survey of this reign to appear in English was Huang Pei's 1963 Indiana University dissertation, "A Study of the Yung-cheng Period, 1723-1735: The Political Phase." This work drew widely on that remarkable monument to the Yung-cheng emperor's industry, the *Chu-p'i yü-chih* or "vermillion endorsements," a vast collection of memorials with imperial notations—often of considerable length—which appeared in the late 1730s. In view of the importance of this period in Chinese history, it is remarkable that thirteen years later that dissertation—now revised and expanded by a section on "Aspects of Autocracy," and two new chapters on the Banner System and on ethnic minorities in southwest China—is still the only detailed survey of the period. Besides the two new chapters just mentioned, the work has detailed chapters on the succession crisis of 1703-23, the development of the palace memorial system, and the formation of the Grand Council, and it gives as well general coverage to "the literati" and "the populace."

These are all major topics, and worthy of monographic treatment, though there are numerous puzzles in Huang's analysis of them. Why, for instance, if the emperor's efforts to emancipate the "mean people" were ineffective should these measures be called "epoch-making, politically and socially" (p. 20)? Why is he called "the only" Ch'ing emperor to master Ch'an Buddhism when his own grandfather was a passionate believer (p. 33)? How, in the vastly complex politics of the period, can "ordinary officials" stand as a category in analyzing factions (p. 86)? Why should palace memorials be used to weaken censors' powers, when the censors had so little power anyway (p. 119)?

Another disappointing aspect of Huang's book is his failure to consider the most interesting work on the Yung-cheng emperor that has appeared in English since he wrote his dissertation. For in the last few years, besides a good deal of monographic work in Chinese and Japanese on the Yung-cheng reign (which Huang Pei surveys quite thoroughly), two important studies in English have appeared that add new dimensions to our knowledge of the Yung-cheng emperor's character, policies, and abilities. One of these has been published, Silas Hsiu-liang Wu's *Communication and Imperial Control in China: Evolution of the Palace Memorial System, 1693-1735* (1970); the other is Kent Smith's Yale dissertation of 1970, "Ch'ing Policy and the Development of Southwest China: Aspects of Ortai's Governor-Generalship, 1726-1731." Huang Pei's chapter on the Southwest, the major new chapter added since his 1963 dissertation, covers almost exactly the same ground that Smith did, though

Huang does not state if he has drawn on Smith's work, an unfortunate oversight in view of their identical coverage; Huang merely mentions (without comment) one of Smith's briefer papers on the same topic in his bibliography. In the case of Wu's work, Huang is consistently dismissive, both of the monograph on palace memorials and of a number of other interesting and scholarly articles by Wu; the dismissive remarks are kept brief and ambiguous in the footnotes, however, so we do not get any hints as to why Huang keeps repeating older arguments that Wu has been trying for some years to modify. Huang's remarks in his new introductory chapter on "administrative efficiency," for instance, seem to draw significantly on the conclusion to Wu's book, but we are not told whether or not that is so.

Autocracy at Work would have been a better book had it been either less grudging or more analytically rigorous. It is still, however, a work of considerable scholarship, and it will be of help to any Western scholar starting out to explore the Yung-cheng period.

JONATHAN SPENCE
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JONATHAN PORTER. *Tseng Kuo-fan's Private Bureaucracy*. (China Research Monographs, number 9.) Berkeley: Center for Chinese Studies, University of California. 1972. Pp. 151.

Recent studies of Tseng Kuo-fan, a pivotal figure in modern Chinese history, have turned from the significance of his regional army in China's military history to the importance of his administrative apparatus in the modernization of China's civil government. Tseng created both institutions in the course of suppressing the Taiping Rebellion (1850-64). His administrative structure evolved from what Jonathan Porter calls his "private bureaucracy" and was intended to support the military operations (p. 14). Porter's monograph examines several aspects of this "private bureaucracy" in order to identify its "institutional departures" that, he claims, contributed to China's modernization in the nineteenth century.

Tseng developed his private bureaucracy from the "ambivalent tradition" of the *mu-fu* system (an official's personal staff), which had evolved into an informal, but distinct, institution in the Ming-Ch'ing period. The author, relying on the Weberian criteria for an "ideal bureaucracy," considers Tseng's method of recruiting talent (specialists) as "rational." It was based on "knowledge" and characterized by "impersonality" (p. 40).

The author has helped clarify the inner mechanism of Tseng's private bureaucracy (just as Philip Kuhn has clarified the structure of Tseng's re-

gional army). He finds that Tseng developed his *mu-fu* system into an "inner *mu-fu*," which consisted of Tseng's intimates and associates and was involved with "higher-level" decisions, and an "outer *mu-fu*" of lesser functionaries and attendants. Out of this dualistic system Tseng was able to build his structured and formalized administrative apparatus, characterized by "autonomous bureaucratization."

This is a useful addition to Kenneth Folsom's fairly recent and more comprehensive study on the *mu-fu* system. Nevertheless, the precise link between Tseng's "institutional departures" (the "rational" aspects of his bureaucracy) and the larger problem of China's modernization process in civil government awaits a more serious study in the future. In particular, scholars still need an analysis of the bureaucratic situation and development in the nineteenth century based on a Chinese, rather than a Western, framework.

SILAS WU
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JOHN K. FAIRBANK, edited and with an introduction by. *The Missionary Enterprise in China and America*. (Harvard Studies in American-East Asian Relations, 6.) Cambridge, Mass.: Harvard University Press. 1974. Pp. 442. \$15.00.

This book consists of twelve articles dealing with various aspects of American missionary work in China. There is an abundance of church archives, missionary writings, and other relevant materials dealing with American missionary work in China, but this is probably the first and the best integrative and interpretive study of such work. John Fairbank mainly discusses three topics: "the American missionary expansion or outward thrust, the impact of this missionary effort on the Chinese people and society, and the backflow of influences affecting the missionaries' home constituency and the American people in general" (p. 6).

The strengths of the work can be enumerated as follows. First, the range of contributors is healthily diverse. While the majority of the contributors are modern Chinese historians, others include historians of American history (James A. Field, Jr. and Arthur M. Schlesinger, jr.), church historians (William R. Hutchison and M. Searle Bates), and even one former influential missionary in China (Bates). The articles by these "outside" authorities are extremely educative and inspiring to students of modern Chinese history. For instance, Hutchison's and Bates' articles explain for almost the first time the mind of the missionaries who went to China. Second, many of the findings in

this work are original and worthy of serious attention. Valentin H. Rabe demonstrates the high quality and noble calling of many missionaries, which should cast doubt on Schlesinger's analytical framework of Prospero versus Caliban in explaining the relationship between missionaries and their converts. Rabe's article suggests that the missionary Prospero did not go abroad to assuage wounded pride at having lost out in his own country. Adrian A. Bennett and Kwang-ching Liu illustrate the debates on Christianity and Confucianism engaged in by the Chinese converts in the nineteenth century. In these debates we see that converts' arguments are by no means all superficial; rather, they are very serious. Therefore, we must reconsider the question as to how many rice-Christians really existed in nineteenth-century China and give more respect to the genuineness of the Chinese faith. Paul A. Varg tries to prove that between 1927 and 1932 American church leaders and missionaries actually shared the same timidity as the American government officials in removing unequal treaties, contradicting our previous belief that church and missionary forces in the 1920s and 1930s were all genuine and enthusiastic supporters of Chinese nationalism.

But some of the findings or conclusions of the work are rather debatable. As for missionary motivations, Field lists the major three: Christ's command, humanitarian compulsion, and romantic adventurism. A perusal of the writings of some major intellectual leaders of the nineteenth century, such as Kidd and Mahan, indicates that one of the major considerations for Western and American outward expansion was to arrest the perils, yellow or otherwise, by Christianizing them. How much influence did this consideration exert upon the missionaries? Recent works by Akira Iriye, Ernest May, and David Healy have shed a different light on this matter.

As for the question of whether missionary endeavor was a failure or success, it seems that many of the contributors of this work would consider it a failure. Was it really? How much success is enough to call it a success? If success means that several hundred missionaries in the nineteenth century and several thousand in the twentieth century should have converted a substantial segment of the Chinese population, then, of course, the endeavor was a failure. But have we asked the question: where else and when in human history has it been that such a meager force (in comparison with the Chinese population) has founded so many schools, hospitals, relief agencies; has transmitted modern thought so effectively to an entirely different culture; and has also converted

one and a half to two million ethnocentrics, which after all is no small number?

Another debatable point of this work is that many contributors have tried to relate the American missionary experience in China to the American fiasco in Vietnam. If we argue that it was a fallacy to use the 1930s experience to justify American intervention in the 1960s, the present attempt might be just another such dangerous historical analogy. Fairbank has this wise rejoinder: "One must . . . be past-minded about the missionaries themselves, for they were Americans of an earlier generation who lived in a far different and simpler world than we face today." He concludes that "it is therefore impossible for today's researchers in this new field to 'do justice' to American Protestant missions in China as a whole" (pp. 15-16). Furthermore, if we agree to this rejoinder, then the accusation that missionaries were cultural imperialists, as Schlesinger has called them, is dangerous and misleading.

The work should also point out many new areas of research for scholars. First, more studies on the Chinese Christians' intellectual responses to Christianity are needed. Philip West's article on Wu Lei-ch'uan of Yenching University gives a fascinating account of how a Chinese Christian leader interpreted Christianity according to Chinese national needs. Any future study should deal with how other Chinese Christian leaders in the twentieth century have thought about and interpreted Christianity. Men like Chao Tzu-ch'en and Hsü Pao-ch'ien, to name a few, deserve serious investigation. Until scholars know their ideas in this matter, they will never know how well or how poorly the Christian message has been transplanted. A topic related to this one could be a study of the Chinese National Christian Council. What role and influence did this organization play in the development of the Chinese Christian movement? Furthermore, Varg's article is a challenge to re-examine our knowledge about the missionaries in the twentieth century. Just how much different was their attitude, spiritual and political, from that of their nineteenth-century comrades? From his article we can see that many missionaries perceived the weaknesses of the Nationalist government and even prophesied the eventual triumph of the Chinese Communists. This should make us, the intellectual mandarins, humbler and more serious about the writings of the missionaries in our study of modern Chinese history, for after all they were first-hand observers and spent most of their lives in China.

The present work is indeed a landmark in the study of American missionary work in China. Fairbank has edited the book with great imagina-



tion and resourcefulness, and his introduction is succinct and masterful.

YU-MING SHAW
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PHILIP C. HUANG. *Liang Ch'i-ch'ao and Modern Chinese Liberalism*. (Publications on Asia of the Institute for Comparative and Foreign Area Studies, number 22.) Seattle: University of Washington Press. 1972. Pp. ix, 231. \$9.50.

Since 1953, five books have been written about Liang (1873-1929). Huang's monograph is the least successful, adding nothing of theoretical interest and little of factual import to the other four. It recognizes and explores briefly a critical development in the growth of Liang's unsteady political philosophy around 1902-03, namely his shift from a focus on individualism to statism. This book also provides some insight into the Japanese intellectual context with which Liang interacted and for which he was a conduit to Chinese readers of his popular journalism. Huang portrays Liang's interpretive translations of Japanese materials and his education into German *Staatsrecht* via Japanese intermediaries in an interesting fashion.

Fortunately, another book focusing with great skill on these issues appeared at the same time as Huang's. Chang Hao's *Liang Ch'i-ch'ao and Intellectual Transition in China, 1890-1907* (Harvard, 1971) does a superb job of explicating Liang's persistent concern with associative values and his key political tract, *The New Citizen*. This book also gives great insight, lacking in Huang's, into the traditional linkages of Liang's political thought, especially the school of "practical statesmanship." Chang Hao's intelligent treatment of Liang's uses of British utilitarianism and Social Darwinism likewise finds no counterpart in Huang's sometimes confused and superficial discussions.

Huang uses the term liberalism indiscriminately, with no more than a casual association with democracy or "individual rights" to specify the term in the first third of the book. Implicitly, in the remainder of the book, utilitarianism, libertarianism, philosophic radicalism, and democracy all jostle each other in the vagaries of his use of liberalism. There is little cooperation between his book's narrative, its subject (Liang and liberalism), and its apparent argument (that Liang abandoned liberalism, understood as individualism, as soon as he embraced it). The *coup de grâce* is the description of Liang's purported reaffirmation of his so-called liberal faith (pp. 147-49). While we are again trying to decode "liberal" (individualist?), the author tells us that Liang has shifted to a belief in cooperation, and he then concludes that Liang has come to a "sub-

stantially different liberal faith." It is unfortunate that the author's useful insights and often imaginative research were saddled with the dead weight of ill-considered terminology.

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Buffalo

ALBERT FEUERWERKER. *Rebellion in Nineteenth-Century China*. (Michigan Papers in Chinese Studies, number 21.) Ann Arbor: The University of Michigan Center for Chinese Studies. 1975. Pp. vii, 101. \$3.00.

ELLIS JOFFE. *Between Two Plenums: China's Intraleadership Conflict, 1959-1962*. (Michigan Papers in Chinese Studies, number 22.) Ann Arbor: The University of Michigan Center for Chinese Studies. 1975. Pp. x, 72. \$3.00.

S. BERNARD THOMAS. *"Proletarian Hegemony" in the Chinese Revolution and the Canton Commune of 1927*. (Michigan Papers in Chinese Studies, number 23.) Ann Arbor: The University of Michigan Center for Chinese Studies. 1975. Pp. x, 187. \$3.00.

A common thread—the strivings of rebels and revolutionaries in retreat and in defeat—connects these three short monographs from the University of Michigan. Albert Feuerwerker's seasoned study treats a century of dissidence at various levels of intensity and menace to the Ch'ing dynasty. Ellis Joffe, with impressive scholarship, ruminates on various interpretations of Cultural Revolution materials that cover the power struggle during four uniquely discouraging years. S. Bernard Thomas conscientiously examines the Comintern's weak adventure in China, the disastrous Canton Commune of December 11-13, 1927.

Feuerwerker, in his one-hundred-page study, attempts "to describe and analyze as one broad social phenomenon . . . a remarkable number and variety of unsuccessful challenges to the Manchu regime" between 1796 and 1895. Following a skillful retelling, apparently from standard sources, of the stories of various uprisings, but emphasizing the Taiping Rebellion, he examines sources of social dissidence under three headings: social strains, various ideologies of rebellion, and the discontent of rebellious social groupings. He presents a valuable classification of ascending levels of rebelliousness in "a hierarchy of dissidence"—"ad hoc local riots; lineage and communal feuds . . . ; religious and political underworld [activity] . . . ; politicalized bandits; rebellion . . . ; and unqualified political and social revolution."

The study ends with a page of reading suggestions, listing twelve contemporary works by ten authorities. Both specialists and teachers will miss intratextual reference to these authorities, along

with critical footnotes. Feuerwerker's ideas overflow and his style is smooth. The book is interesting and provokes reflection. His analysis highlights the fact that dissidence usually vanishes quickly when it goes unchanneled by purposeful leadership.

Joffe's purpose in *Between Two Plenums* is to isolate threads of historical continuity that stretch directly to the Cultural Revolution, in the context of power struggle. Thus, the collapse of the Great Leap Forward was the event that shattered intra-leadership consensus and opened the period under discussion. The public disputes of 1962, eventuating in Mao Tse-tung's return in formal triumph with the nominal support of a united leadership at the September 1962 Tenth Plenum of the Eighth Central Committee, ended the period. Between these points Joffe discusses the failure of P'eng Tehuai's 1959 challenge to Mao and the purge of P'eng, subsequent political churning and leadership retreat, intellectual dissidence in Peking, continuing submerged controversy over what to do, and Mao's retreat and return.

Joffe does not orient his readers with respect to the PLA and its politicization by Lin Piao, contemporary strategic and nuclear matters, the Sino-Soviet quarrel, Chou En-lai's crumbling diplomacy of those years, the faltering economy as a whole, the ongoing planning crisis, or the significance of the dissidence of the Peking party committee dominated by P'eng Chen, then a formidable figure. It is fortunate that pages 344-61 of Joffe's chapter in the Lindbeck volume ("The Chinese Army Under Lin Piao: Prelude to Political Intervention," in John M. H. Lindbeck, ed., *China: Management of a Revolutionary Society* [1971], pp. 343-74) cover in succinct and usable fashion most of the ground of the present fifty-eight-page study and can be recommended for his views on PLA involvement at that time.

S. Bernard Thomas, in an ideologically directed study of 102 pages with 84 pages of notes, analyzes the outcome of the Canton commune failure for urban-oriented Communists. He takes up the 1928-31 Comintern and Chinese Communist party evaluations, the later appearance of the proletarian hegemony theme in the Kiangsi Soviet from 1931 on, "Maoist proletarian hegemony" in the war years, and, after 1949, Maoist ideological, class, and developmental patterns in relation to the "new symbolism" of Canton.

Thomas' conclusions begin with the statement that "the Canton Commune, both as revolutionary history and political symbol, has apparently run its full course in China to its present status as a virtual nonevent" (p. 175). However, his own work shows it was more than that: the symbolic content metamorphosed over time, and the original signifi-

cance of the proletarian hegemony theme against the backdrop of an old-fashioned semicolonial peasant society has simply lost its relevance in the current phase of socialist construction in China.

The ideological analysis deserves more minute evaluation than I can provide. However, Thomas has clearly traced a stream of ink, if not of causation, from Canton to Peking in 1966, when radical democracy was again briefly in vogue. If only Thomas had discussed the 1966 and 1971 propaganda themes of the Paris Commune against the backdrop of his study, then the most intriguing current question stimulated by his work might have been satisfied.

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WINSTON HSIEH. *Chinese Historiography on the Revolution of 1911: A Critical Survey and a Selected Bibliography*. (Hoover Institution Studies, 34.) Stanford: Hoover Institution Press, Stanford University. 1975. Pp. xvi, 165. \$9.00.

This short book is an important contribution to the growing Western literature on the republican revolution of 1911. It consists of two unequal parts. The first is an extended essay highlighting "the representative works and the major trends of Chinese historical writings on the Revolution" (p. 14). In it Hsieh deplors the narrow focus through which both Nationalist and Communist writers have heretofore viewed the revolution, particularly their excessive preoccupation with Sun Yat-sen and his revolutionary organizations. It is an outstanding historiographical survey—independent-minded, critical, comprehensive, yet concise—but aimed perhaps more at the working scholar than the beginning student. Some of the references, for example to Sun's disavowal of parts of his book *Kidnapped in London* (p. 16) and Ch'ü Ch'iu-pai's attacks on P'eng Shu-chih's social analysis (p. 44), deserve more of an explanation than they are given.

The second part of the book is a list of 368 selected Chinese titles, articles as well as books, for which the preceding essay serves as the annotation. This list, which is by no means exhaustive, consists mostly of secondary works and compilations of primary materials but, generally, not the primary sources themselves. The utility of the list is greatly enhanced by the inclusion of the call number of each item at the East Asian Collection of the Hoover Institution, which has all but about twenty of the works listed. One wishes that the location of the remaining items had also been indicated, since they appear to be quite rare. One wishes, too, that the book had been proofread with

greater care to catch the excessive number of missing characters and mistaken transliterations.

The author of a recent Harvard dissertation on the 1911 revolution and the coeditor of the Chinese volume of the Skinner bibliography on modern Chinese society, Winston Hsieh brings to this project unparalleled credentials. It is thus to be regretted that, for some unexplained reason, the book, though completed in 1972, was not published until 1975.

EDWARD RHOADS
University of Texas,
Austin

DIANA LARY. *Region and Nation: The Kwangsi Clique in Chinese Politics, 1925-1937*. (Cambridge Studies in Chinese History, Literature, and Institutions.) New York: Cambridge University Press. 1975. Pp. x, 276. \$19.50.

The relationship between regionalism and nationalism is one of the most difficult questions facing the historian of modern China. To what extent was China becoming, in the words of St. Augustine, "an assemblage of reasonable beings bound together by a common agreement as to the objects of their love," and to what extent was the primary object of their love the nation rather than the region, the village, the clan, or the family? Diana Lary deals with these questions forcefully and directly. She argues that the leaders of Kwangsi province, the Kwangsi Clique, were both regionalist and nationalist. They believed in a kind of "layered nationalism" in which regionalism in the present was a means to achieve nationalism in the future. The author quite correctly points out, however, that the persistence of regionalism undercut national unity. The Kwangsi Clique sought to escape their regionalist identity by becoming part of a nationalist movement; in the process they became more, rather than less, regionalist and in control of a larger region.

Lary has performed a number of difficult tasks well. First, she has taken a single province, Kwangsi, and placed it in the context of national politics from 1925 to 1937. As a provincial study, *Region and Nation* underlines the precariousness of KMT power in the border provinces. Second, she provides an excellent discussion of the second level of the Kwangsi Clique. In addition to the four leaders—Li Tsung-jen, Huang Shao-hsiung, Pai Ch'ung-hsi, and Li Chi-shen—the second level provided a core of men indispensable to the survival of the clique. Why was the Kwangsi clique unusually cohesive? The answer lies in the strong sense of provincial identity at the second level, in military service patterns which encouraged second-level leaders to remain in Kwangsi, in "a com-

munity of interest" between the first and second levels, and in the fact that no prominent individuals emerged from the second level to challenge the leaders for supremacy. Third, she provides a lucid account of the Kwangsi Reconstruction Movement and contrasts it with the National Reconstruction Movement. Unlike national leaders, the Kwangsi Clique was aware that reconstruction has to start in rural, not urban, areas, and had to penetrate to the village level. For all their efforts in reconstruction, however, Kwangsi leaders ultimately lacked the determination to effect thorough social change.

Region and Nation might well serve as a model for other writers of provincial history. Lary has both grounded her work in careful research and exercised her sense of historical imagination to illuminate the ways in which provinces relate to the Chinese nation.

DAVID DEAL
Whitman College

T. A. BISSON. *Yenan in June 1937: Talks with the Communist Leaders*. (China Research Monographs, number 11.) Berkeley: Center for Chinese Studies, University of California. 1973. Pp. 71. \$5.00.

DEREK J. WALLER. *The Kiangsi Soviet Republic: Mao and the National Congresses of 1931 and 1934*. (China Research Monographs, number 10.) Berkeley: Center for Chinese Studies, University of California. 1972. Pp. 116. \$5.00.

The studies under review deal with two of the most critical periods in the history of the Chinese Communist movement. They do so in completely contrasting styles.

Derek Waller's *The Kiangsi Soviet Republic: Mao and the National Congresses of 1931 and 1934* is a careful study of the politics of the formative meeting of the Central Soviet government created in Kiangsi in November 1931 and of its Second National Congress in January 1934, held only a half-year before the beginning of the legendary Long March to the north.

The addition of Mao's name to the drier title of Waller's 1968 University of London dissertation on the same subject is ironic, given his conclusion that the 1931 and 1934 congresses, "far from being stepping stones on Mao's road to power, were in reality evidence of his temporary decline" (p. 114). The group known as the "Russian Returned Students" or "28 Bolsheviks" had replaced Mao and many associates from most positions of authority by early 1934, but the disasters of succeeding months and the early stages of the Long March subsequently precipitated the definitive ascendancy of Mao Tse-tung as the pre-eminent Communist leader. It is also ironic that the writer gives so

little attention to the elaboration of the governmental apparatus these congresses set up, since work along these lines, as Ilpyong Kim and others have shown, permitted the development of the principles of the "mass line" and other techniques that would carry the Communists to victory in the 1940s, even if they could not stave off Nationalist attacks in 1934.

Waller's study, however, gives sound evaluations of Chinese Communist party politics and struggles of the period from 1930 to 1934, and it is the authoritative work on the government congresses themselves. One hopes for future work by the author and others on the localities represented at the National Congresses not only in Kiangsi but in other areas where the Communists managed to survive against enormous odds in the early 1930s.

The appeal of T. A. Bisson's *Yenan in June 1937: Talks with the Communist Leaders* is quite different. It offers the vicarious thrill of a trip by train taken by the author and three other Westerners from Peking to Sian, and then by a rickety car over flooded streams and roads to the Communists' new capital of Yenan. The adventures of this trip and the return journey to Sian, the continued breakdowns of the car and other hazards of the road, and the ingenuity of the driver (a son of Scandinavian missionaries long resident in North China) in dealing with these problems are constantly diverting.

The other part of the book consists of reports, recorded from Bisson's preserved notes, of interviews with Army Commander Chu Teh, a leader of the "Russian Returned Students," Ch'in Panghsien (Po Ku), Chou En-lai, and Mao Tse-tung. At the time they took place, just two weeks before the Marco Polo Bridge Incident of July 7, 1937 (which began World War II in China), these interviews would have illuminated the Chinese Communist movement, but thanks to the work of Edgar Snow, then writing *Red Star over China*, of Nym Wales, and of many others, there is little new in them now.

Yet, in the wake of the death of Chou En-lai, these interviews are further proof of the brilliance and confidence of the top leaders, who correctly foresaw the imminence of war with Japan and who had devised a tactic that would ensure the growth of their power, while temporarily subordinating themselves to the Nationalists in a new united front against Japan. This point contains several erroneous statements, perhaps based on what Bisson was told at the time—that Chang Kuo-t'ao, who would be expelled from the Chinese Communist party in April 1938, opposed formation of the second united front with the Nationalists as a betrayal of Communist principles. If anything, Chang was expelled, as he later said, for the opposite reason—that he accused Mao Tse-tung of not

making enough concessions to the Nationalists and of being interested only in expansion at the expense of both the Nationalists and the Japanese. Finally, the excellent and little-known photographs of the Communist leaders and various scenes of the trip are alone sufficient cause for the publication of this book.

JAMES P. HARRISON
New York, New York

DAVID M. LAMPTON. *Health, Conflict and the Chinese Political System*. (Michigan Papers in Chinese Studies, number 18.) Ann Arbor: Center for Chinese Studies, University of Michigan. 1974. Pp. xii, 149. \$3.00.

David Lampton's theme in his study of the politics of health care in the People's Republic of China is simple: "Scarce resources must be distributed. In a socialist society, there is no entirely satisfactory way to accomplish this. As a consequence, policies have generally reflected the prevailing range of pressures, many of which are beyond the elite's control" (p. 17). Such a hypothesis, even though it seems only common sense, needs to be stated and demonstrated. Too many Westerners have taken statements of goals in the Chinese media for statements of accomplishments. Too many writers have portrayed a monolithic China run by a few decisionmakers; or, with the revelations of factionalism during the past decade, scholars have sought one overall model of political conflict in China since 1949.

Lampton starts with the assumption that Chinese leaders wish to make the vital health services available to all citizens; he then analyzes the implementation of this goal and illustrates the limitations on policy alternatives and the consequences thereof. For example, the problem of leader-mass linkage became acute during the Great Leap Forward because middle-level cadres heading the communes had few ties with either the local populace or the professional organizations. The cadres were thus not always responsive to local health concerns, and they lacked the professional contacts to appreciate some of the medical consequences of their policies. In this vein, Lampton points out that the proliferation of barefoot doctors resulted in a multiplication of referrals to urban hospitals and thus overburdened the professional physicians and the medical facilities. Under the category of fragmentation of authority Lampton discusses three health policy-making arenas: the Ministry of Public Health, the Nine Man Subcommittee on Schistosomiasis, and Commune Party Committees. The policies of the three differed because of dissimilar responsibilities and also

because of variations in their resources, perceptions, and values.

Despite conflicts and limitations and a succession of policy-making systems that made no one model adequate, Lampton concludes that the health care bureaucracy has embodied a relatively broad range of social interests. He notes that the problems under discussion are characteristic of a developing medical system, and this in itself is evidence of significant progress in the delivery of health care.

One criticism must be made: the work is composed of three separate essays, and the result is considerable repetition. This duplication, combined with Lampton's penchant for summarizing each section, leads the reader to a feeling of *déjà vu* by the time he reaches the third essay.

JESSIE G. LUTZ
Rutgers University

CYRIL E. BLACK *et al.* *The Modernization of Japan and Russia: A Comparative Study*. New York: Free Press. 1975. Pp. xiv, 386. \$15.95.

For many years one of the most satisfying seminars in my course on the world economy has been the comparison of modern economic growth in Japan and Russia. I opened this book, therefore, with some excitement, and I was not disappointed when I closed it. What distinguishes this study, aside from the expertise of the eight authors, is their achievement, after protracted exchanges, of an agreed structure. This is not a loosely coordinated group of learned essays. It is a quite coherent book. Modernization is defined as both the capacity to accept progressively the fruits of modern science and technology and the societal changes wrought by that process.

The analysis proceeds along a horizontal and a vertical axis: three stages of modernization take us through time; five criteria permit systematic cross-sectional comparison of the two societies for each stage. The three stages are: the premodern past as it contributed to (or frustrated) the preconditions for modernization; the transformation, embracing, in my vocabulary, both the take-off and the drive to technological maturity; and "high modernization," when the full range of modern technologies have been more or less efficiently absorbed and, in one degree or another, the mature industrial system provides the gadgetry (as well as services) of mass consumption.

The most original portion of the analysis covers the first stage. It is argued at length and with subtlety that Japanese and Russian history had, over preceding centuries, prepared their societies rather well to react positively to the shocks of Commodore Perry and the Crimean War. Prior

centuries of effective national government and the experience of adapting much from China and Byzantium, respectively, are particularly emphasized.

Similarities and divergences in the transformation and high modernization emerge clearly, although more might have been made of the differences in pace and character of nineteenth-century land reform, and I found the economic comparisons excessively over-aggregated. Moreover, the stage of transformation is a bit too long and complex to be dealt with comfortably in a single sweep.

The greatest puzzlement, for authors and readers, is the question of the general lessons that can legitimately be drawn from the story of the modernization of these two remarkable nations. The authors are, properly, uneasy and indecisive as they look over their shoulders and comment on China's delay of almost a century in entering the transformation. And the broad concluding lessons toward which they lean (without dogmatism) might have been modified if they had considered some other nations which entered the transformation at about the same time as Japan and Russia, for example, Sweden, Italy, Hungary, and Canada.

But on balance this is a model in comparative historical analysis which ought soon to be available for students in paperback.

W. W. ROSTOW
University of Texas,
Austin

HERMAN OOMS. *Charismatic Bureaucrat: A Political Biography of Matsudaira Sadanobu, 1758-1829*. Chicago: University of Chicago Press. 1975. Pp. xiii, 225. \$10.50.

We now have scholarly English-language monographs on the two powerful ministers who in turn dominated Japan's shogunal government in the last four decades of the eighteenth century: John Whitney Hall's book on the corrupt but innovative Tanuma Okitsugu (1719-1788): *Forerunner of Modern Japan* (1955), and Herman Ooms' new work on Matsudaira Sadanobu, who purged Tanuma's supporters, reversed his economic policies, enforced an austere Confucian moral code, and banned heterodox teachings in the schools.

Ooms argues that the last two of these actions made Matsudaira a "charismatic" bureaucrat who was "the first conscious creator of an 'ideology' for Japan." He justifies this description by redefining "charismatic" so that "a leader can be charismatic through the particular self-perception he has of his own role," without attracting "mass followings." It is much easier, however, to accept what Ooms rejects—Nakane Chie's observation that Japanese

history shows "a conspicuous absence of charismatic leaders" and a conspicuous preference for collective leadership and decision-making.

The strong points in Ooms' account are the sections describing Matsudaira's copious writings, his ethics, his political ideology, and his economic measures. All of these improve our understanding not only of the man but of Japan in his era. However, the sections on political history have several lacunae. Ooms discovered an astonishing document itemizing bribes paid by Matsudaira to Tanuma for an appointment which enabled him to plot Tanuma's downfall. This new evidence tends to complicate, rather than answer, questions Hall raised about that appointment. Ooms does not discuss Hall's questions, or explain why Matsudaira's sister opposed his next appointment: as chief councillor.

Nor is there discussion of Ooms' implicit disagreement with Conrad Totman's portrayal (in *Politics in the Tokugawa Bakufu* [1967]) of Matsudaira as more the agent than the principal in subsequent maneuvers, but an agent who soon became unresponsive to his backers' wishes. Similarly, in an otherwise admirably detailed account of the constitutional crisis over an imperial title, we are not told why the imperial regent opposed his own brother and his nephew, Emperor Kōkaku, in order to side with Matsudaira. Ooms argues that Matsudaira's downfall was due more to the Tokugawa Three Houses' displeasure over this "stern treatment of the imperial court" than to resentment over the sumptuary laws. But he says so little about the motives of the Three Houses or the impact of the sumptuary laws that the conclusion seems inadequately documented.

ROBERT M. SPAULDING
Oklahoma State University

IVAN PARKER HALL. *Mori Arinori*. (Harvard East Asian Series, number 68.) Cambridge, Mass.: Harvard University Press. 1973. Pp. x, 535. \$19.00.

This is a surprisingly good book—surprising because biographies of Japanese leaders rarely succeed. Not the least among problems that a biographer frequently encounters is a lack of material on the personal life of his subject, an absence of diaries and personal correspondence that reveal the private thoughts of the person and the sources of his behavior. Ivan Parker Hall encountered this problem, too. "Data relating to Mori's personal life," he admits at the outset, are "particularly thin and disappointing." To a considerable extent Hall compensates for this lack by making imaginative use of Mori's public writings, by providing new and important details of the institutions Mori wanted to reform, and by relying on Mori's flam-

boyance as a public official to give color and interest to the biography.

The background of Mori Arinori (1847–89) sharply contrasted with that of other Meiji leaders. While they were plotting the overthrow of the Tokugawa he was a world apart, ensconced in a Swedenborgian community in upstate New York. He returned to Japan after the Restoration, and, using his knowledge of the West, his language skills, and his Satsuma connections, he gained important positions in the government for the next two decades until his assassination. Among the pragmatic, free-wheeling new leaders he was as innovative as anyone. He founded the first modern philosophic society and the first commercial college, engaged in a Western-style contract marriage, urged adoption of English as the national language, and was the first to suggest abolition of sword-wearing. Mori spent nearly a quarter of his life abroad, including service as Japan's first diplomatic representative in Washington (1871–73) and later as minister at the Court of St. James (1880–84).

Hall provides fascinating accounts of these episodes, but he saves his most detailed treatment for Mori's role as minister of education (1885–89), ascribing to him more influence in this position than any of his successors. The author displays here a refined understanding of the statist and elitist policies that Mori instituted and of their origin in his personality and in contemporary education theory in Europe. In places one may want to cry out at the prolixity of Hall's account, but if the reader will bear with the superabundance of detail he will be rewarded with much important information on the Meiji education system. Hall supplements Japanese scholarship with exhaustive research in Western archives and shows the powerful influence that contact with individual Westerners, especially Herbert Spencer, exercised over Mori's ideas and subsequent policies.

In its meticulous analysis of Mori's utilitarian approach to Western culture, this book makes a substantial addition to our literature on the cultural revolution of early Meiji.

KENNETH B. PYLE
University of Washington

JOHN W. DOWER, edited with an introduction by. *Origins of the Modern Japanese State: Selected Writings of E. H. Norman*. (The Pantheon Asia Library.) New York: Pantheon Books. 1975. Pp. vii, 497. \$15.95.

E. H. Norman became one of the giants of Japanese studies in the West in 1940 upon publication of his classic *Japan's Emergence as a Modern State*. Two monographs followed on the origins of con-

scription in Japan and on the eccentric eighteenth-century utopian thinker Andō Shōeki. The main part of Norman's career took place in the Canadian diplomatic service, not academia. He was ambassador to Egypt at the time of his death by suicide in 1957, a matter generally assumed to be directly linked to allegations made in United States Senate committee hearings that as a student in 1938 Norman had been a Communist.

The core of this book is a long-needed reprint of *Japan's Emergence*. There are also two other of his works that have not previously appeared in English. One is a brief preface, "The Shrine of Clío," written in 1955 for a Japanese-language edition of his essays. It is a gracious and personal statement of his view of history. The other, "Feudal Background of Japanese Politics," was a book-length paper prepared for the Ninth Conference of the International Secretariat of the Institute of Pacific Relations in January 1945. If it had been published at that time it would have been a real contribution to the English literature, particularly on the character of Tokugawa feudalism. Now its interest is mostly historiographic. It is an excellent example of that bleak view of the Japanese past—and for that matter the Japanese present—that a later generation of revisionist historians in Japan and the West brightened up in order to account for Japan's spectacularly successful modernization.

Perhaps the brightening has gone too far. John Dower's introduction, which is the most valuable part of the book, is a long, eloquent essay on the relation between Norman's work and that of his successors among American historians of modern Japan. His charge against the latter is severe: in contrast with Norman's humane concern for the social inequality, authoritarianism, and oppression of modern Japan, they see a past that was not so unbearable after all, moving toward a present and future full of promise. The specific target for criticism is the prevailing point of view of the authors in Princeton University Press' *Studies in the Modernization of Japan* series, a product of the early 1960s when the ongoing dynamic of Japan's social and political development was such as to permit reasonable men to take a positive view of the long-range progress of Japanese history. Dower's critique is to a considerable extent a reflection of the more sober 1970s, when that dynamic seems to have taken a turn for the worse. All seek to explain a particular present in terms of elements selected from history, as Norman had done in the desperate times before and during World War II. Dower does handsomely by Norman in showing and honoring the humanity and depth of his scholarship, but he does ill by the modernization theorists of the 1960s, for their correction of overly negative appraisal of Japanese tradition by a pre-

vious generation of historians was as necessary as Dower's correction of their too cheerful one.

HERSCHEL WEBB
Columbia University

MARK R. PEATTIE. *Ishiwara Kanji and Japan's Confrontation with the West*. Princeton: Princeton University Press. 1975. Pp. xix, 430. \$16.50.

This is the first and, I hope, not the last major biography of a significant Japanese military figure of the 1930s. Ishiwara's career and thought were readily accessible to the historian through a literary output more prolific than is usual with professional military men. Ishiwara was first a theorist, a General Staff Headquarters officer who devised the strategy of the Manchurian operation, developed the dialectic of an apocalyptic Final War, and fostered the organization of the East Asia League. His own tragedy was that, as the author notes, all of his ideas miscarried. Ishiwara's career intersected with three major series of events of the 'thirties: the "Manchurian Incident," the Young Officers' Revolt, and the China war, and in this respect an appreciation of the man is central to the understanding of the 'thirties. Ishiwara's theories were an anomalous amalgam of sophisticated military analysis and mystical revelation, in part derived from his Nichiren faith. His theory of a Final War rested on questionable historical assumptions based on only a slender body of information about his hypothetical enemy, the United States, which he never took the trouble to visit.

In his mild revisionism of the interpretation of Ishiwara as critic of Japan's despotism in China, Mark R. Peattie relies on the work and views of Hata Ikuhiko, an eminent military historian. On the *gekokujo* question, too, Peattie sides neither clearly with the traditional interpreters of Manchuria as the major instance of that phenomenon nor with the revisionists who emphasize the command principle, *dokudan senkō*. In fact, his mention of this controversy seems more an afterthought brought into his conclusions than a carefully developed theme. He contributes an important corrective to John Hunter Boyle's misreading of Ishiwara's hypothetical chief enemy as the Soviet Union. Peattie correctly identifies the antagonist of Ishiwara's Armageddon as the United States.

Without taking the author to task for not dealing with matters he may not have intended to settle, it would nonetheless have been relevant had he devoted some attention, for example, to the system of Imperial Army education of which Ishiwara was a product, and the process of military decision-making that in many instances rejected Ishiwara's theories. The reader might have hoped, too, for a sharper delineation of the differences that sepa-

rated Ishiwara and Itagaki, on the one hand, from General Staff Headquarters superiors, on the other. We might have anticipated, too, that in 374 pages of text the author might have devoted more than a scant 25 pages to the setting and system within which Ishiwara operated—the world of the Imperial Japanese Army. We must at the same time gratefully acknowledge our debt to the author for contributing a well-researched and welcome biography of a fascinating military figure central to Japan's China venture.

JOYCE C. LEBRA
University of Colorado,
Boulder

R. N. SALETORE. *Early Indian Economic History*. Totowa, N.J.: Rowman and Littlefield. 1975. Pp. xv, 859. \$27.50.

This compendium of economic practices and ideas covers two millennia. R. N. Saletore examines a vast body of literary, epigraphical, and numismatic sources and organizes these into chapters devoted to the general structure of external trade, composition and direction of imports and exports, sea and land routes, markets and state control, agriculture, "guilds," and currency and banking. Since a major focus of the volume is on foreign trade, many of the sources are foreign—from the classical West and from China—and some effort is made to compare economic arrangements in all three civilizations. For the field of Indian history, this large work is a unique contribution, valuable especially for its scope, its often erudite judgments on both foreign and indigenous evidence, and for its well-ordered format. To the wider field of ancient economic history—Asian and Western—it provides the most convenient inventory of early Indian economic thought and action.

The volume is a magnificent example of nineteenth-century taxonomic scholarship with the strengths and weaknesses of that genre. There is even something of a nineteenth-century quality in the author. Saletore followed a brilliant university career by an equally successful career as an Indian civil servant (IAS) during which time he published important research on early economic history. He also was a frequent contributor on this subject in the popular English-language press of India. In this he resembles the scholarly administrators of colonial India, whose Orientalist enthusiasms resulted in the critically edited Sanskrit series that comprise the central core of evidence in the present work and whose published research of the late nineteenth and early twentieth centuries comprised a major part of Saletore's bibliography. In Saletore's work, as in that of his British predecessors, the cataloging of traits dominates analysis.

The subject matter of each chapter is organized according to the time period of the specific classes of texts used. This textual periodization is questionable in some particulars (as when early Vedic is given as 2500–1500 B.C.); in general, this periodization has no relationship to phases in the development of the economic order. Saletore gives little attention to the relationship of specific economic arrangements to the larger society or to the causes of changes in these arrangements.

Therefore, as an analysis of the economy of ancient and classical India, this is inadequate. The author completely ignores the work of the Allchins and others on the Neolithic foundations of peasant agriculture in peninsular India and the imaginative scholarship of D. D. Kosambi. Also, the legacy of the Harappan culture (or Indus civilization) is barely mentioned in a two-page discussion of material elements suggesting significant contact between the archaic urban civilizations of Sumeria and India. The author makes no effort to clarify the relationship of early Indian monarchical states and the economic order, a subject that has long been neglected; in the examination of ancient "guilds" (*śrenī*) as corporative institutions, he fails to address large questions pertaining to sastric conceptions about caste with its crucial occupational concern. In failing to consider these and other fundamental issues, the volume yields less than it should, considering the effort and erudition it represents.

BURTON STEIN
University of Hawaii

JAMES MILL. *The History of British India*. Abridged and with an introduction by WILLIAM THOMAS. (Classics of British Historical Literature.) Chicago: University of Chicago Press. 1975. Pp. xlvii, 599. \$20.00.

This abridgement presents James Mill's essential philosophy to general readers and students who might shrink from the full six-volume version. That is its basic merit.

Given the inherent limitations on any abridgement, this one is well conceived. William Thomas (student and tutor in modern history at Christ Church, Oxford) begins with an informative introductory analysis. He provides background and interpretive guidance for casual readers and scholars and shows his own perceptions of Mill, his philosophy and work, and the context in which he operated. This is followed by the editor's equally valuable bibliographical commentary. Finally, Thomas' selections from the formidable *History* are judicious and reasonably balanced. Believing that Mill's central purpose was to demonstrate through his *History* "what he regarded as the most impor-

tant social, economic, and moral thought of his time," Thomas makes his selections to show Mill's general philosophy, omitting lengthy narratives of military campaigns and much of the early history of British activities in India. Ten chapters detail Mill's harsh analysis of Indian culture which he characterized as ignorant, depraved, and backward and, by Utilitarian standards, generally defective. Five chapters represent Mill's philosophic indictment of British domestic institutions and especially of the East India Company's rule in India, chapters in which he offered Utilitarian measures (and Utilitarian Englishmen) to transform India and ultimately England.

For scholars and students of cross-cultural relations, Mill's *History* is an important source, and, on another level, Thomas' abridgement should also be useful. Mill's ideas helped to reverse late eighteenth-century trends toward a modicum of understanding and accommodation between British and Indian cultures and to invoke new British attitudes and policies, characterized by incredible cultural arrogance. These developments produced manifold and often shattering consequences for both India and Britain. For historians and others concerned with cultural interaction, the implications of such developments are compelling.

Students of British intellectual history, British Indian policy, and British perceptions of India in the early nineteenth century should find Thomas' abridgement similarly useful and convenient. For those who want more, Chelsea House Publishers' excellent 1968 reprint provides easy access to Mill's *History* in its entirety.

EDWARD B. JONES
Furman University

R. E. M. IRVING. *The First Indochina War: French and American Policy, 1945-54*. London: Croom Helm; distrib. by Crane, Russak and Company, New York. 1975. Pp. 169. \$11.50.

ROBERT F. TURNER. *Vietnamese Communism: Its Origins and Development*. (Hoover Institution Publications, number 143.) Stanford: Hoover Institution Press, Stanford University. 1975. Pp. xxix, 517. \$14.95.

VO NGUYEN GIAP. *Unforgettable Months and Years (Nhưng Nam Tháng Không Thế Nào Quên)*. Translated and with an introduction by MAI VAN ELLIOTT. (Southeast Asia Program. Data Paper, number 99.) Ithaca: Cornell University Southeast Asia Program. 1975. Pp. viii, 103. \$6.50.

These three volumes provide a valuable though incomplete story of events in Vietnam since World War II. All three have their own points of emphasis as well as shortcomings, but each adds significantly to the ever-increasing literature on the sub-

ject. And while the Vo Nguyen Giap volume might be dismissed as a poorly written propaganda piece, both the R. E. M. Irving and Robert F. Turner books are quality additions in a field where, unfortunately, emotion, journalistic sensationalism, and quantity at any price have long outstripped solid, objective scholarship. This is the real contribution of both these books, and thus I believe they deserve to be classed with some of the works of the late Bernard Fall and Ellen Hammer.

Irving sets out to answer three questions in his book: To what extent and in what ways did French political parties influence Indochina policy? What were the motives behind the policies pursued by France, the United States, and Vietnam in the period 1945-54? In what ways were these three countries affected, both at the time and subsequently by the process of decolonizing Indochina? Irving succeeds admirably in the first, does an adequate job with the second, and covers the third sufficiently with the exception of the United States. In fact, the United States operates primarily on the periphery as an influence on French politics. Here the subtitle of the book, "French and American Policy, 1945-54," does not hold up. Regardless of what the book purports to be, it is actually a political history of French actions in Indochina and an analysis of the impact of those actions on the French political scene, focusing primarily on the Christian Democratic party. In his consideration of that party, Irving is superb as he traces the vacillations in French policy toward Indochina and identifies those key points when the Indochina problem might have been alleviated. He also sorts out the motivations for French policy and presents them clearly and meaningfully. He brings order out of the chaotic French political scene and makes his most valuable contribution by putting Indochina policy into perspective vis-à-vis French political chaos. He has not, however, written about the first Indochina war and has actually plowed little new ground. He ignores the military situation except as it bears directly upon French politics and draws the United States ever closer to active intervention—a policy to which he believes the U.S. was committed even prior to the 1960s.

Turner's volume is certainly one of the most refreshing to appear in several years on the subject of Vietnam, and it rekindles confidence that there is quality work again appearing after an interlude of emotional and severely biased pieces. In fact, Turner's work must rank as a landmark in the treatment of Vietnamese communism. The Communist party that emerges from his pages is not that of the kindly old Vietnamese nationalist, Ho Chi Minh, or the party that has the complete and absolute support of all the people in North Viet-

nam. Rather it is a fledgling party attempting to consolidate its control over an apathetic and often hostile people in the name of nationalism, using all the tricks in the book to do so. Of particular value is Turner's analysis of the North Vietnamese problems with the Communist bloc, as well as with former enemies. He also finds Chinese influence strong in the political formation of the Vietnamese government and the party much less independent of foreign influence than many authors would lead us to believe. Not only is Turner objective and thorough, he has gone into both Communist and non-Communist sources extensively and produced a carefully researched volume. Included as appendixes are many of the relevant documents needed to obtain a complete picture of the evolution of the Communist regime during its rise to power in North Vietnam. The primary weakness of the volume, as with Irving's, is his almost complete lack of treatment of the military and its role in the rise of Vietnamese communism.

Certainly the least valuable of the three volumes is Giap's *Unforgettable Months and Years*. It bears the clear marks of Giap's writing: poor organization, rambling style, and heavy propaganda. If there were any doubt about its being a propaganda piece, such doubt is rapidly dispelled by reading Mai Van Elliott's introduction, which is also sharply biased toward the objectives and purposes of the North Vietnamese. Yet the Giap book is valuable when its many stories of the trials and tribulations of the kindly "Uncle Ho" struggling to help all his people are contrasted with the stark reality of events as seen by Turner and the tremendous political infighting and indecision portrayed by Irving. One must also note the contribution of Cornell University to the study of Southeast Asia in publishing (so far) over one hundred volumes on all facets of Southeast Asian society, politics, and culture.

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JOHN A. LARKIN. *The Pampangans: Colonial Society in a Philippine Province*. (Publications of the Center for South and Southeast Asia Studies, University of California, Berkeley.) Berkeley and Los Angeles: University of California Press. 1972. Pp. xvii, 340. \$13.50.

It is difficult to review John Larkin's monograph, which has been reviewed adequately in several journals some years ago. The task is even more challenging when I remember telling the author (while we were both in Manila in late 1963) that any historical study of a Philippine province was next to impossible and certainly a waste of a graduate student's time. Today this work stands as a

model for young scholars interested in the Philippines. Its publication proves that regional or provincial research is both possible and rewarding. Most older Filipinista scholars, myself included, were trained to see everything from the perspective of imperial Manila, and we rarely ventured into the provinces or municipalities and *barrios* for any understanding of Philippine history. We tended to stay where the research materials were available without excessive physical or financial burden to ourselves. We chose, in many instances, subjects that would include Spaniards, Americans, or Japanese because these regimes tended to have voluminous records, if not always well arranged, kept in Madrid, Manila, Washington, or Tokyo. No one suggested that maybe the history of a region (other than Mindanao and Sulu because they are the homeland of the Filipino Muslims) or a province might contribute to Philippine historiography. In exculpation, my generation was not trained to look upon local history as necessary or rewarding anywhere, including the Philippines.

Larkin traces Pampangan history from the earliest records through the three centuries of Spanish rule down to the third decade (1921) of the American occupation. He not only treats the usual fare of historians, but he also discusses the socioeconomic aspects of Pampanga's existence. What comes forward from his well-researched study is that Pampanga's problems (the rise of a cash-crop economy, later based heavily upon sugar, the migration of Chinese into the province and their intermarriage with the indigenous population, the growth of socioeconomic discontent among the peasantry, and the dominance of local issues over Manila politics) were not Pampangan alone. But scholars will not know, without studies of many more Philippine provinces, to what extent local priorities counted among the provincial elite or how much discontent among the peasantry was attributable to local conditions over which Manila had no control. Perhaps scholars will conclude that the Philippines, whether under Spain, Washington, or President Marcos, is not a highly centralized state.

The author has been told many times that his work presents a new challenge to Philippine historiography. It is good that he persevered through nearly a decade of research and writing.

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JOHN R. M. TAYLOR, a compilation of documents with notes and introduction by. With an introduction by RENATO CONSTANTINO. *The Philippine Insurrection against the United States*. Volume 1, 1571 to May 19, 1898; volume 2, May 19, 1898 to July 4, 1902;

volume 3, *May 19, 1898 to February 4, 1899*; volume 4, *February 5, 1899 to November 13, 1899*; volume 5, *November 13, 1899 to July 4, 1902*. (Eugenio Lopez Philippine History Series.) Pasay City, Philippines: Eugenio Lopez Foundation. 1971. Pp. xxii, 525; xii, 524; viii, 672, 129; xiv, 809; xviii, 829.

The publication of these volumes is an event of considerable importance to Philippine and American historians. The Eugenio Lopez Foundation in Manila has produced a five-volume limited edition of *The Philippine Insurrection against the United States*. The documents included were selected from the enormous body of official papers of the Philippine Republic, papers seized by the invading American Army between 1899 and 1902. The totality of the papers will always serve as the chief source of information about the young nation, its administration and other affairs, and its relations with its Spanish and American adversaries. For this reason the so-called Philippine Insurrection Records represent a valuable addition to our knowledge of the Philippine-American War, and the foundation of United States imperialism in Asia.

As the war reached its conclusion, cartons of papers were shipped to Washington where Taylor began the monumental task of choosing and translating what he considered the items most germane to an understanding of the events of the preceding years. By 1906 he had completed the job and had arranged thousands of translations into 1,500 exhibits. To this compendium he added his own historical introduction that described the Philippine struggle for independence from Spanish colonial times to the final military suppression of the young republic in 1902. And despite Taylor's obvious tendency to portray the United States in the best possible light, his notes and arrangement still prove a useful way into the documents. When he sought to publish his great work, however, he ran into opposition from Secretary of War William Howard Taft, who feared that it might serve as a source of embarrassment for the Republican administration; hence although the work ultimately reached the stage of a third galley, it never became available to the general public. For years it has remained only on microfilm at a few selected archives in the United States and the Philippines.

To encourage the historical investigation of that era, the Lopez Foundation has printed this edition from the microfilmed galley proofs and has distributed five-volume sets to a number of Island and American libraries and to a select group of Philippine specialists. Chief editor Renato Constantino has drawn upon two of the three galley versions in carefully preparing this edition, and he has meticulously added a list of the errata he and his staff have corrected. Moreover, he has kept the galley

numbers for those who might wish to check the current edition against the proofs. In spite of its length, the work is remarkably free from flaws, except for the misplacement of a small number of exhibits that had to be added to the end of volume 5. The Lopez Foundation certainly merits our gratitude for this fine scholarly effort. Though the set has not been offered for sale, it can now be found in many major university and public libraries.

Finally, scholars should note that these five volumes contain only a minute portion of the captured papers. The printed text came from galleys on nine reels of microfilm; an additional 643 reels of largely unedited, unclassified, and untranslated materials also exist. While Taylor selected judiciously, he tended to favor those documents of national and international importance. Much material pertaining to provincial- and village-level affairs lies buried in the confusion of the unused portions. In recent years the original records have been returned to the Philippines where they are being rearranged at the National Library in Manila. However, given the fact that with each move and each new handling some papers are lost, it is probable that the 643 reels of unsorted papers located in the National Archives in Washington remain the most complete, if the most cumbersome, collection of documents. The Taylor distillation, now more readily available for the first time, provides an excellent introduction to this crucial source.

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KHOO KAY KIM. *The Western Malay States, 1850-1873: The Effects of Commercial Development on Malay Politics*. (East Asian Historical Monographs.) New York: Oxford University Press. 1972. Pp. x, 244. \$15.50.

M. C. RICKLEFS. *Jogjakarta under Sultan Mangkubumi, 1749-1792: A History of the Division of Java*. (School of Oriental and African Studies, University of London. London Oriental Series, volume 30.) New York: Oxford University Press. 1974. Pp. xxv, 463. \$29.00.

WILLIAM R. ROFF, editor. *Kelantan: Religion, Society and Politics in a Malay State*. New York: Oxford University Press. 1974. Pp. xvii, 371. \$25.50.

Southeast Asian historical scholarship since World War II has shifted in focus from the regional to the national to the local. Already more than two decades ago scholars discussed and encouraged this direction. Now the results are here, and they are stimulating and welcome, for not only do they provide sharper insights into the motivating forces

of change, but they also lay a new foundation upon which historians of the future can rebuild national histories.

Khoo Kay Kim, recently designated professor of history at the University of Malaya, has analyzed the political impact of the influx of capital and migrant labor from the Straits Settlements, especially Malacca and Penang, upon the nearby Malay states of Perak, Selangor, and Negri Sembilan on the west coast of the peninsula. In the quarter century before direct British intervention in 1874, this influx proved the undoing of the traditional Malay authority patterns, which were unable to provide either the necessary stability or the mobilization of resources that the new circumstances demanded. In willy-nilly fashion they fell into disarray and confusion. Khoo moves the story forward from one state to another in such detail that the reader may be forgiven for skipping names and details. But the main directions are obvious, for out of confusion there must emerge order—capital investment would not have it otherwise—and the age-old flux of Malay politics between local autonomy and central authority moves in the end to the latter pole.

The book about the east coast Malay state of Kelantan, edited by W. R. Roff of Columbia University, focuses upon a more limited area and concerns a slightly later time period, namely from the 1890s to the present. But the main themes of the story it tells resemble those of the Khoo book. Outside authority, first in the form of Siamese administrative reorganization and after 1909 in the form of British "advisers," proves the undoing of the traditional Malay system, replacing it by something that could probably best be described as a neotraditional Malay system. Twelve authors, including local Kelantanese, have each written a chapter. The story is carried forward both topically and chronologically, and the editor is to be praised for the skillful fashion in which he has integrated all the contributions into a comprehensible whole, for while the book is essentially a history it contains contributions by authors in other disciplines. The overall effect is to bring the reader very close to the local scene and into the institutions and society of Kelantan.

The period from 1892 to 1915 witnessed the same incoherence that occurred earlier in the west coast states as traditional patterns crumbled. The chapters of this book take us through a rebuilding of institutions and a restructuring of society that show historical change as something which is much more than a surrendering of the traditional to the modern. Instead, a complicated competition developed among social groups striving for new relationships with each other while each was variously affected by forces of modernization. Tradi-

tional values were used to different ends: some institutions, such as the Islamic religion, could emerge more organizationally powerful than they ever had been, while others, such as the traditional elite, had their status eroded by new social forces. In the search for meaning and value in life the Kelantanese in some ways politicized a fiction and suppressed reality in trying to uphold Malay ideals. This study of social change will not provide easy answers, but the reader will be rewarded with thought-provoking insights into what modernization means in the context of a Malay society.

In the volume by M. C. Ricklefs, who is currently at the School of Oriental and African Studies in London, we confront a different set of values and circumstances. The breakup of the Javanese empire of Mataram plays on a stage of great tradition that relegates the Malay states discussed by Khoo and Roff to the realm of little communities. Using hitherto untapped Javanese sources and skillfully blending these with coeval Dutch accounts, Ricklefs has drawn a revealing and sympathetic picture of the first sultan of Yogyakarta and the way in which he viewed the events of his time and place. At the heart of this story is the dissolution of a once-great empire. The European presence was undoubtedly involved in this decline, but Ricklefs makes clear how very peripheral this presence was to the Javanese participants. Malay states of the eighteenth century were still masters of their own destiny, or thought they were, and as a consequence dealt with Europeans in a manner quite different from what they were to do later. Because their values were still vital, their written records embody cultural values that are difficult to translate into clear expositions of intent and purpose, but Ricklefs manages this feat with skill and beauty. The Javanese tradition has found its modern historical expression in this volume.

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J. D. LEGGE. *Sukarno: A Political Biography*. New York: Praeger Publishers. 1972. Pp. ix, 431. \$10.95.

John Legge's temperate and tentative assessment of Sukarno is an accomplishment of the first order in modern Indonesian history, and in Southeast Asian and postcolonial Afro-Asian history as well.

Legge knows Indonesia and the Sukarno record, and he makes sensible and supple use of the scholarly context built up by the students of Feith, Benda, Geertz, Dahm, and Kahin. In addition to monographic material, he uses the Indonesian press, Sukarno's writings and speeches, and interviews with many who knew the Indonesian leader. Legge has not intended to write an exhaustive

biography, but he has asked significant questions and answered them with care and dispassion.

How much of a revolutionary was Sukarno, as distinct from a dynamic rhetorician? How much a unifier was he, as distinct from an accomplished balancer? How much a modernizing statesman was he, as distinct from a Javanese traditionalist? What made him tick? Sound answers require sensitivity to differing conditions and opportunities in the four decades of Sukarno's public career. Legge's interpretation (for example, pp. 14-15, 319-20) emphasizes three principal periods of political creativity. First, in the late 1920s, as the leader of the PNI, Sukarno gave a new direction to Indonesian nationalism. Then came exile. Second, during the Japanese occupation he established himself as the national leader of Indonesia, leading it toward revolution. Then came a figurehead presidency. Third, as the political parties and Constituent Assembly failed their critical tests from 1956 to 1959, Sukarno became the chief architect of "guided democracy."

Legge sees the years of greatest formal power, those after 1959, as ones in which magico-mysticism, along with moral coarsening, prevailed in Sukarno's behavior. He was devoid of substantial thought on how to achieve economic progress or fundamental alteration in the structure of society. The events of September 30-October 1, 1965, shattered the triangle of forces among which he had maintained a manipulative balance.

Legge implies that without the Sukarno of the late 1920s and 1942-45, there would have been a different and much weaker anticolonial revolution. Without the Sukarno of those periods and 1956-59, there would have been a far less successful synthesis of the national idea in a diverse state. In the end, however, the Javanese traditionalist in his makeup outweighed the modernizing statesman. He changed Indonesian history with his syncretic energy and magnetic personality, but his political *mantras* were unequal to the problems of the 1960s.

What made Sukarno run? With this book we can listen closely to the ticking but cannot look inside the case. There will be much to learn if a psychohistorian of the caliber of Erikson, Mazlish, Brodie, or Rogin has the patience to master pre-modern Javanese culture, as well as post-Freudian analytic theory. Meanwhile, we have a first-class political biography of a major leader of the mid-twentieth century.

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R. H. W. REECE. *Aborigines and Colonists: Aborigines and Colonial Society in New South Wales in the 1830s and 1840s*. [Sydney:] Sydney University Press; distrib.

by International Scholarly Book Services, Portland, Ore. 1975. Pp. ix, 254. \$19.80.

DAVID DAVIES. *The Last of the Tasmanians*. New York: Barnes and Noble. 1974. Pp. 284. \$13.50.

W. K. HANCOCK. *Discovering Monaro: A Study of Man's Impact on His Environment*. New York: Cambridge University Press. 1972. Pp. xii, 209. \$12.50.

Sir Keith Hancock has made a highly successful venture into Australian local history. The book is a brilliant study of a six-thousand-square-mile area in the southeast corner of Australia. Composed of grassy upland and alpine watershed, Monaro is one of the few sources of water in the world's driest continent. Investigators usually focus on Monaro's nineteenth-century impact, good or bad, on the Australian environment. Hancock begins with the aborigines, who over many generations adapted the land to their use by burning it off and turning it into grassland. The author speculates, too, on the effects on Monaro of the arrival of animals and plants. Finally, the white man drove the aborigines off the land. Hancock's basic question is, "How has man in Monaro used the land on which he lives" (p. 14)? He calls upon anthropologists, biologists, botanists, geographers, geologists, and other social scientists to speak of the area, and he allows settlers to report for themselves in letters, songs, poetry, and journals on their struggle with the land and the effect the way of life had on them. Each wave of occupants despoiled the land, then were forced to improve it if they were to survive. This book is an object lesson in the use of interdisciplinary studies; it has complexity, variety, and clarity. Through his asides or running commentary, Hancock gives the reader the sense of having a personal tour of this rural area.

R. H. W. Reece's book pioneers in trying to discover the essence of the aboriginal problem in the 1830s and 1840s and the way contemporary philanthropists, squatters, and colonial administrators viewed the problem. Reece admits he is limited by the fact that the only existing documentary material is from white sources, meaning that white perceptions of aborigines dominate the problems of culture contact. He regrets the lack of documentation of aborigines' perceptions. The objective of the Colonial Office in London was to "civilize," Christianize, and protect the indigenous people. How could this objective be accomplished in the outback where aborigines outnumbered settlers and where the white philosophy was that the only good aborigine was a dead one? Reece organizes his answers thematically rather than chronologically. He describes how the sheep station hands ridiculed and bullied aborigines, killed their women and children, and then claimed additional wages or rations for warding off "treacherous at-

tacks" by enraged male aborigines. He treats exhaustively the Myall Creek massacre, placing it in the context of racial conflict, and he notes the roles of Governor Sir George Gipps and Attorney General J. H. Plunkett in remanding the prisoners so that a second trial (which found seven guilty) could occur. There is a long, three-page quotation of Judge W. W. Burton's sentencing of the guilty parties. Reece's footnotes are extremely useful, as is his bibliography. He has made excellent sound piece of research.

Physical anthropologist David Davies' book is quite another matter. He deals with a sad and dreadful topic—the extermination of the Tasmanian aborigines. It is unfortunate that it is not well researched; there are no footnotes (though excessive use of very long quotations), and the bibliography is inadequate. Neither organization nor treatment is nearly as good as Clive Turnbull's *Black War* (1948) or Robert Travis' *The Tasmanians* (1968), and Davies' chapter on the origins of the Tasmanians (ch. 14) does not compare with the brilliance of Geoffrey Blainey's third chapter in his latest book, *Triumph of the Nomads* (1975). Davies' theme is the utter destruction of the Tasmanian aborigines within seventy-five years. In 1802 there were an estimated 20,000 of them. Then the convicts declared war on them. By 1817 there were only 7,000 left; by 1825 only 320. At this point the bricklayer-turned-evangelical protector of the aborigines, George Augustus Robinson, learned their language, lived among them, and persuaded about 195 to move to Flinders Island forty miles north of Tasmania. There he tried to Europeanize them, and there they languished and began to die. He left in 1839 to become Port Phillip protector of the aborigines. By 1848 only 40 were alive on Flinders Island, and they were returned to the mainland to a mission outside Hobart. By 1854 there were only 16; by 1869 the famous Truganina was the only one left, and she died in 1876. The illustrations, photographs, and maps are superb. They brighten an otherwise disappointing book.

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UNITED STATES

HERBERT G. GUTMAN. *Slavery and the Numbers Game: A Critique of Time on the Cross*. (Blacks in the New World.) Urbana: University of Illinois Press. 1975. Pp. 183. \$7.95.

No scholarly work published in this century has agitated the intellectual community as much as

Time on the Cross by Robert Fogel and Stanley Engerman. Heralded long before it was published, and reviewed extensively in print and on television immediately thereafter, *Time on the Cross* implicitly and explicitly attempted to correct "the traditional characterization of the slave economy," revolutionize the study of Southern slave society, and, by its novel methodology, reverse or question every assumption previously held about slavery and the Afro-American in the United States of America. That was an enormous claim not only for the work, but also for the methodology.

The deliberately intimidating format of *Time on the Cross* succeeded in blunting some early skepticism, and the work was duly hailed as a landmark, especially in the popular press. While economists attacked the validity of the model and challenged its assumptions, historians remained rather mild in their observations. Within two years, however, the situation has completely changed, and *Time on the Cross* is coming under attack from all directions. *Slavery and the Numbers Game*, previously published as a review essay, constitutes the most massive attack yet on the assumptions, methods, and implications of *Time on the Cross*. In this book Herbert Gutman has destroyed the mathematical mystique of *Time on the Cross*, punctured its claims of novelty, accuracy, and understanding, examined the past reviews of the work, and begun a new tradition in his critique.

Sometimes with humor, but more often with unmitigated venom, Gutman has accused Fogel and Engerman of egregious errors, fabrication of facts, dubious quantitative data, exaggerated rhetoric, flawed assumptions, faulty inferences, inept research, incompetent reading, confused thinking, vague generalizations, pointless questions, curious answers, unfair treatment of U. B. Phillips, Kenneth Stampp, Stanley Elkins, and others, absurd explanations, and ahistoricity. These terrible indictments, frequently repeated throughout the text, end with the painful *coup de pied* that *Time on the Cross* ends up being "old fashioned" (p. 169).

Aware that these are serious charges, Gutman goes to great pains to substantiate them, not only by reanalyzing the data presented by Fogel and Engerman, but also by introducing new data as well. The purpose is not to quibble about figures, acceptable statistics, or useful sources. *Slavery and the Numbers Game* insists that variations in percentages are irrelevant, that Fogel and Engerman cannot correct their model, and that they ought to discard it: "If the estimates were more accurate and the quantitative data examined more soundly the model meant to explain slave beliefs and behavior would still be inadequate" (p. 169). The central refutation, therefore, is that *Time on the Cross* lacks a historical perspective, that it is ahistor-

ical and cannot yield the interpretations suggested by its authors. Aggregate data, however carefully selected, cannot by themselves explain either individual behavior or individual motivation. Gutman points out repeatedly that the still-camera photographic techniques of Fogel and Engerman negate the inherent dynamism of Southern slave society. Statistics randomly selected by time and place fail to portray the pattern of changes through time and across the diverse socioeconomies of the South. This is particularly true for the Afro-American slave family, the forte of critic Gutman, who devotes nearly one-half of his book to a demonstration of the unacceptability of the conclusions of *Time on the Cross*.

In general, Gutman's arguments are persuasive. He occasionally indulges in overkill, as though it were insufficient merely to breach the dam, the stream having to be diverted above the shattered wall. Two obvious instances are his discussions dealing with occupational structure (pp. 61-78) and the age data on Southern Afro-American mothers (pp. 148-50). The essential argument in both cases is that probate records alone provide an inadequate basis for establishing either actual ages of slaves or their occupations. The analysis is persuasive, without having to resort to the equally inadequate Kentucky Union Army Rolls. Without further qualification, the geographic and economic relationship of Kentucky to Southern plantation agriculture may be no more appropriate than any Southern city as a representative sample of the occupational profile of the enslaved Afro-American population. (This reservation, incidentally, does not hold true for Gutman's use of the military census of former slaves living in Princess Anne County, Virginia, in 1866, which he employed in the slave family discussion on page 105.) Nevertheless, readers of *Time on the Cross* will find *Slavery and the Numbers Game* an indispensable companion volume and guide to the proper study of the South, econometric or otherwise.

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EDWIN R. LEWINSON. *Black Politics in New York City*. New York: Twayne Publishers. 1974. Pp. 232. \$10.95.

In this brief volume Edwin Lewinson surveys the political status of blacks and the black political leadership of New York City from pre-Revolutionary days to 1970. The first half of the book provides a narrative of blacks' attempts to gain political office, patronage, and civil service positions. In the second half of the work, Lewinson devotes chapters to Adam Clayton Powell, J. Raymond Jones, West Indian immigrants, and the civil service.

Lewinson has undertaken a large task given the limited length of this book. He leaves, as a result, little space for analyzing the significance of the political developments he describes. He discusses economic and residential patterns in New York City, but unlike many students of Afro-American history, such as August Meier in his pioneering study *Negro Thought in America*, Lewinson fails to explore the interrelationship of political developments, social structure, and ideology. The chapters on Powell and Jones illustrate this shortcoming. They are mainly narrations without much analysis of the two men's political careers.

Since the primary focus of the volume is upon black political leaders, little room is left for examining the voting patterns of the black electorate. Lewinson frequently misses the opportunity to compare the New York experience with that of other cities and does not fully use the studies of black politics in other cities by Allan Spear, Harold Gosnell, James Q. Wilson, and Martin Kilson.

This study provides the reader with an overview of black political leaders of New York City. It would have benefited, however, from a more analytical approach.

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HERBERT C. KRAFT, editor. *A Delaware Indian Symposium*. (Anthropological Series, number 4.) Harrisburg: The Pennsylvania Historical and Museum Commission. 1974. Pp. vii, 160. Cloth \$4.50, paper \$3.00.

We need many more such studies as these essays on the Delaware Indians. For a hundred years teachers of American history have paid scant heed to native Americans. We have fashioned them after Hiawatha and Nokomis, set them by the Shining Big Sea Water in the ceremonials of the Camp Fire Girls, and watched their star-crossed lovers leaping off jutting river cliffs. We have filled our forests and plains with howling savages dashing out the brains of helpless infants, scalping fathers, carrying mothers to ignominious captivity. We have ignored the fact that it was they who first hybridized maize and believed that their medicine men and women were charlatans, sorcerers, magicians.

There is an abundance of material on these fictitious Amerindians. There is little reliable material available on the Indians as they actually were, especially those Indians living east of the Mississippi in colonial and frontier days before the advent of modern anthropology. There is little information on Indian cultures just prior to contact with European settlers. Though we have a few biographies of Indians such as Pontiac, we

scarcely recognize the names of other Indian leaders.

A *Delaware Indian Symposium* includes papers on the Indian prehistory of New Jersey; European trade relations in the seventeenth and eighteenth centuries; the Delaware language; and Delaware social organization. There is a discussion of the reasons the Delawares went to war against Pennsylvania in 1755, and also a biography of Moses Tatamy, a Delaware diplomat, landowner, and interpreter who played an important role in Pennsylvania Indian affairs during the French and Indian War. The appendix contains a short discussion of the identity of the Unalachtigo and another on Dutch loan words in the Delaware language. Excellent bibliographies accompany each paper.

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ADOLPH L. DIAL and DAVID K. ELIADES. *The Only Land I Know: A History of the Lumbee Indians*. San Francisco: The Indian Historian Press. 1975. Pp. xvi, 188. \$9.75.

CHARLES M. HUDSON, editor. *Four Centuries of Southern Indians*. Athens: University of Georgia Press. 1975. Pp. 177. Cloth \$7.50, paper \$3.00.

Past decades have witnessed the call for greater cooperation between historians and anthropologists in studying the American Indian. More recently Indian scholars have been encouraged to contribute more to the writing of their own history and to help provide the Indian point of view that has so often eluded the best-intentioned white historians and anthropologists. The books under review show progress in these two areas.

The Only Land I Know is a brief but informative study of the Lumbee Indians in North Carolina by Adolph L. Dial, himself a Lumbee, and David K. Eliades, a Greek-American. Both are now faculty members at Pembroke State University, which was a school exclusively for Indians until 1953. The authors argue without convincing evidence for a direct connection between the Lumbee and the Croatan Indians of Raleigh's Lost Colony and then review the full span of Lumbee history with particular attention to the Lowrie Band following the Civil War, a subject examined in greater detail in W. M. Evans' *To Die Game* (1971). More recent political, social, and cultural history includes the Lumbee's challenge to the Klan in 1958 and the controversy over reaction to the militancy of the American Indian Movement (AIM). Based in part upon a large number of oral interviews, this study includes a bibliography but no documentary footnotes or index.

Four Centuries of Southern Indians, edited by the anthropologist Charles M. Hudson, contains papers presented in 1971 at the annual meeting of the American Society for Ethnohistory in Athens, Georgia. Anthropologists wrote the introduction and four essays; five more essays are by historians. James W. Covington, a historian, highlights the difficulties in relationships between the Timucuan Indians and the Spanish and French in the sixteenth century. Douglas W. Boyce, an anthropologist, argues that the Tuscaroras in the early eighteenth century included independent villages as major political units rather than a confederacy suggested by earlier writers. For the American Revolution, James H. O'Donnell, a historian, analyzes the role of major Indian groups in the South, a subject examined in more detail in his recent book. Jack D. L. Holmes, a historian, identifies the major facets of the Indian policy of Spanish governors and agents designed to thwart United States expansion during the 1790s. Re-examining the critical period of Indian removal, Arthur H. DeRosier, a historian, attacks myths found in the Choctaw experience and calls for a more accurate evaluation of the varied conduct of individual officials. John H. Peterson, an anthropologist, focuses on enclaves of Louisiana Choctaws and supplements earlier studies by identifying their success in finding an economic niche for themselves and thereby preserving much of their Indian culture. Building upon earlier studies, Raymond D. Fogelson, an anthropologist, seeks to put in proper perspective the subject of Cherokee sorcery and witchcraft by combining documentary sources with data from informants to give what he terms a "qualitative and inferential" treatment rather than one that is "quantitative and firmly documented." Albert L. Wahrhaftig, an anthropologist, probes social change among today's Oklahoma Cherokees and argues with limited evidence that the major institutional innovations come from the "full-bloods" rather than the "mixed bloods." The final selection assays the broadest theme by noting the national pervasion of racism as related to Indians and Afro-Americans.

This volume makes a valuable contribution to the study of Indians in the South even though the subjects are diverse and brevity militates against full development of some themes. The cooperation of anthropologists and historians is commendable, but continued efforts are needed by scholars in both fields to yield studies that reflect the strengths of both disciplines.

W. STITT ROBINSON
University of Kansas

DMYTRO M. SHTOHRYN *et al.*, editors. *Ukrainians in North America: A Biographical Directory of Noteworthy*

Men and Women of Ukrainian Origin in the United States and Canada. Champaign, Ill.: Association for the Advancement of Ukrainian Studies, 1975. Pp. xxiv, 424. \$27.50.

This first Ukrainian *Who's Who*, edited under the supervision of the head of Slavic cataloging at the University of Illinois Library, represents an ambitious and meritorious effort. In compiling the some two thousand biographies of notable Ukrainians, the editors employed one or more of the following criteria: current positions, duties, and responsibilities; scientific, scholarly, and professional endeavors; cultural, civic, and political activities; and former positions, achievements, and services. The entries are impeccably arranged, following the model of *Who's Who in America*, *The Canadian Who's Who*, and other such references. Most of the biographical data were obtained from the completed questionnaires furnished by the biographees, but the editors secured additional information from other sources.

The directory includes numerous biographies of former members of parliaments and governments of the Ukraine, veterans of the Ukrainian armed forces, scientists, scholars, educators, writers, and artists. One also finds in it a wide range of current activities and achievements among Ukrainians. For example, this book contains the biographies of thirty-five members of the Canadian Parliament, one Canadian senator (P. Yuzyk), one former Canadian minister of labor (M. Starr), two mayors (Winnipeg and Edmonton), one provincial governor (Saskatchewan), and several state legislators; film stars (Jack Palance and Mike Mazurki) and celebrities (Michelle Metrisko, Miss USA for 1964, now solicitor at the Department of the Interior); seven members of the National Academy of Sciences; G. Kistiakovsky, special assistant to President Eisenhower on nuclear power; the internationally famous biologist T. Dobzhansky (who holds eighteen honorary degrees); J. Charyk, the president of Communications Satellite Corporation and the chairman of COMSAT; L. Dmochowski, the leading international authority on cancer research; W. R. Tkach, the physician to former President Nixon; W. M. Lukash, the personal physician to President Ford; T. J. Danusiari, chaplain to President Kennedy; and the champion of the housewives everywhere, Bohdan Hurko, who invented the self-cleaning oven. (I am still attempting to verify the identity of the Ukrainian who invented the pop-top can.)

Aside from the usual minor typographical errors, the most apparent shortcoming of this directory is its incompleteness. It fails to list, for example, some important members of the academic community and the various officials and staff members of the Ukrainian section of Radio Amer-

ica. The fault, of course, may not be that of the editors. As a work of reference, however, this book is invaluable. It also represents a significant cultural and sociological source, for it provides a comprehensive picture of the general tenor, direction, and concern of the sizable Ukrainian community in North America.

ALEXANDER SYDORENKO
Arkansas State University

SILVIO A. BEDINI. *Thinkers and Tinkers: Early American Men of Science.* New York: Charles Scribner's Sons, 1975. Pp. xix, 520. \$17.50.

Traditionally, the history of science and technology is written from the top—a procession of great men, events, and trends. But the middle and bottom levels are gaining attention. Perhaps the best example is Musson and Robinson's *Science and Technology in the Industrial Revolution* (1969) with its uncovering of a wide, popular diffusion of mathematical and chemical knowledge in Lancashire and the Midlands in the early years of the Industrial Revolution. Silvio Bedini wants to demonstrate something comparable for the United States from the earliest British settlements to the Jacksonian period. His model, however, is the late E. G. R. Taylor's work on the British mathematical practitioners, individuals using (or making) instruments of precision or applying mathematics, not only in science and engineering (speaking anachronistically) but also in a variety of economic activities.

Three principal theses are advanced: first, an independent movement of such individuals appeared and flourished in the America of Bedini's study; second, the movement had considerable impact in its day; third, the movement had major consequences for later American developments. Unfortunately, problems of definition and use of evidence render the work unconvincing despite a considerable array of sources. Bedini's subject is the men of "practical science," the instrument makers, mappers, surveyors, and navigators; also included are the teachers of science and "philomaths." Pure natural philosophers are excluded (were there any then?), as well as practitioners of "earth sciences," a weird term used by Bedini for natural history. (The study of geology was rare in America before 1800.) The language used is so vague that it conveys the impression that any user of a compass on land or sea is included. Carrying this form of reasoning to our day, one could describe any person switching on an electric light as a "practical physicist."

The text discloses an almost continuous influx from Britain of individuals, instruments, and literature; it even has a glowing tribute to this contri-

bution of the mother country to what hardly seems an independent movement. Disregarding these facts, Bedini stresses distinctive native practices and a lesser American professionalization. For example, British surveying practices were presumably not suitable to the American forest. Bedini then notes with triumph the appearance of the native work of John Love, *Geodesia . . .*, in 1688. But by 1792, eleven editions of the book appeared in England, indicating to me that the contents were not uniquely American.

The author apparently believes that the British (and Continental) counterparts of his subjects were largely professionally educated and employed. While a few were, Bedini is clearly not considering them in the framework of the seventeenth and eighteenth centuries. He minimizes and disregards the possibility of formal and informal apprenticeships in the United States, as in the case of youngsters learning navigation at sea from older hands. As to formal education, Bedini decries the lack of it despite his own extensive treatment of an active group of teachers largely outside the colonial colleges. The pattern of education in the United States, around 1800, is a colonial variant of that disclosed by Nicholas Hans' book on England, a work not cited here.

With his amorphous definition, Bedini cannot successfully measure the impact of the mathematical practitioners. It becomes exceedingly hard to pose and to answer meaningful questions—intellectual, social, and economic. For example, nowhere in dealing with the instrument makers does the author adequately convey the feel of life in the shop, the social role of colonial craftsmen, or the economic impact of the trade. Instead there is a self-defeating attempt to cram a diversity of types into a "little man of science" mold. A few were scientists and engineers, in our terms. Most were skilled artisans, surveyors, sailors, and so forth, and they merit treatment as such. There is nothing shameful about not being a scientist or an engineer. The approach of this book underestimates the extent to which past and recent knowledge can permeate sectors of a society, becoming part of a vernacular culture with a notable life of its own.

NATHAN REINGOLD
Smithsonian Institution

BRUCE M. STAVE, editor. *Socialism and the Cities*. (Interdisciplinary Urban Series.) Port Washington, N.Y.: Kennikat Press. 1975. Pp. ix, 212. \$13.50.

Much of the historical investigation of American socialism has focused on national leaders and national organizations. These essays by seven scholars broaden the inquiry to the rank and file and to municipal socialism. Four chapters explore move-

ments in Milwaukee, Schenectady, Reading, Pennsylvania, and Bridgeport, Connecticut; two others examine failures in Oklahoma City and Passaic, New Jersey. James R. Green's collective biography of the most effective salesmen of the appeal to reason provides a glimpse of ordinary working socialists; much of the chapter on Reading and part of that on Bridgeport have a similar emphasis. Bruce M. Stave, the author of the piece on Bridgeport, has also contributed an introduction that relates the essays to one another and to other scholarship. A letter of Walter Lippmann, written in 1913, is printed as an appendix and argues that doctrinal failure often accompanied electoral success.

The socialist that emerges in these pages looks quite like other Americans. Salesmen of the appeal to reason shared the rural and small-town experience of their customers and, like urban socialists, came of old immigrant stock. Socialists were literate working people who enjoyed the fellowship of picnics as well as politics; new immigrants, blacks, and women were not usually admitted to the club.

Socialists won or lost elections as a result of local circumstance. They failed in Passaic because they could not enlist Slavic immigrants and succeeded in Milwaukee because of a firm base in the German community and an alliance with organized labor. George R. Lunn in Schenectady and Jasper McLevy in Bridgeport had personal, nonpartisan support. The corruption and inefficiency of opponents sometimes enhanced political opportunity. Socialist campaigners frequently ignored the program of the national party and became even less radical if elected.

Socialist activity at the local level, then, had only a tenuous connection with the Socialist party of America and owed little to Karl Marx. The socialists considered in these essays were mainstream Americans who were linked by their common willingness to ignore the ideological content of their common political label. That interpretation is in part a consequence of the format of this volume; several authors studying discrete events are not likely to produce a unified synthesis. Those who would in the future provide that synthesis will have to take account of these judicious studies.

HENRY F. BEDFORD
Phillips Exeter Academy

JOHN SHELTON REED. *The Enduring South: Subcultural Persistence in Mass Society*. Foreword by EDWIN M. YODER, JR. Chapel Hill: University of North Carolina Press. 1975. Pp. xxi, 135. \$4.95.

Is the South today really any different from the rest of the country? Most students of the region emphasize its "emergence" or "transformation"

during the period since World War II, its increasing resemblance to the other parts of the United States. In 1941 Wilbur J. Cash felt obliged to reply to those journalists and professors who said that the South was a mere figment of the imagination, that it existed only as a geographical division of the United States. He disagreed. The South, he said, is not quite a nation within a nation, but the next thing to it.

The author of the present book hardly goes so far as Cash. But, as the title indicates, the theme here is that of a persistent inner differentness which has survived the spectacular changes of the last quarter-century in the trappings of Southern life. This theme is drawn partly from a wide variety of conventional sources on the history, sociology, and political science of the region. But the core of the work is an impressive set of tables constructed from opinion surveys, most of them taken by the American Institute of Public Opinion and some by the National Opinion Research Center. The tables are supported by well-written, convincing textual analyses and commentaries.

By comparing the answers of sample groups of Southerners and non-Southerners to a series of questions on political, social, religious, and family attitudes, the author discovers a genuine regional state of mind that tends to ignore the usual sociological boundaries. He finds that whether urban or rural, rich or poor, educated or uneducated, Southerners are more likely than others to rely upon religion, parental authority, violence in settling disputes or upholding personal honor, localism in politics, and an outlook of regional defensiveness. This book reveals through survey techniques a Southern mentality quite similar to what the regionalists among the humanistic scholars and observers have already pictured more impressionistically. Hence the title *The Enduring South*.

Although this work does not answer once and for all the question to which it is implicitly addressed, it does provide data-processed evidence of the existence of a sense of uniqueness that, according to the perceptive foreword by Edwin M. Yoder, Jr., the native Southerner feels in his bones.

CHARLES P. ROLAND
University of Kentucky

RENÉ LAUDONNIÈRE. *Three Voyages*. Translated with an introduction and notes by CHARLES E. BENNETT. Gainesville: University Presses of Florida. 1975. Pp. xxii, 232. \$10.00.

For two centuries English, Spanish, French, and Swedes explored the coasts and rivers of Florida, founding and abandoning settlements. Yet few accounts, good or otherwise, in print today describe these activities. The University Presses of Florida

in 1964 published Charles Bennett's volume, *Laudonnière and Fort Caroline*, a documented account of the founding of that permanent European settlement on the northeast coast of Florida. For the present volume, Bennett translated Laudonnière's accounts of his three voyages to Florida and the Caribbean in the years 1562, 1564, and 1565. Appended are a shipping contract Laudonnière made in 1572 and a quite remarkable mutual gift agreement of the same year between the explorer and his wife. A third appendix presents Tom V. Wilder's useful study, "Plant Life in 16th Century Florida."

Though an introduction summarizes the history of the voyages, much fuller biographical material appeared in the earlier volume. Since little is available elsewhere concerning Laudonnière, it is unfortunate that the two volumes are not now published as one.

The accounts of the three voyages presumably were written solely as reports for Charles IX or Admiral Coligny and were not arguments in favor of further French colonization. Laudonnière writes in detail of the discovery of the gold fields in the southern Appalachians, which were the principal source of gold for minting United States coins before 1849. He describes his relations with the Spaniards, the English, and the Indians, none of whom he trusted. Of the Indians, however, he writes as much as did the anthropologist Tonti, who later described the Illinois Indians in *Des Gannes Memoir*. Indian women get far more attention than I have found in other histories, and Laudonnière makes it evident that among certain Florida tribes the women sometimes had considerable status and authority. They were also very good looking.

N. M. BELTING
*University of Illinois,
Urbana-Champaign*

SACVAN BERCOVITCH. *The Puritan Origins of the American Self*. New Haven: Yale University Press. 1975. Pp. x, 250. \$15.00.

Sacvan Bercovitch is a student of early American literature who admires the artistic achievements of Puritans and who emphasizes the formative influence of Puritan ideology on American culture. In this book he explores the rich Puritan imagination for the source of the American preoccupation with the meaning of America. He finds its spring in the rhetoric of New England Puritanism. Bercovitch argues that "the myth of America is the creation of the New England Way" (p. 143).

The author approaches his task by "opening" Cotton Mather's biography of John Winthrop, "Nehemias Americanus," in the *Magnalia Christi Americana* as if he were a Puritan preacher analyz-

ing a scriptural passage to uncover its meaning. In the *Magnalia*, Bercovitch suggests, Cotton Mather created a distinctive concept of the representative American saint that differed from the traditions of medieval hagiography and from the conventions of filiofetism. Mather's rhetoric used Christology to identify the individual with scriptural heroes and used typology to unite the saint's life to sacred history. Thus John Winthrop as Nehemiah was both historical and allegorical, a representative of New England society and a forecast of the sweep of history. Mather used the word "American" in a novel way by making it express the conjunction of sainthood and nationality. The idea of an elect nation became central to the New England Way because Puritans justified themselves by justifying America. Compelled to contrive a rhetoric equal to their sense of mission, they "invented a colony in the image of a saint" (p. 114). Puritan writers could then establish the saintliness of their subjects by identifying them with the communal enterprise. Their contribution to the idea of America is revealed in the persistence of a remarkable rhetorical continuity from Puritans to Romantics. Writers from Jonathan Edwards to Ralph Waldo Emerson drew on a Puritan rhetoric that extolled the representative self as America and the American self as the incarnation of a universal providential plan.

Bercovitch's analysis is subtle and complex. Unfortunately his writing is sometimes discursive and his prose somewhat tangled; at one point the author calls his own argument "this tortuous dialectic" (34n., p. 211). Many historians will feel that Bercovitch forces rhetorical analysis to carry too heavy an interpretive burden. These are weaknesses in what is a brilliant and sophisticated study. A forthcoming treatment of the nineteenth century and Emerson presumably will carry forward the argument so well begun in this book.

GERALD J. GOODWIN
University of Houston

MICHAEL KAMMEN. *Colonial New York: A History*. (A History of the American Colonies.) New York: Charles Scribner's Sons. 1975. Pp. xix, 426. \$15.00.

According to the dust jacket, Michael Kammen's volume has a felicitous style and an urbane approach, a breadth of scholarship and richness of texture, and a judicious balance of fact and interpretation. For once, the publisher's blurbs are accurate.

New York's colonial development is not easy to describe, because it lacks the unity of purpose of a Massachusetts or the commitment to an economic system of a Virginia, both of which consequently

have some homogeneity. New York's hallmark, however, is its heterogeneity; the author's ability to deal with this while structuring a meaningful past for the colony is most praiseworthy.

Kammen has relied heavily upon the contributions of recent scholars who have delved into specific aspects of the colony's past, an indebtedness he acknowledges in a thirty-three-page bibliography organized by chapters (in lieu of footnotes) but which provides a first-rate compendium of older as well as more recent scholarship.

In synthesizing this material, Kammen occasionally lapses into interpretive errors, particularly in the 1664-1710 period. He seems, for example, uncertain about the common law's real meaning in New York (pp. 78, 129), uses various dates for the applicability of the Duke's Laws (pp. 83, 85), confuses the condemnation of "common Barrator" in the Duke's Laws with hostility toward lawyers (p. 130), wrongly identifies Nicholas Bayard as an attorney trained in England (p. 131), and confuses Lord Cornbury's place of imprisonment (p. 158). He also falls for the old canard that, except for Jacob Milborne and Samuel Edsall, "all of Leisler's close associates were Dutch" (p. 124), when actually his council of eight contained four Englishmen, and when six of the twelve close aides arrested with Leisler were Englishmen. Finally, the publisher should be chided for dropping one or more lines at the top of page 157.

Putting such criticisms aside, this volume offers a valuable blend of topical and chronological approaches, beginning with an extensive description of the area's Indian civilization and ending with the decision for independence. It deals extensively with economic, religious, cultural, and societal changes without losing sight of the chronological framework in which these occur. *Colonial New York* fills a long-standing need for an up-to-date, scholarly, and readable survey of a major development in brief compass.

LAWRENCE H. LEDER
Lehigh University

EMORY ELLIOTT. *Power and the Pulpit in Puritan New England*. Princeton: Princeton University Press. 1975. Pp. xi, 240. \$10.00.

In the last ten years the publication of fresh studies of seventeenth-century New England towns and families has made desirable a synthesis of what we know of the New England "mind" and what we are learning of New England society. The "new" social history is still very new and fragmentary. If it develops further, perhaps it will help evoke an original, general, and rounded view of the history of New England in the seventeenth century.

Emory Elliott has written a more modest book,

but he has attempted to use methods and data from several types of studies. In doing so he may alert the interested historian to new possibilities. He is interested in understanding the hold that the sermon had upon the imaginations of New England Puritans. The explanation of the power of the sermon seems to revolve around the relationship of several generations in New England. The founders, according to Elliott, repressed their children, prolonged their dependence, and loaded them down with a burden of shame and guilt for their reputed failure to live up to the standards of the first generation. The founders did this by their harsh child-rearing practices, by denouncing their sons in jeremiads, by withholding land and denying them the opportunity to marry, by keeping them from church membership, and by other unsavory practices.

Second- and third-generation ministers slowly came to realize how destructive these attitudes and practices of the founders were to religion and social harmony. As they came to this realization—especially in the 1680s and 1690s—they redefined the message from the pulpit and provided new hope, both invigorating and strengthening, to the younger generation. They accomplished this in the thirty years between 1660 and 1690 by changing in the sermon from “one dominant archetype—the image of the angry and wrathful God the Father—to another archetype—the figure of the gentle, loving, and protective Christ.”

Elliott devotes a little more than half the book to the examination of the transformation of the sermon. By concentrating on the form and content of the message to the rising generation he offers an interesting, albeit incomplete, explanation of how Puritan culture sustained itself in the second half of the seventeenth century. His analysis of the language of these sermons seems especially valuable. This part of his book is more convincing than the first, which lays too much weight on the still fragmentary findings of recent social historians. It is not yet clear, for example, just how widespread was the practice of fathers withholding land from sons. Elliott may also lean too heavily on Erik Erikson in reconstructing the inner lives of Puritans; his borrowing from Erikson may not reckon with the important differences between seventeenth- and twentieth-century psyches. But Elliott's use of other disciplines is done with an admirable tentativeness, and his conclusions should be received in the same way.

ROBERT MIDDLEKAUFF
University of California,
Berkeley

E. BROOKS HOLIFIELD. *The Covenant Sealed: The Development of Puritan Sacramental Theology in Old and New*

England, 1570–1720, New Haven: Yale University Press. 1974. Pp. xi, 248. \$12.50.

In the years since the publication of Perry Miller's *The New England Mind: The Seventeenth Century*, much of the scholarship on Puritan studies has taken the form of either elaboration on Miller's themes or attempts to refute them. Yet occasionally a work appears that illuminates an aspect of Puritan thought largely ignored by the Harvard scholar. *The Covenant Sealed* is such a book. In it E. Brooks Holifield carefully untangles the complex and often contradictory strands of sacramental theology advanced and debated in English and American Puritan circles from 1570 to 1720.

Holifield opens his study by setting forth the role of sacramental controversy in the fragmentation of early Protestantism and by outlining the attitudes of Luther, Zwingli, Calvin, and later reformers on issues such as the eucharistic presence and the efficacy of the Protestant sacraments of baptism and the Lord's Supper. He proceeds to explain the evolution of early Puritan views, emphasizing the pastoral concerns of the clergy and their integration of sacramental theory with covenant theology. Though their views were permeated with ambiguities and ambivalence, these clergymen believed that sacraments were seals of existing covenantal relationships rather than converting ordinances. Holifield describes the seventeenth-century debates that such ambiguities occasioned both in England and in New England. He is one of a growing number of scholars who is tuned to the importance of viewing the American Puritans in an international context, not just for the first two decades of New England settlement but for the entire colonial period. Indeed, his exposition of the growth of New England sacramental thought argues persuasively that the main lines of the debate between Stoddard and the Mathers were set by English Nonconformist divines. In his concluding chapters Holifield traces a revival of sacramental piety in the late colonial period and suggests reasons for that development.

While the prime focus of *The Covenant Sealed* is sacramental theology, the author is aware of how changes in that area were indicative of shifts in the New England mind that were manifested in other areas as well. One of the strengths of his analysis is his skill in indicating these connections. He notes the parallelism between the increasing symbolism in Puritan sacramental thought and the development of more elaborate forms of tombstone art. Similarly, he relates the increasing sacramentalism of the late seventeenth century to the movement toward a sacerdotal concept of the ministry as recently described by David Hall. Compared to the volume's strengths, its shortcomings are mi-

nor. Holifield shows how the changing sacramentalism of New England Puritans was in part influenced by alterations in their social and institutional needs and circumstances, but he fails to provide a comparable analysis for English intellectual developments. There are also a few minor errors; for example, John Goodwin was not the brother of Thomas Goodwin, nor were they related. But such caveats should not appreciably affect the book's reception, for it is a significant analysis of an important topic in the intellectual and religious life of the trans-Atlantic Puritan community.

FRANCIS J. BREMER
Thomas More College

CLARENCE L. VER STEEG. *Origins of a Southern Mosaic: Studies of Early Carolina and Georgia*. (Mercer University Lamar Memorial Lectures, number 17.) Athens, Ga.: University of Georgia Press. 1975. Pp. xiii, 152. \$6.00.

One finds upon reading this excellent monograph that the first two essays are less satisfactory than the last two chapters. This difference in quality arises primarily from the lack of reliable sources and the necessity of interpreting the kaleidoscopic events that prevailed in the early history of the two Carolinas. Out of the conflicts and confusion, however, one clear theme predominates—the determination of the settlers to assert the rights of Englishmen to self-government. Contrary to the Turner frontier thesis, the demand for the establishment of more democratic institutions in the Carolinas and Georgia had little to do with the influence of the frontier but came instead from England and from notions of the rights and privileges of Englishmen, as proclaimed especially at the close of the eighteenth century in the Declaration of Rights.

This small volume, rich in insights and fresh interpretations, attacks certain "assumptions" of early Southern history. The author maintains, for example, that the slaves were not regarded as ignorant and incompetent because they were gifted tropical farmers, were enrolled in the militia, served along with Indians to defend the colony from the Spanish, and were used as cowboys on the thriving cattle ranches. Before the plantation system arose in South Carolina, the export of deerskins and Indian slaves was the most important commercial activity. The startling development of the naval stores industry after 1705, when Parliament offered a bounty for their production, and not the growth of rice culture, the author thinks, explains the rise in slave importation in early eighteenth-century Carolina. Before the boom in naval stores, relatively few slaves were imported. The coincidence of slavery expansion

with the development of naval stores does not, however, support his thesis except as one contributing factor. I could not discern how the great diversity of nationalities in the population and the turbulent history of the colonies contributed to the rise of the concept of "the South" any more than a similar diversity of population in Pennsylvania or New York imparted "a special quality" to those states. The essay on Georgia, where the sources are more abundant than on the Carolinas, is a little gem.

CLEMENT EATON
University of Kentucky

EDMUND S. MORGAN. *American Slavery, American Freedom: The Ordeal of Colonial Virginia*. New York: W. W. Norton and Company. 1975. Pp. x, 454. \$11.95.

At the heart of this book is a scintillating account of the first six decades of the history of Virginia. The pages devoted to the period between 1600 and 1660 rest upon careful research on both sides of the Atlantic, and they make imaginative use of fresh materials in the county court records. Thoughtfully and critically written, they present a lucid, occasionally witty, analysis of the transformation of a commercial company into a settled society, related to but different from its English antecedents. This part of Edmund S. Morgan's volume will long reward its readers. The flaws in it are minor.

The chapters that precede and follow it, however, create serious problems. They present a novel, challenging, but largely erroneous, interpretation of the relationship of slavery to freedom in colonial Virginia.

The question asked is that which troubled the revolutionary generation. How was it possible to reconcile the dedication to liberty of the great Virginians who participated in the movement for independence with their acceptance of slavery at home? Morgan answers that slavery and freedom not only coexisted, but that the two conditions were locked into a symbiotic relationship in which slavery made possible the freedom Jefferson's contemporaries espoused. The evidence is not adequate to support these sweeping conclusions.

The opening chapters show that Englishmen before Jamestown were not racists; they conceived of a future society with places for both Indians and whites. The arrival of blacks scarcely affected attitudes toward color through Virginia's first half century. The development of tobacco culture and the plantation, the appearance of slavery and racism, and ultimately the emergence of a political system that united the interests of large landowners and yeomen changed those attitudes. The

republicanism toward which Virginians moved in the middle of the eighteenth century was compatible with slavery because the English Commonwealthsmen from whom they adopted it did not scruple to thrust their own poor into bondage.

This ingenious, if tortuous, argument occasionally emits a burst of illumination, but more often is wrong-headed. The inability to define such concepts as plantation, slavery, and race exposes its grave distortions.

"Virginia slaves were introduced into a system of production that was already in working order." That sentence opens a key paragraph pointing out that the substitution of slaves for servants increased the profitability of the plantation system, but required new methods of discipline linked to racial contempt (p. 316). All the terms are questionable. The plantation into which blacks were introduced did not employ gang labor and was, therefore, not the same as that which took form after the slaves appeared. The identical word, plantation, appeared in 1607 and in 1667 and in 1767, but it bore entirely different meanings at each date.

Slavery was a slippery word in the seventeenth century and grew only slightly more precise in the eighteenth when the lawyers set to work on it. The assertion that James Burgh, one of the English Commonwealthsmen, "wanted to enslave 'idle and disorderly persons'" (p. 382) is correct. But his proposal that "a set of press gangs" seize and set to work "during a certain time" all "idle and disorderly persons, who have been three times complained of before a magistrate" (p. 324), was by no means analogous to the bondage of blacks at the time, which in turn differed from the servitude of 1650.

Morgan's meaning for racism is also unclear. He presents no evidence of views similar to those of the proslavery apologists of the nineteenth century. And if he has in mind the belief that Indians and blacks were inferior, brutish peoples, then that belief also existed in the 1580s and applied as well to the Irish.

OSCAR HANDLIN
Harvard University

LESTER B. SCHERER. *Slavery and the Churches in Early America, 1619-1819*. Grand Rapids: William B. Eerdmans Publishing Company. 1975. Pp. 163. \$5.95.

For analytical purposes Lester B. Scherer divides his survey of the relationship of the early American churches to slavery into three parts: the colonial period, 1619-1700, the provincial, 1700-63, and the revolutionary-early national, 1763-1819. Within each division he devotes one chapter to the history

of slavery and then follows with one that focuses specifically on the relationship of Christianity to slavery.

Scherer finds little to distinguish Christian attitudes toward, and treatment of, slaves from those of early American society at large. For the most part, Christians thought of blacks as a people set apart, to be judged by criteria quite different from those applied to whites. He concludes that "one comes away from the materials of the period with the overwhelming impression that the churches generally related passively and permissively to the dehumanization of blacks."

Christian opposition to slavery occurred, but it was located primarily in groups alienated in one way or another from the larger white culture. Quakers consistently proved the most outspoken critics of slavery. In their formative years the Methodists prohibited slaveowning among their preachers, although the force of this prohibition weakened as their numbers grew and as the church became less sectarian in outlook.

Scherer leaves two areas largely unexplored. He says little about black Christianity, an understandable omission given the current state of the historical literature. Further, he does not deal at length with one important question—why most Christians supported slavery while only relatively few did not. The difference was seemingly rooted not only in doctrine but also in the social function of the churches. The matter merits serious attention, for it offers one way to understand better the social role of Christianity in early American society.

M. L. BRADBURY
University of Maryland,
College Park

RICHARD P. GILDRIE. *Salem, Massachusetts, 1626-1683: A Covenant Community*. Charlottesville: University Press of Virginia. 1975. Pp. x, 187. \$8.75.

Richard P. Gildrie's *Salem* is the latest in the genre of town-based, early New England studies that has, over the past decade, altered our view of the Puritan domain. Indeed, Gildrie's basic assumptions are the very product of that genre. Diversity, not homogeneity, marked the section, yet there was a rough ordering to the towns, "a spectrum ranging from the stable, usually inland, agricultural communities where Puritan organicism remained essentially unchanged for decades to the rapidly changing port of Boston where institutions adapted swiftly to the pressures engendered by the town's commercial and political role" (p. ix). Above all, the symbolic language of New England—in this case, that embodied in Salem's covenants—is not a distinct entity to be studied apart

from social realities. It affects those realities and in turn is affected by them.

Gildrie offers nothing spectacularly new. We have seen before (in John Waters' work on Hingham) the divisions between old and new settlers and between settlers from one part of England and from another. We have seen the effects of commercial growth (in my study of Boston), the alienation of parts of a New England township from the town center (in Kenneth Lockridge's work on Dedham), the tendency toward an unequal distribution of wealth and political power (via William Davisson and others), and the elitism, deference, and occasional insurgencies within the towns (Lockridge again). This observation, however, is not a criticism. The insistence that every historian break new ground in every work is the bane of the profession; certainly it unhinges completely the case study method.

If Gildrie has neither offered a new thesis nor overturned an old one, he has in good workmanlike fashion presented the kind of evidence that is necessary for making generalizations about early American society. One might wish that he had paid more (or less) attention to this or that aspect of the town. With his command of the Salem materials, for example, Gildrie might well have contributed to the demographic base being created; there is nothing here but a shallow bow toward the demographers. One might wish, too, for a stronger, keener interdisciplinary stance. An evening with a sociologist discussing status consistency vis-à-vis status inconsistency might well have given Gildrie a new perspective from which to view Puritan organicism. Another such evening—this time discussing "community" as an abstract concept—might have led him to a better understanding of both the communal aspirations of the seventeenth century and the functioning of seventeenth-century communities. As it is, the two are regularly confused. Nevertheless, one comes back to the main point: this is a sound, workmanlike case study.

DARRETT B. RUTMAN
University of New Hampshire

half this volume covers the years from the Civil War to the present. Short bibliographies follow each essay.

Perhaps the most significant problem with this book is that its narrative format frequently obscures the process of change under the guise of chronological continuity. Many of the essays demonstrate the importance of Baltimore in the state's social and political history, yet no chapter is devoted to it. This is unfortunate because the city's social and economic diversity, its wealth, and its growth as a mercantile city had a statewide impact. Indeed, much of Maryland's nineteenth- and twentieth-century political and social history can just as easily be explained in terms of an urban-rural, intrastate conflict rather than as a reflection of national patterns or trends. The treatment of industrialization shows another aspect of this same flaw. While no one would question the utility of a descriptive chapter on this important topic, it is significant that its preindustrial stage needs equally careful analysis. The volume even lacks any systematic study of agriculture. Considering that regional specialization has long been a characteristic of Maryland agriculture and that farming and farm service have brought sustenance to many Maryland citizens, this omission is incomprehensible. Similarly, social groups (such as immigrants and blacks) or trends not neatly fitting into national periodization (such as race relations or urban-rural conflict) are treated episodically and amorously.

Perhaps we can better understand these problems when we appreciate the absence of much creative work in local history. The excellent social demographic studies of colonial Maryland being done by the St. Mary's City Commission or those being conducted by nonhistorians, notably historical geographers, should be represented in such a general history. It is ironic that this state history, given its national emphasis, poorly reflects local history.

WHITMAN H. RIDGWAY
*University of Maryland,
College Park*

RICHARD WALSH and WILLIAM LLOYD FOX, editors. *Maryland: A History, 1632-1974*. Baltimore: Maryland Historical Society. 1974. Pp. xvi, 935. \$12.50.

This narrative history serves better as an introduction to, rather than as the final statement on, our present knowledge of Maryland's past. The ten essayists view events from a national and chronological perspective, placing Maryland in the mainstream of American history; the result is an emphasis upon institutional, social, and political topics. Unlike earlier Maryland histories, over

SANBORN C. BROWN and LEONARD M. RIESER. *Natural Philosophy at Dartmouth: From Surveyors' Chains to the Pressure of Light*. Hanover, N.H.: University Press of New England, for Dartmouth College. 1974. Pp. xii, 127. \$6.50.

At a time when American colleges and universities are facing a forced redirection, particularly in science, this modest volume presents a most enlightening account of how scientific research was conducted and supported at Dartmouth College over a period of more than a century. In a series of short

historical sketches of activities at Dartmouth in the area of natural philosophy, now known as physics, the authors trace the evolution of a science department, dealing not only with the curriculum but with the individuals involved. The work has a telling impact, for the reader is made aware of what happens to the educational process when scholarship is not only discouraged but the search for new knowledge is not supported. It becomes clear that there was relatively no communication between institutions of learning, an isolation arising from the practice of colleges to hire their own graduates. The book is illustrated with twenty-five interesting photographs of the major figures, sites, and equipment relating to the study of physics at Dartmouth.

Following his establishment of Moor's Charity School in Lebanon, Connecticut, Eleazar Wheelock founded Dartmouth College in 1769, moving it to Hanover in 1770. One of Wheelock's students at Moor's, Bezaleel Woodward, graduated from Yale and returned to work with Wheelock. He eventually became a professor at the new college at Hanover and served it in various capacities; he was treasurer, librarian, trustee, president pro tempore, and even contractor in charge of the construction of Dartmouth Hall. There were other figures as well, such as the unflappable John Smith, who became professor of languages and who during his years as tutor recorded the curriculum in natural philosophy as it existed under the college's first two presidents. John Hubbard, who succeeded Smith as professor of natural philosophy and mathematics, proved a much more forceful figure. The teaching of natural philosophy at Dartmouth, however, was a static discipline and rigid in its offerings through the tenure of Ebenezer Adams, who taught until 1833. It was under Adams that the first scientific observations were undertaken at Dartmouth, with the initiation of a "thermometrical register" maintained by his son, and which has been continued at the college to the present time.

With the addition of Ira Young to the faculty as professor of natural philosophy, the science curriculum emerged from the doldrums. Young acquired new science teaching apparatuses to supplement the few outworn instruments. Furthermore, his enthusiastic teaching and the new equipment stimulated interest in natural philosophy to the degree that when an appeal for public support of the college was made in 1841, the largest donation was given to endow a professorship in natural philosophy. With the death of Young in 1858, Dartmouth learned the lesson that one-man departments in small colleges made no allowances for continuity. His successor, Henry Fairbanks, emphasized even more Young's great

vitality. With the appointment of Ira Young's talented son, Charles, the focus of scientific study became astronomy. He succeeded in acquiring better instruments for the support of research astronomy and personally made a number of substantial contributions.

Charles Emerson, who succeeded Young, saw the advent of physics as part of the curriculum and the end of the single-man department at Dartmouth. At the end of the nineteenth century, the department of physics assumed a greater role in original scientific research under the management of Edwin Brant Frost, A. C. Crehore, and Ernest Fox Nichols, who invented the Nichols radiometer and cooperated with G. F. Hull in experiments on the pressure of light.

The authors succeed in maintaining historical continuity in describing the science program from its early pedantic teaching to vigorous research, through the change in productivity from faculty concentration on teaching to administratively supported faculty research. The work provides a vivid re-creation of the austere student and faculty life in a small New England college in the late eighteenth and early nineteenth centuries. Despite its modest size, *Natural Philosophy at Dartmouth* is a valuable record that successfully illuminates yet another dimension of early American science, which will prove of lasting value for the historian and student in this field.

SILVIO A. BEDINI
Smithsonian Institution

L. H. BUTTERFIELD *et al.*, edited and with an introduction by. *The Book of Abigail and John: Selected Letters of the Adams Family, 1762-1784*. Cambridge, Mass.: Harvard University Press. 1975. Pp. ix, 411. \$15.00.

This volume is what publishers call a coffee-table book: a handsome volume for the general reader who wishes to dip into a subject and be entertained and instructed by it without mastering its complexities. To promote a brisk sale, the Harvard University Press publicized the book vigorously and brought it out just before the Christmas season. It was also timed to coincide with the Bicentennial and with the appearance of the television series, the "Adams Family Chronicles." Some of the profits of the volume will presumably be used to subsidize the scholarly edition of the Adams Family Papers now underway at the Massachusetts Historical Society. Therefore, one wishes it well as a commercial venture.

The subtitle of the volume is somewhat misleading, for it contains not only the letters of John and Abigail Adams, but entries from John Adams' diary that illuminate the relationship between the

famous couple. The letters and diary entries are connected by a stream of editorial headnotes. There are no footnotes. Virtually all the material in the volume has appeared in L. H. Butterfield's magisterial editions of the *Diary and Autobiography of John Adams* and the *Adams Family Correspondence*. The scholar will, of course, want to use these volumes rather than the distillation under review. Yet so well selected are the letters in the present volume, so informative are the editorial notes (in which the editors, at times, throw off some of their reserve—so necessary and appropriate in the comprehensive edition of the Adams Family Papers), so charming is the correspondence, that the volume could be used with advantage in an undergraduate course as a way of introducing beginning students to collections of source materials.

As one would expect, there are no revelations in the collection. Abigail at times looms larger than John, not only because the editors choose to stress her accomplishments, but because much of John's wartime correspondence was brief and bland, a necessary precaution because of the vulnerability of letters to spies and the British navy.

John and Abigail Adams have captured the American imagination during this Bicentennial season, owing, in no small part, to the work of Butterfield and his colleagues in meticulously publishing their writings over the past fifteen years. The success this pleasant volume will enjoy is as much a tribute to them as to the Adamses themselves.

JAMES H. HUTSON
Library of Congress

DAVID AMMERMAN. *In the Common Cause: American Response to the Coercive Acts of 1774*. Charlottesville: University Press of Virginia. 1974. Pp. xii, 170. \$9.75.

CAROL BERKIN. *Jonathan Sewall: Odyssey of an American Loyalist*. New York: Columbia University Press. 1974. Pp. xi, 200. \$10.95.

JOHN M. COLEMAN. *Thomas McKean: Forgotten Leader of the Revolution*. Rockaway, N.J.: American Faculty Press. 1975. Pp. xv, 332. \$16.95.

The three books reviewed here are diverse in interpretation, topic, and chronology, but they have a unifying theme: the reaction of Americans to British policy before, during, and after the American Revolution. Carol Berkin's biography of Jonathan Sewall relates the story of a Massachusetts loyalist; John M. Coleman's book is a history of Thomas McKean, a patriot from Delaware-Pennsylvania; and David Ammerman's volume analyzes American response to the Coercive Acts in the years 1774 and 1775.

As a doctoral dissertation Berkin's work on Se-

wall received the Bancroft Award from Columbia University. The prize was deserved, for the author tells Sewall's story well and briefly. If her book has a fault, it is that her narrative at times is almost too truncated; for instance, she could have said more about Sewall's private life. It would appear, however, that Berkin included all pertinent information that her sources yielded—if at times in brief snatches and in footnotes.

Her description of the origins of Sewall's loyalty in his rise from childhood poverty with the assistance of well-placed conservative men is convincing. The author's deftness in handling Sewall's political writing is commendable. The psychological portrait of Sewall as a man who sought to retreat from trouble rather than face it is both poignant and supported by her evidence. Her analysis of Sewall in the years after he went into political exile and lost his prestige and power in Massachusetts society is particularly well written and touching.

Unlike Sewall, the Thomas McKean of Coleman's portrait was rewarded for devotion to his cause. McKean was an important patriot-politician in the middle colonies during and after the Revolution, and Coleman's listing of the positions McKean held attests to the author's argument that he deserves biographical treatment. In some ways, however, Coleman's biography does not fill the need. While generally well written, the book contains some curious stylistic slips that force the reader on occasion to reread sentences and phrases for meaning. The author asserts that this work is a biography, yet in many places he wanders from his subject and discourses excessively on peripheral topics that have little bearing on McKean's life. The book is advertised as being the complete story of McKean, but it stops abruptly in 1780, thirty-seven years before the man died. As a consequence, the reader is left with little feeling that he has come into contact with a real historical figure.

Interestingly enough—since Coleman says he intended to write a biography—the greatest strength of his book (aside from its copious research) is its analysis of Pennsylvania politics in the period leading to the Declaration of Independence. The author deftly shows how divided the colony was on the question of independence and how “radicals” like McKean were forced to manipulate politics to assure “unanimity” on this important matter. It seems clear from Coleman's writing that even as late as June 1776 there was no consensus among Americans on independence. A powerful conservative element at that time wanted to await events in the fond hope that a reconciliation could take place between Britain and its American subjects.

Coleman's point seems to fly in the face of David Ammerman's thesis in his book on the Coercive Acts. Ammerman contends that American resistance to these acts, more than a year before the Declaration of Independence, marked the turning point of the colonies toward revolution. Ammerman says that the striking thing about the American response to British coercion is that a consensus was achieved which cut across political and class lines, reaching a high-water mark in the First Continental Congress, where both conservatives and radicals united in opposition to a number of laws passed by Parliament.

It appears that Ammerman has overstated his thesis, for one need only look at Coleman's survey of Pennsylvania politics to see that American dissension on how to respond to Britain's coercion was not stifled in 1775. Ammerman only manages to show that Britain's attitude toward America had hardened and that it was in London where consensus was achieved on forcing the Americans to yield to parliamentary authority.

The real strength of Ammerman's compactly written book lies in his excellent survey of how the patriots organized the political structures of resistance, first the committees of correspondence and later the powerful committees of inspection set up to enforce nonimportation, nonexportation, and nonconsumption agreements. Such committees were used by patriots, at least throughout 1776, to suppress continuing opposition to their policies from conservative men like Sewall, who were not included in the consensus of 1774.

In looking at these three books, it seems obvious that Americans responded to the Revolution in various ways. It is also clear that the quality of scholarship in Revolutionary history is diverse. Berkin's and Ammerman's books are filled with penetrating insights and are soundly organized. Coleman's biography of McKean also contains some cogent observations, but the quality of the study is weakened by unnecessary and unfortunate digressions that overshadow the subject of the book.

PAUL DAVID NELSON
Berea College

CHARLES S. OLTON. *Artisans for Independence: Philadelphia Mechanics and the American Revolution*. Syracuse, N.Y.: Syracuse University Press. 1975. Pp. xii, 172. \$9.95.

This book is a welcome addition to our historical literature. Philadelphia, the second largest city in the British empire, commanded a leading economic position in the Middle Colonies, and knowledge of its politics is vital to understanding the Revolutionary movement.

The Philadelphia mechanics, colonial America's skilled "blue collar" workers, held a place of high esteem in the city's largely commercial, bourgeois social structure. They became politically galvanized after the Stamp Act and as a political block played an increasingly larger role in resisting British taxation and regulation. The mechanics' interest was heightened by boycotts of British goods which they helped enforce and from which they benefited. By 1776 they were among the chief advocates of independence. After independence they initially supported the radical Constitutional party for the political freedoms it offered; later they backed the Republican party for the economic stability it promised. The mechanics supported the federal constitution because it allowed the new government to inaugurate national economic programs. On this point the Philadelphia counterparts were economic nationalists.

This otherwise good work on the mechanics is marred, because the author raises questions that are unanswered. At times there are puzzling contradictions. He claims that the mechanics were not proletarians, but he does not examine probate, tax, or similar records to establish the point. He says the mechanics favored revolution because they were manufacturers in competition with England's manufacturers. While many artisans were manufacturers, not all were. Why were nonmanufacturers on the side of Revolution? The author scores economic determinism, perhaps justifiably in the case of the mechanics, yet the expressions he uses, together with the quotations from the mechanics, definitely have an economic bias. He says the mechanics were not radical, and yet the reader finds that they often favored radical measures, and often supported Philadelphia's poor, of which there were a growing number. Why? Too many puzzles still remain about this important Philadelphia and national group.

RICHARD WALSH
Georgetown University

ROBERT A. EAST and JACOB JUDD, editors. *The Loyalist Americans: A Focus on Greater New York*. Tarrytown, N.Y.: Sleepy Hollow Restorations. 1975. Pp. xiv, 173. \$12.00.

Here is yet another example of the revived scholarly interest in the loyalists that has accompanied the Bicentennial celebration. The editors of this volume have collected a group of papers originally presented at a conference in Tarrytown, New York, during the fall of 1973. In addition, they have included ten handsome illustrations, a rather cursory bibliography, and the complete documentation supporting the claim of the Philipse family to

the Royal Commission of Enquiry. The essays themselves are of uneven quality and significance.

In a tantalizingly brief opening essay, John Shy discusses the significance of the British military use of the loyalists in the Hudson Valley and suggests a new and fascinating perspective on the relatively pacific consequences of America's Revolutionary experience. But succeeding essays by Catherine Crary and Jacob Judd are weakened by a tendency to become mired in a morass of insecure detail. Furthermore, Crary fails to provide a persuasive argument for the significance of "De Lancey's Cowboys," while Judd's explanation of Frederick Philipse's loyalism is disappointingly superficial. Similarly, William Benton's essay on Peter Van Schaack does little more than repeat an interpretation the author has articulated more fully elsewhere, and, in any case, Benton's treatment pales in comparison to Carl Becker's elegant essay ("John Jay and Peter Van Schaack") written in 1919.

Despite these weaknesses at the book's center, however, the final two contributions are excellent. Willard Randall's analysis of William Franklin is thoughtful and admirably sensitive to the ambiguities in the New Jersey governor's attitudes and character, and the article also contains some provocative insights into William's relationship to his more famous father, Benjamin. The last piece in the book, Esmond Wright's interpretation of the New York loyalists as a cross section of colonial society, provides an appropriate conclusion, embodying perceptive suggestions both about loyalism and the complex character of New York society and politics in the Revolutionary era.

Like most books of its genre, this collection whets rather than satisfies the appetite of scholars, yet it contains some fine original research and, for that reason, deserves careful consideration by historians concerned with the losers in America's first civil war: the loyalists.

DOUGLAS GREENBERG
Lawrence University

DONALD J. D'ELIA. *Benjamin Rush: Philosopher of the American Revolution*. (Transactions of the American Philosophical Society, New Series, volume 64, part 5.) Philadelphia: the Society. 1974. Pp. 113. \$5.00.

Benjamin Rush has a unique position among the revolutionaries of 1776; he was a reformer who saw the Revolution as an opportunity to transform America into a Christian republic. He condemned slavery, helped form the first abolitionist society in America, and donated land in western Pennsylvania so that former slaves could become free-soil farmers. He believed in a liberal education for everyone, and he specifically included women in

his plans because their enlightened influence on society could help make the republic a better place to live. Rush also challenged traditional education, raised questions about intemperance toward liquor, and urged a rededication to Christian principles. He was a deeply religious man, not tied to any denomination, who believed in equality of men, human perfection, and an eternal life.

With such a bent toward reform, Rush should be popular with modern historians and progressive Americans, but he remains obscure and unappreciated. D'Elia's study makes a solid contribution by putting Rush's bright ideas into the intellectual culture of early America, but it fails to bring excitement, clarity, and drama to those ideas and lacks a comprehensive view of Rush's theories. His medical ideas and practices disappear into the background, while his religious, moral, and ethical ones seem to have too much importance. His interests in politics and the new nation also give way to religion or ethics. In the narrative, other philosophers obscure Rush so that his general position as a second-rate hero of the Revolution seems reconfirmed. Intellectual history of this kind, however, is difficult to write; balances, relationships, and explanations require skill as well as insight. This fine study has much in content and analysis that advances knowledge of Rush.

JOHN A. SCHUTZ
University of Southern California

EMORY G. EVANS. *Thomas Nelson of Yorktown: Revolutionary Virginian*. (Williamsburg in America Series, 10.) Williamsburg, Va.: The Colonial Williamsburg Foundation; distrib. by University Press of Virginia, Charlottesville. 1975. Pp. x, 204. \$8.95.

Thomas Nelson is little known, even though he was a signer of the Declaration of Independence and the governor of Virginia during the trying months of the British invasion of that state in 1781. The guides at the Yorktown battlefield point to his house in the town and speak of the selfless patriotism of Nelson, who ordered that artillery fire be aimed at his own mansion, supposing it to be the headquarters of Lord Cornwallis. But in spite of these claims to honor, he is generally assumed to be a rather undistinguished member of the eighteenth-century Virginia gentry.

This valuable biography by Emory G. Evans of Northern Illinois University corrects this mistaken impression and saves Nelson from relative obscurity. It has not been an easy task: Nelson is difficult to understand, and there are apparent inconsistencies in his character and career that have to be resolved.

The Nelsons were not a typical family of the Virginia elite. Unlike the Byrds, the Corbins, the

Randolphs, and the Lees, whose wealth and prominence in the affairs of the colony dated back to the seventeenth century, the Nelsons were newcomers. Thomas' grandfather arrived in Virginia in 1705, prospered, and pulled himself up from middle-class status to the gentry. In contrast to the older aristocracy, the family derived its wealth from trade, not land. But by 1730 the Nelsons were clearly among the hundred or so ruling families.

Thomas Nelson, Jr. is portrayed in this thorough, painstaking biography as a man of considerable ability, especially in public finance, and clearly one of the important shapers of the fortunes of the state. He emerges as a credible human being, occasionally dissipated, inclined to obesity, plagued by poor health, yet enormously energetic and resourceful. In addition to skillfully managing the task of supplying money for the War for Independence, he contributed much of his own money and personal credit.

WILLIAM M. DABNEY
University of New Mexico

RICHARD H. KOHN. *Eagle and Sword: The Federalists and the Creation of the Military Establishment in America, 1783-1802*. New York: The Free Press. 1975. Pp. xx, 443. \$13.95.

Richard Kohn's work focuses on the military, yet it includes social, intellectual, and political aspects of the post-Revolutionary period. He believes that two conceptions of American defense emerged and the structure of the army became "the key issue" (p. 296) of the 1790s. The nationalists advocated centralization, professionalization, and preparation. Their opponents supported a decentralized militia system. These attitudes reflected divergent world views.

To the nationalists a standing army was a dependable institution in a disintegrating world, a symbol of order and stability. Distrustful of democracy and afraid of mob violence, they believed that the real danger lay not in Europe but in American dissent. Events convinced them that Republicans were part of widespread efforts to undermine the American experiment and civilization itself. The New England apocryphal psychology and jeremiad tradition sensitized them to feelings of doom. History was cyclical; every sign of weakness indicated decay. No tradition of loyal opposition existed. They believed that their generation was exceptional, so every event took on extra significance. This all added up to a cast of mind that "drove them toward a pessimistic and malevolent interpretation both of events and of the character of the opposition party" (p. 217). The new army of 1798, conceived as a "bulwark against rebellion" (p. 239), would insure stability. Officers were cho-

sen for their ideological reliability. When the nationalists divided over the question of defense, "the first and only completely political army in American history" was dismantled (p. 267).

Kohn is revisionist yet not polemical. Deep divisions existed in the early republic, and there were continuities between the factions of the 1780s and the parties of the following decade. The military establishment was not primarily a response to need, but was political "in the purest sense of that term" (p. xii). On several occasions the nationalists attempted to use the army to influence the development of government. They had a militaristic streak that appeared only in times of crisis.

Eagle and Sword is an example of the finest in American historical literature. Detailed and often subtle, it is far more complex than this brief essay can indicate. Kohn grounds informed speculation on ten years of exhaustive primary and secondary research. He is aware of Kurtz, Buel, Smith, and others who have written on the period and analyzes his relationship to them. More important, *Eagle and Sword* carefully treats an important and neglected topic in a fashion that illuminates the period as a whole.

RICHARD M. ROLLINS
Ohio State University

DANIEL SISSON. *The American Revolution of 1800*. With an introduction by HARVEY WHEELER. New York: Alfred A. Knopf. 1974. Pp. xvii, 468, vi. \$12.50.

Daniel Sisson charges that in following Henry Adams' interpretation of the election of 1800 as a "pseudo-revolution," historians have "revealed that they failed to comprehend the nature of revolution." Properly understood—and to Sisson this "requires a mind that is somehow attuned to revolutionary times"—that election did not establish "a new mode of political organization, viz., [sic] that of political parties," but a model for revolution. Sisson maintains that "it is this book's contention that when Jefferson wrote, 'The Revolution of 1800 was as real a revolution in the principles of our government as that of 1776 was in its form,' he meant precisely that." To the author, the election also represented "a repetition . . . of the early phases of the French Revolution of 1789."

On what grounds does Sisson accept as literally true an assessment by Jefferson that most other historians view as a pardonable instance of partisan hyperbole? On what basis can he plausibly argue that one of the closest elections in our history, a contest that resulted primarily in a change of men and not of measures, constituted a revolution? He handles such questions by centering on the antecedents of the "revolution" and not its fruits.

Asserting that "those who are revolutionary 'quick-witted'" can detect his "indebtedness to [Bernard] Bailyn's masterful work" on the Revolutionary era, the author announces that he intends "to establish an ideological framework for revolution as it developed during the decade after the Constitution." That ideological framework, it turns out, is the familiar Jefferson-Hamilton dispute. Despite Sisson's disclaimer that he sought "to avoid the black-and-white perspective of those historians who have set Jefferson against Hamilton as two cocks in a pit," he unhesitatingly jumps into the political arena of the 1790s, presenting an assault on Hamilton and a defense of Jefferson and his allies at which all but the more ardent Democratic-Republicans of that period might have blinked. Maintaining that Jefferson "acted to preserve the spirit and principles of the American Revolution," Sisson proceeds to the highly disputable contention that these principles were threatened by the proponents of monarchy led by Hamilton, a New World "Caesar," a man of "conspiratorial mentality," one who was "ready and willing to overthrow the republic." The issue of monarchy "still remained viable," Sisson writes; Hamilton, "blinded by power," "simply would not allow even the *germ of a spirit of resistance*" (italics added).

To summarize all of the sinuous arguments by which Sisson seeks to prove that the election of 1800 was a genuine and not a "pseudo-Revolution" would require a review of essay length. This also applies to his other contentions, of which the following are representative: that "the political system, in the context of its basic ideological conflicts, remained largely unchanged from the early 1770s to 1800"; that a "style of politics, dominated by faction," not party, "continued through the . . . 1790s"; or that Jefferson's presidential campaign in 1800 was a re-enactment of the "old-style politics" of the Revolutionary War era. Apropos of such arguments, one can only say that some of them are based on baffling exercises in semantics while others are startlingly implausible inferences drawn from highly selective "facts."

Had Sisson confined himself to the proposition that the election of 1800 was a revolution in the sense that it represented "a constitutional change of power without violence and bloodshed, a triumph of reason over armed might," his theme would have been unexceptionable, although perhaps too commonplace to sustain a book of 450-odd pages. As it is, Henry Adams still stands.

JACOB COOKE
Easton, Pennsylvania

RUDOLPH J. PASLER and MARGARET C. PASLER. *The New Jersey Federalists*. Rutherford: Fairleigh Dickinson University Press. 1975. Pp. 256. \$13.50.

With this little volume Rudolph and Margaret Pasler join Richard McCormick, Carl Prince, and Herbert Ershkowitz in analyzing New Jersey politics between the time of George Washington and that of Andrew Jackson. Since New Jersey had a strong two-party system and since the state occupied a strategic location between New York and Philadelphia, the subject is an important one. The authors had the chance to examine the shift from the first two-party system to the second and to evaluate the work of other historians on politics during the early national period.

It is unfortunate that the Paslers have not taken advantage of their opportunities. They present evidence to document earlier studies rather than adding much that is new. Like parties in other states, the Federalist party in New Jersey evolved from a faction that managed to win the first Congressional election. Compared with their Republican opponents, Federalists in New Jersey were older, better educated, from better families, and more often engaged in manufacturing. Since their candidates were often from the upper class, the Federalists benefited from deference politics, and as deference faded so did Federalism. It comes as no surprise that opposition to the War of 1812 allowed the Federalists to stage a comeback that year or that the war eventually destroyed the party. It is interesting to learn, however, that Republican merchants outnumbered Federalist merchants and that the national bank and the Jay Treaty had little impact on New Jersey politics.

The Paslers have presented a workmanlike local study, but they have not broken important new ground. Content for the most part to stick to chronology and narrative history, they only occasionally turn to interpretation. Although the volume is well organized and easy to follow, it is not especially well written. There are too many stilted phrases, and there is too much dependence on the passive voice.

DONALD B. COLE
Phillips Exeter Academy

WILLI PAUL ADAMS. *Republikanische Verfassung und bürgerliche Freiheit: Die Verfassungen und politischen Ideen der amerikanischen Revolution*. (Politica: Abhandlungen und Texte zur politischen Wissenschaft, number 37.) [Neuwied:] Luchterhand. 1973. Pp. 394. DM 58.

This illuminating study covers the early years (1773-80) of an age that Samuel Eliot Morison has described as one of the great creative periods of American history. With rare perception of the distinctiveness of American political culture the author leads his reader from the rule of provincial conventions and congresses (chapter 1) through the decision for new constitutions and independ-

ence, through a series of neat sketches of the state conventions, and finally to the meat of his book, an analysis of "rhetoric and reality" in this first period of constitution-making. Though entirely logical, this structure appears on first inspection of the table of contents to promise a static result, something akin to a political science treatment. At the outset, however, the author leaves no doubt about his historical intention and his performance realizes his intent: the sense of movement continues to the final chapter on federalism with its squint toward the Constitution of 1787-88.

This volume is *Ideengeschichte* of the first order, the interplay of ideas and happenings, the interaction between the felt theories of a time and the march of events. Significantly, the epigraph is a Weber aphorism: interests (material and imaginary), not ideas, rule the doings of man, but the *Weltbilder* created by these very ideas often determine the pathways in which the dynamics of interests impel courses of action. The inevitable abstractions in Willi Adams' treatise do not float airily but mesh firmly in reality, in the movement toward independence and the evolving constitutional arrangements as the colonists debate fundamental concerns, each treated in a separate chapter: liberty, equality, property, representation, confederation, and the "general welfare." Moreover, the chapters are tightly knit in their progression toward confederation, the greatest political achievement of the founding fathers (chapter 13).

The rich and distinctive texture of Adams' book sparkles with insights, enhanced by his clean-cut organization. From wide research the author formulates a noteworthy statement (pp. 99-109) on the conceptions of a "republic" and a "democracy" in contemporary rhetoric. At another level he focuses on an anomaly of revolutionary thinking in a concise analysis of the problem of liberty and chattel slavery (pp. 186-90). Here, as elsewhere, his mastery of the secondary literature, which is noted in his bibliographical commentary, informs his conception of the Revolution. This conception is no simplistic schema but one of process, unique in antecedents and experiences, conservative in the Burkeian sense, enlightened in problem-solving methods, but not without ambiguities and inner tensions.

This volume deserves translation in its entirety. In the literature on the American Revolution it will find a place in select company.

AUBREY C. LAND
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FLOYD J. MILLER. *The Search for a Black Nationality: Black Emigration and Colonization, 1787-1863*. (Blacks in the New World.) Urbana: University of Illinois Press. 1975. Pp. xiii, 295. \$10.95.

During the antebellum period, nine-tenths of black Americans were slaves, and free blacks, half of whom were located in the South, suffered intense discrimination and proscription. A major response to these conditions on the part of Northern black leadership was the advocacy of selective emigration, black nation-building, and a pan-black ideology. These themes form the focus of this volume; they have been treated before and are quite well known to scholars. Miller's book, however, has the merit of being the most minutely researched and sharply focused treatment of the subject. The author traces the origins of articulated Afro-American emigration sentiments and interest in Africa among urban New England blacks in the last two decades of the eighteenth century and their growth in the early nineteenth century through the stalwart efforts of Paul Cuffe. The ebb of these feelings in the 1830s resulted from the rise of militant abolitionism and from concerted opposition to the influence of the American Colonization Society, which blacks feared would lead to their forcible deportation. Interest in emigration flowered in the decade before the Civil War under the stimulus of aggravated and intensified discrimination against free blacks and from the leadership of such major figures as Delany and Holly.

Although the author's focus is clear, and though he has shown remarkable diligence in his pursuit of sources, the final product is disappointing. There are, particularly when he is dealing with Africa, too many outright errors, dubious statements, and shockingly anachronistic usages. What is one to make of a scholar writing in the '70s who uses such phrases as "benighted Africans" (p. 26), "degraded peoples of Africa" (p. 38), and "backward heathens" (p. 258) without quotation marks or in any way disclaiming that such are his views? He confuses the names of African peoples with their area of habitation. Instead of writing of the Yorubas of Western Nigeria, for example, he employs the term Yoruba throughout as synonymous with Western Nigeria and writes of the Guinea coast and the Windward coasts without defining them. He is grossly incorrect in stating that in the 1850s the Niger Valley was unknown, that the mouth of the Niger lay undiscovered by Westerners, and that Haiti had a "monarchial" government. The phrase "black Liberians" further shows his weakness in African history.

The author's style is lackluster and sometimes clumsy. There are needless repetitions throughout, and the work is not endowed with much imaginative sympathy. The critical faculty is employed in petty ways; there are frequent references, for example, to alleged contradictions in action and thought on the part of those the author calls nationalist-emigrationists. These individuals could,

more precisely and with little contradiction, be called pan-black nationalists. Not enough appreciation is given to the overwhelming odds they faced and the bold steps they took to meet their enormous problems. In short, this is a useful, workmanlike, but quite flawed volume.

HOLLIS R. LYNCH
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EDWIN J. PERKINS. *Financing Anglo-American Trade: The House of Brown, 1800-1880*. (Harvard Studies in Business History, 28.) Cambridge, Mass.: Harvard University Press. 1975. Pp. xi, 323. \$15.00.

The House of Brown, now Brown Brothers, Hariman & Company in the United States and Brown, Shipley & Company in the United Kingdom, was founded in Baltimore in 1800 by the linen merchant Alexander Brown. At first the firm offered customers the usual range of mercantile services; operated on its own account in the buying and selling, importing and exporting, of manufactures and commodities; and extended its business into such ancillary activities as shipowning. To all appearances the House of Brown was no different from a hundred other enterprises along the Atlantic seaboard. While the rapidity of the firm's growth quickly set it apart from competitors, much more fundamental distinctions were less easily seen: early specialization in commodity markets, particularly cotton; active participation in the financing of foreign trade; the establishment of branch offices on both sides of the Atlantic rather than dependence on commission agents; and a managerial concept admitting to the inner partnership circle only those family members who met the highest standards of ability and integrity. By 1830 the House of Brown had surpassed the Barings in capital resources, and after the demise of the Second Bank of the United States it became pre-eminent in the management of the economy through its oligarchic position in foreign exchange markets. Throughout, but especially in the sixty years from 1820 to 1880, the House of Brown used its power to stabilize and regularize the international trading system by participating in the development of the futures market in foreign exchange, by monetizing commercial paper through the telegraph and transatlantic cable, and by contributing significantly to the form and acceptability of commercial instruments. As cautious and conservative as they were in most things, the Browns seemingly acted out of character by speculating in the specie point market. The truth was that their information about and understanding of price movements were so precise that the risks were more apparent than real.

This is an exceptionally fine study of the financ-

ing of foreign trade. Naturally, the Browns dominate the book in the same way they dominated the evolution of financial forms and mechanisms; however, they are studied not for themselves but as a vehicle for analyzing and explaining fundamental economic processes.

PETER J. COLEMAN
University of Illinois,
Chicago Circle

MICHAEL FELDBERG. *The Philadelphia Riots of 1844: A Study in Ethnic Conflict*. (Contributions in American History, number 43.) Westport, Conn.: Greenwood Press. 1975. Pp. xi, 209. \$14.50.

Michael Feldberg has made an important re-evaluation of Philadelphia's notorious Bible Riots of 1844, when pitched battles between nativists, Irish-Catholics, and the state militia left twenty dead in Kensington and Southwark. In the tradition of E. J. Hobsbawm and George Rudé, with a street-wise toughness that marks the latest urban research, Feldberg traces the conflict to the growing rivalry between Protestant working-class and Irish-Catholic "cultural sub-communities," a rivalry sharpened by intense job competition. His Philadelphia is a pre-industrial city with a tradition of artisan radicalism, climaxed by the General Trades' Union's ten-hour movement in 1835, which skilled native craftsmen even extended to Irish day laborers. These attempts to control the labor market were dashed by the 1837 depression and by the thousands of immigrants who flocked to the steam factories in Kensington, Southwark, and other industrial suburbs. The 1840s saw a bitter retreat to an ethnic solidarity. Fire and militia companies, gangs, and parish churches sustained the Irish, while the native-born joined similar but separate institutions, including evangelical churches, temperance societies, and Philadelphia's American Republican party (ARP). Feldberg's socioeconomic analysis of ARP membership and close reading of party manifestoes substantiate his argument that American Republicans enjoyed a large artisan following that was attracted by campaigns for free banking, hard currency, and "reform" of immigrant involvement in politics. Where Richard Hofstadter, Seymour Martin Lipset, and other consensus historians saw paranoid, anti-Catholic ravings, Feldberg finds fairly level-headed politics designed to give the aspiring craftsman control over his market conditions.

Feldberg is less convincing in attempts to connect ARP ideology with ethnocultural institutions on the ward level. While the party's anti-Catholic propaganda touched off the confrontation about Bible-reading in Kensington's public schools, the subsequent bloodshed was the work of nativist

gangs, probably indifferent to ARP politics, and bands of teenage rowdies at loose ends between a dying apprenticeship system and a factory discipline not yet in gear. Feldberg adds that after the riots, Philadelphia's business elite considered city-county consolidation, along with professional police, to tame the fire companies and youth gangs. We need to know more about these volatile cobblestone elements.

In this serious, patient study Feldberg has focused on a crucial moment in Philadelphia's transformation into a "modern" metropolis and has brought to it a mastery of the crosscurrents that swept the antebellum city. He has returned students of nativism to a consideration of social structure and substantive grievances, a contribution historians must welcome.

JOEL SCHWARTZ
Montclair State College

JOHN F. REIGER. *American Sportsmen and the Origins of Conservation*. New York: Winchester Press. 1975. Pp. 316. \$10.00.

Reiger's brief text considers American sportsmen and their code, wildlife, the fight for forests, the development of the national park concept, the Boone and Crockett Club, and the establishment of a national conservation policy. The book offers the following dogmas: scholars, "instead of ignoring sportsmen, or treating them with contempt," should acknowledge and investigate their role in motivating environmental reforms; conservation began with wildlife and not with the nation's forests; sportsmen, in time, propelled an interest in forests because of the need to preserve the habitat for wildlife, and they also assumed a leading role in creating and protecting national parks; the Boone and Crockett Club, formed in 1887, and not the Sierra Club was the first private organization to deal with conservation; George Bird Grinnell was the originator and synthesizer of conservation ideas who "prepared" Theodore Roosevelt for Gifford Pinchot.

How new Reiger's interpretation rests, in part, on his dates for conservation's beginnings. The sportsman-historian prefers the early 1870s, when *American Sportsman*, *Forest and Stream*, and *Field and Stream* provided communication outlets and a group identity for outdoorsmen. Other historians have seen conservation as an unimportant political movement until the twentieth century. Still others have begun with the philosophers of conservation and their works: Henry David Thoreau and *Walden* in 1854; George Perkins March and *Man and Nature* in 1864; W. J. McGee and his report, "The Relation of Institutions to Environment," to the board of regents of the Smithsonian Institution, May 23, 1896.

The newness of the interpretation continues with an expression that the public does not distinguish between sportsmen and other factors that produce species extinction: habitat alteration and destruction, killing by farmers, and commercial hunting. Yet there is evidence that conservation authorities have noted, without distinguishing special categories, that hunters were partly responsible for the tragic loss of wildlife and that they were mainly responsible for attempts to maintain or re-establish wildlife population. Theodore S. Palmer's *Chronology and Index of the More Important Events in American Game Protection, 1776-1911*, for example, could lead one to suggest that 1776 might well serve as the beginning of American conservation and to conclude that the name of the conservation game has been long known, if not too well played, throughout the nation's history.

Of 316 pages comprising *American Sportsmen and the Origins of Conservation*, less than half of the total pagination represents text. Seventy pages are given to a picture album of sport and conservation, which would have enlivened the reading if the isolated cartoons, water colors, and portraits had been appropriately positioned with subject matter. Ninety-three pages are devoted to notes, a select bibliography, and an index. Here, too, some of the annotations might have been more fairly presented if they had immediately followed, or in some instances been incorporated within, the textual material.

WILLIAM T. DOHERTY, JR.
West Virginia University

HARLOW LINDLEY, editor. *Fort Meigs and the War of 1812: Orderly Book of Cushing's Company, 2nd U.S. Artillery, April, 1813-February, 1814 and Personal Diary of Captain Daniel Cushing, October, 1812-July, 1813*. Columbus: The Ohio Historical Society. 1975. Pp. xiii, 146. \$3.00.

This is a new edition of the orderly book and personal diary of Captain Daniel Cushing. It is mainly concerned with Fort Meigs during the War of 1812. The first publication was in 1944 as volume 11 of the *Ohio Historical Collections*. Since the original publication attracted little notice among historians and is now quite scarce, this new publication is of interest. A bonus is the addition of several maps drawn by officers serving at the fort.

Following various American defeats in the west during 1812 and early 1813, Major General William Harrison realized American success would necessitate a strong western base. That is the significance of Fort Meigs; far enough from the British to discourage casual raids, near enough to the overland supply trails from Ohio, and strong enough to serve as an assemblage and deposit point for potential offensive operations.

Like many among the newly augmented federal officer force, Cushing accepted a reduction in rank from a brigadier general of the Ohio militia to become a captain of the regulars, in his case the Second Artillery. The orders and diary include the period of the British siege of April and May 1813 and the abortive attack of July. In short, this was the period of the first real success for the United States in the West.

ALEC R. GILPIN
Michigan State University

MAJOR L. WILSON. *Space, Time, and Freedom: The Quest for Nationality and the Irrepressible Conflict, 1815-1861*. (Contributions in American History, number 35.) Westport, Conn.: Greenwood Press. 1974. Pp. x, 309. \$13.95.

Relying "primarily on the congressional debates and presidential messages" of the antebellum period and dismissing any sharp distinction between "rhetoric" and "reality," Major L. Wilson has attempted to write an intellectual history of American nationalism and the coming of the Civil War by relating the conceptual elements of space, time, and freedom to the subject.

The book is richly studded with arresting quotations. Most of them reinforce Wilson's argument that Federalists and later Whigs saw the Union in a process of continuous reconstruction; the United States was to experience qualitative improvement through time as it moved across unsettled space. In Wilson's lexicon this is the idea of "corporate freedom," more familiarly known as positive liberalism. Jeffersonian Republicans and Jacksonian Democrats on the contrary were, according to Wilson, advocates of a "federative freedom"—state rights and negative liberalism—assuming that freedom was perfect at the creation of the nation and calling, over time, for quantitative improvement, that is, the extension of the area of freedom.

Out of this dialectical process of conflicting interpretations of the concept of freedom there came the idea of "freedom national"—that individual freedom alone, without slavery, should give form to the new and unsettled part of the Union and ultimately to the entire nation. "Freedom national" was defended as a form of negative liberalism; slave power took the place of state power, the abandonment of which would allow freemen to go on to "unplanned glory." The alliance between "plain republicans" of the North and Southern slaveholders under the umbrella of federative freedom broke down; "freedom national" versus "slavery national" transcended the older debate between corporate freedom and federative freedom, making the Civil War inevitable.

These crucial parts of Wilson's argument are not altogether clear and not entirely persuasive. The

methodology is at fault here: the quotations are never demonstrated to be representative of the individuals or leaders cited; the analysis is narrowly political, with only scant attention given to the works of Lee Benson, Joel Silbey, and Edward Pessen. People's daily behavior, the dynamics of the economy, ethnocultural affiliations and tensions, and religious ideology play little or no role.

While we can agree with Wilson that ideas are "a part of reality" and "have a life of their own," we must severely fault him for failing to attempt to relate political ideology to social context. Ideas are critical to historical analysis because they are ultimately what people act on, but they can no longer be viewed simply as independent entities with no connection to economic or social setting.

Wilson's dialectical framework that the thesis of corporate freedom (Whigs, American system, time) versus the antithesis of radical federative freedom (nullifiers, extreme laissez faire, space) equals the synthesis of Jacksonian Democracy (negative broker state) and, later, republicanism (the more positive broker state: aid, but no direction), is useful, ingenious, even brilliant, but it does not go beyond his 1967 article on the same subject and ultimately fails to explain the alleged irrepressibility of the sectional conflict.

GERALD SORIN
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HERMAN J. VIOLA. *Thomas L. McKenney, Architect of America's Early Indian Policy, 1816-1830*. (Sage Books.) Chicago: Swallow Press. 1974. Pp. xii, 365. \$15.00.

RONALD N. SATZ. *American Indian Policy in the Jacksonian Era*. Lincoln: University of Nebraska Press. 1975. Pp. xii, 343. \$12.95.

ROBERT A. TRENNERT, JR. *Alternative to Extinction: Federal Indian Policy and the Beginnings of the Reservation System, 1846-51*. Philadelphia: Temple University Press. 1975. Pp. ix, 263. \$15.00.

These three carefully researched, narrowly conceived monographs begin in defense of their protagonists' Indian policies. Each then tells a story of failure. Thomas McKenney, chief administrator of Indian affairs from 1816 to 1830, was the major official spokesman for civilizing Indians. This "sincere humanitarian," in Herman Viola's words, "did his best to maintain a balance between national desires and national honor by just treatment of the Indians" (p. xii). Insufficient control over both Indian culture and expanding, acquisitive frontier society led McKenney to abandon hope for civilizing Indians in the East and to support Indian removal west of the Mississippi. McKenney did not contemplate forced removal, however, and

his dismissal by Andrew Jackson signified the new president's intention to drive the southern tribes west despite their own desires.

Ronald Satz begins with Jacksonian removal. Denying Jackson's historic, personal stake in Indian dispossession, ignoring the illegality of extensions of state law over the southern tribes, and exposing the political motives behind National Republican Indian sympathy, Satz initially defends removal. But its implementation, he argues, was disastrous. Removal against tribal consensus meant fraudulent treaties and military force. Removal by private contractors and an economy-minded government meant corruption, starvation, and Indian death. Removal manipulated by sale of individual Indian land allotments made the Choc-taws and Creeks "victims of one of the most flagrant cases of fraud, intimidation and speculation ever recorded in American history" (p. 83). Removal to make room for land-hungry eastern whites failed to guarantee Indians their newly promised land against expansionist pressures in the West. To remove Indians from the midst of a society of "expectant capitalists," writes Satz, was "an acceptable and desirable policy" (p. 54). But since the character of that society explains the character of removal, the distinction between policy and implementation fails.

For the administrators who followed removal, and their historian Robert Trennert, Jacksonian policy was too *laissez faire*. It left western Indians at the mercy of frontier traders and settlers. Efforts to reassert government control, Trennert suggests, led to the reservations policy: confining, controlling, and protecting Indians, in the face of Manifest Destiny, was the alternative to extinction. But postbellum implementation of the reservations policy, Trennert indicates, reproduced the failures of McKenney's civilization and Jackson's removal. "Brutal and disastrous" reservation confinement limited the tribes to smaller and poorer lands (p. 196). These consequences, Trennert insists, were unimagined by originators of the policy.

This lack of imagination, reproducing illusions shattered a decade earlier, was itself an achievement. Policy makers thought the tribes would passively accept white plans because they failed to see Indians as human agents, with purposes and cultures of their own. When McKenney evoked "their helplessness and their dependence on [the president] as their father" (p. 37), his intention was more than descriptive. He wanted to make Indians over into the "children" (p. 176) he described. Indians were victims not just of expectant capitalist society but of a paternalist state.

Viola, in spite of his sympathy for McKenney, pictures a "patronizing and self-satisfied man" (p.

144) who longed for power over his Indian children, opposed Cherokee literacy in the native language, "delighted in tearing apart the fabric of Chippewa culture," and busily rescued Indian relics from, in McKenney's words, "the ultimate destruction which awaits his race" (p. 243). McKenney, according to Viola, also turned the Indian plight to his own promotion and profit. Satz defends Jackson's "great paternalism" (p. 9) early in his book, but he ultimately recognizes that "the paternal hand of the United States . . . undermined tribal power" (p. 126-27). The civilization program in the West, he writes, "reduced the Indians from cultural maturity as members of tribal societies to cultural infancy as civilized men" (p. 277).

Satz shows that Jackson sacrificed promises of Indian autonomy in the west to government political and social control. Trennert is thus wrong to suggest that the reservation policy of "benevolent paternalism" (p. 1) departed radically from a *laissez-faire* removal. Like Viola, Trennert defends "the sincerity of the humanitarian concept" (p. 194). The reservation policy was "ethnocentric" (p. 1), to be sure, but this assertion functions as apology. Nineteenth-century humanitarians assumed that Indians were less than adult humans, and they formed their schemes for Indian betterment accordingly. Trennert insists that the twentieth century not sit in judgment on widely shared assumptions of the nineteenth.

Here the power of positive historical thinking weakens all three of these books. Cultural symbols have histories, too. Nineteenth-century ethnocentrism was not a given but a human project, serving functions for society and the psyche in red, white, and black America. Interpreters of Indian policy must learn from recent studies of white over black to penetrate racial ideologies and not accept them at face value. The alternative is positive history, exculpatory and ahistorical. To be bound by the world view of policy makers, in the name of historical objectivity, is not to think historically.

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RUSH WELTER. *The Mind of America, 1820-1860*. New York: Columbia University Press. 1975. Pp. xvi, 603. \$14.95.

Rush Welter's study of the ideology of antebellum United States suggests the resilience of an older tradition of intellectual history recently eclipsed by the profession's greater social emphasis. Despite claims about writing from "the often inchoate ideas" of "relatively ordinary men and women,"

Welter explores the "American mind" through the words of social and political leaders. The "ordinariness" of the American women quoted (once each)—Catherine Beecher, Angelina Grimké, Julia Ward Howe—is not much different from that of the men. Politicians, with emphasis on those active in state constitutional conventions, ministers, college figures, and social publicists and theorists make up the bulk of the book's sources. If the sources are not different in kind—except for the exclusion of literary ones—from those normally used, they are uncommonly various. One value of the study is as a treasury of quotations from neglected materials.

Rush Welter centers his argument on the conventional dichotomy: liberal or radical Jacksonians versus conservative Whigs. Many of the study's weaknesses are rooted in this structure, especially in the repetition of alleged ideological opponents making identical arguments, and in its tendency to let categories control ideas. A four-page footnote pretending to distinguish Whigs from "conservative" Democrats represents the book's faults at their worst: lengthy quotations that illustrate little—certainly not the distinctions Welter insists on. The book avoids the worst fault of the political-ideological tradition Welter cultivates, the good guys versus the bad guys syndrome. Welter analyzes with sufficient critical vigor, so that both sides turn out to be composed of mistaken, if not bad, guys.

Welter sees Jacksonian ideas as grounded in a concept of "equal rights," which meant hostility to government favoritism or interference. Welter does justice to the liberal impetus and conservative conclusion that grew from Jacksonian hostility to government "privilege" and the toleration of economic and social distinctions deemed "natural" rather than "artificial." Other aspects of his Jacksonian description are less convincing, especially his stress on the party's political "Calvinism" or "fatalism." The antigovernment bias of Jacksonians owed much more to abstract eighteenth-century notions of a just, natural order than to a Calvinistic acceptance of pervasive evil. It was, as the *Democratic Review* said, "a cheerful creed," weakened more by naive hopefulness than any sense of grim human inevitabilities.

The Whigs' argument is less interestingly explored. Their central tenet, Welter says, was a perception of property rights that moved from an organic conception of property as subservient to the social good to a notion of its absolute sanctity. The case, at best, is simplification; that Welter is wrong in asserting that "when a conservative spoke of rights during the 1840s, it was an all but foregone conclusion that he had in mind the rights of property," is indicated in many quotations in

the book and in the author's own later discussion of civil liberties. Nor does he present any real evidence for his view that the Whigs gave up "the vision of society as an organic unity," although democratic sensibility led them increasingly to define this in promotional or reformist rather than paternal terms.

The gist of Welter's criticism of both parties is that their separate ideas led to a joint result: a fear of government power that paralyzed positive action. This conclusion loses much of its bite because Welter recognizes little danger in power. His discussion of Jacksonian views of limited government or Whig emphases on constitutionalism and minority rights show little response to the fact that power in modern societies has often been as destructive as it has been constructive. Welter gives little heed to Democratic stress on the sanctity of majority and individual power, and the way that these themes accentuated Whig concern for both natural rights and legalism as protective devices.

The author incorporates chapters on American views of history, church, school, and frontier that share the substantial virtues and lesser faults of the specifically political material. Themes are often pushed to misleading lengths. His first chapter insists that Americans believed "that the United States had already fulfilled the progressive dreams of mankind;" but the material suggests the more moderate belief that the American Revolution had laid a foundation which, if properly built on, could support a better society than any previously known. Throughout these chapters the book suffers from an abstraction from the social and political realities that gave ideas life. Welter chides Eric Foner for "his tendency to render ideas as the expressions of practical interests or as instruments of Republican polemics," but Foner's emphasis on the way ideas are shaped by, as well as shape, practical reality gives his account of the coming of the Civil War a social and psychological credibility lacking in Welter's work.

Whatever its faults, Welter's study has a breadth, thoughtfulness, and richness of research that commend it to anyone seriously concerned in making sense of the tangled ideological-political web of antebellum United States.

DAVID GRIMSTED
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SHERRY PENNEY. *Patrician in Politics: Daniel Dewey Barnard of New York*. (National University Publications.) Port Washington, N.Y.: Kennikat Press. 1974. Pp. xv, 206. \$12.50.

This traditional biography of Daniel Dewey Barnard traces the response of a patrician Whig politi-

cian to the changing political and social landscape of the Jacksonian era. Born into wealth, Barnard was trained as a lawyer, served four times in Congress from upstate New York, was a chief contributor to the *American Whig Review*, and headed the American mission to Berlin during Fillmore's administration.

From his first involvement in politics until his death in 1861, Barnard maintained a conservative philosophy that included a belief in slow, orderly growth regulated by a political leadership composed of superior men of intellect, ability, morality, and wealth. He disdained popular mass politics and had a passionate interest in preserving the Union.

Sherry Penney explains Barnard's actions, however inconsistent, in terms of his devotion to these beliefs. For instance, although he vigorously opposed social reformers, Barnard was a fervent supporter of educational reform. He argued for state funding of common schools in order to teach the masses due respect for order, stability, and the proper virtues. Despite ruminations about the horrors of mass politics which kept him at odds with seasoned politicians within his own party, Barnard campaigned for Harrison in 1840 and urged that patronage be used whenever possible to satisfy the Whig interest—not simply to pay off political debts but to place in power principled men like himself. Not surprisingly, Barnard vigorously opposed Andrew Jackson and the Democrats. Dedicated to Clay's American system, he saw Whiggery as the best hope for America's future. Yet in the 1850s Barnard supported Democratic candidates in an effort to destroy the Republican party and preserve the Union.

The conceptual framework employed in this study is hardly novel. Undue emphasis is also accorded Barnard's perceptiveness and influence. To note, for example, his prediction in the 1850s that the Civil War was inevitable as an illustration of the man's prescience is not convincing. Nevertheless, Penney's account helps illuminate the reactions of a certain class of individuals to the convulsions of antebellum America.

PETER LEVINE
Michigan State University

GILBERT GIDDINGS BENJAMIN. *The Germans in Texas: A Study in Immigration*. Austin, Tex.: Jenkins Publishing Company. Reprint. 1974. Pp. 159. \$12.95.

While a doctoral candidate at Yale between 1904 and 1907, Gilbert Benjamin researched and wrote *The Germans in Texas*. The work was originally published by the University of Pennsylvania Press in 1909; the present reproduction is by photo-offset with only the title page having been replaced.

The decade before World War I was not only famous as the most expansive years in American immigration history, but it was also an era in which sociologists, economists, and special investigators attempted to explain the phenomenon. A score of influential and sometimes controversial accounts were published. Those by Prescott F. Halls, Edward A. Steiner, John R. Commons, Jeremiah W. Jenks, Peter Roberts, and Henry P. Fairchild were read not only by scholars, but also by government officials, congressmen, and the general public. Benjamin, however, was little influenced by these widely discussed and popularized sociological studies. The era also became known for a growing filiopietism. One of the more famous pro-German books was Albert B. Faust's massive two-volume account, *The German Element in the United States*, published the same year as *The Germans in Texas*. In his Texas study, Benjamin made no special claims for the German migrants and did not reflect the nationalistic bias prevalent in many immigrant stories. By the turn of the century a number of literary observers were using fiction to provide a meaningful insight into America's immigrant heritage. But even the repeated appearance of the Teuton in works like those of Sidney Lanier, William Dean Howells, Henry James, and Berthold Lindau seemingly had limited impact upon Benjamin's historical evaluations.

In short, *The Germans in Texas* is a detailed and factual historical monograph, a brief and somewhat restricted account of both individual and group settlement during the first half of the nineteenth century. Benjamin surveys the German response to slavery, secession, Reconstruction, and the Indians, and their contributions in establishing newspapers, singing societies, literary clubs, and churches is surveyed. While not engagingly written or philosophically expansive, the study is carefully researched, historically balanced, and nonpropagandistic. It has been used as an authoritative reference by immigrant scholars (Carl Wittke often refers to Benjamin) and perhaps will find a new audience with the current revival of German festivals in Texas.

Unfortunately, this reproduction provides no introduction, updating, editorial comment, or evaluation of the book, the author, or recent immigrant studies on Texas settlement. A brief note on the dust jacket declares the work to have been "researched at a time when many of the original leaders and participants were still alive." After nearly seventy years *The Germans in Texas* deserved a more explanatory and reflective second launching.

WILBUR S. SHEPPERSON
*University of Nevada,
Reno*

MARK E. NACKMAN. *A Nation Within A Nation: The Rise of Texas Nationalism*. (Series in American Studies.) Port Washington, N.Y.: Kennikat Press. 1975. Pp. 183. \$12.50.

In this work, Mark Nackman maintains that Texas remains a "nation within a nation." A strong and persistent sense of identity was generated by the common hardships of the republican period. Indian harassment and the specter of Mexican reconquest characterized this era, yet many Texans viewed annexation with reluctance. Exaggerated claims to Santa Fe and renewed interest in the republic as an alternative to secession were other manifestations of this nationalism. Nackman concludes that the annual celebration of Texan independence, the annual proclamation of Texas History Week, and the ever-present symbol, the five-pointed "Lone Star," indicate that the virus is still rampant.

Nackman is correct when he emphasizes that since most immigrants were fleeing economic ruin and later prospered in Texas, they developed a fierce attachment to their adopted land. Like all who write about Texas history in the early period, the author is intrigued by the high suicide rate among persons of prominence, and he speculates on the possible "death-wish" of Travis at the Alamo and Fannin at Goliad. Their martyrdom and that of the men who willingly followed them also fostered a powerful sense of patriotism and cohesion among Texans of later generations. The author is perceptive when he observes that nationalism has never had much attraction for the black and Mexican residents of Texas.

On balance, this is a worthwhile book, but the writer's conclusions are labored and overstated, and furthermore, I do not believe they are peculiar to Texas.

STANLEY E. SIEGEL
University of Houston

LUCILLE WRUBEL GRINDHAMMER. *Art and the Public: The Democratization of the Fine Arts in the United States, 1830-1860*. Stuttgart: J. B. Metzlersche. 1975. Pp. 86. DM 20.

In this brief, 25,000-word monograph, published in Germany but based on her American dissertation, Lucille Wrubel Grindhammer surveys the various methods employed to popularize the fine arts in antebellum America. She focuses on "the newly emerging mass media," the illustrations and articles that publicized artistic achievements in newspapers, magazines, gift books, and fiction, as well as on the work of the immensely popular art unions, which distributed engravings and original art in the 1840s and 1850s. Another sign of spreading interest, according to Grindhammer, was the creation of art and drawing classes in American

schools and colleges; the "educational efforts did turn around the culture's formerly negative attitude toward art." She concludes that the origins of contemporary mass culture can be found in the Jacksonian era and in the encouragement that printed media gave to hundreds of American painters, sculptors, illustrators, and engravers.

The study brings together much useful information and demonstrates bibliographic mastery. However, the conclusions present little that is new, and the brevity of the volume forces a certain level of reductionism into the cultural analysis. That periodicals, publishers, and other patrons supported and stimulated American artists is undeniable, but one wishes to know whether they increased understanding of or sympathy with esthetic philosophies and techniques, whether they developed a permanent mass clientage for the fine arts, and whether they truly originated a mass culture, for mass culture is structurally and functionally quite different from the popularization they aided. Grindhammer is less successful in answering these questions. Finally, there are some errors in the references to artists, including a series of misspellings.

NEIL HARRIS
University of Chicago

NORMAN D. BROWN. *Edward Stanly: Whiggery's Tarheel "Conqueror."* (Southern Historical Publications, number 18.) University of Alabama Press. 1975. Pp. viii, 365. \$10.00.

Arthur Charles Cole called him "one of the ablest champions of whiggery in Congress," but North Carolina's Edward Stanly, who served ten years in the House of Representatives from 1837 to 1853, gained his greatest fame in that body as a result of his personal disputes—sometimes verbal and sometimes physical—with fellow lawmakers, Democrats and Whigs alike. In 1851 he engaged in "the last duel in the United States arising out of congressional debates."

As a congressman, Stanly was difficult to categorize. Despite his "occasional gasconading" on the subject of slavery, his enemies labeled him "a Southern man with Northern principles" because of his inconsistent stand on the gag rule and his support of the Tariff of 1842. He was one of two Southern Whigs in the House who supported President Zachary Taylor against Henry Clay's Omnibus Bill in 1850, yet he ultimately voted for more individual compromise measures than any other North Carolinian in Congress. Gerrymandered out of his seat by a Democratic legislature, he moved to San Francisco in 1853. His departure for California occurred at a time when the Whig party was fast disintegrating, and he never again found a political organization in which he felt completely at home.

Stanly's most controversial political role was as President Abraham Lincoln's military governor of North Carolina from May 1862 to March 1863. He faced an impossible task, for he was charged with establishing a pro-Union government in the eastern part of the state during "the high noon of Confederate military success." On the one hand, old friends and relatives regarded him as a traitor; on the other, Northern abolitionists accused him of seeking to perpetuate the institution of slavery. The issuance of the Emancipation Proclamation, which he believed made his mission hopeless, prompted his resignation.

Norman D. Brown concludes that Stanly cherished "three abiding loyalties—North Carolina, the Whig Party and, above all else, the Federal Union." It is unfortunate that the biographer has presented only an incomplete picture of the man, for his diligent search for Stanly letters in the manuscript collections of his contemporaries did not adequately compensate for the complete lack of family and personal correspondence. Brown's study is at its best in dealing with those phases of Stanly's career in which he figured prominently in the political arena. But at times "Whiggery's Tarheel 'Conqueror'" appears merely as a shadowy individual offering occasional opinions on the stirring events of his time.

EDWIN A. MILES
University of Houston

ton's Farewell Address. Furthermore, his ambiguity left his listeners uncertain of his commitment to republicanism and his attitude toward slavery.

Komlos' study, originally presented as a master's thesis at Northeastern Illinois University, would be well received by any graduate faculty. As a book, however, it retains too many of the flaws common to master's theses. For a subject that is inherently colorful, the writing style is dry, overburdened with unnecessary quotation and documentation, and marred by errors of grammar, syntax, and citation. The work also lacks emphasis; all the facts of Kossuth's journey are elaborately set forth without consideration for their relative significance. The East European Institute of the State University College at Buffalo, under whose auspices the volume appears, owed the author stronger editorial guidance.

Kossuth found to his own disillusion that Americans' wild acclaim was not easily translated into concrete assistance for his future plans. Most were simply seizing upon him as a convenient symbol by which to celebrate themselves. The ideological and emotional undercurrents of America's response are not deeply touched upon by Komlos and continue to offer an inviting subject for exploration.

JAMES M. BERGQUIST
Villanova University

JOHN H. KOMLOS. *Louis Kossuth in America, 1851-1852*. Foreword by C. A. MACARTNEY. (Program in East European and Slavic Studies, number 4.) Buffalo: East European Institute. 1973. Pp. 198.

Few foreign visitors to the United States have evoked a welcome as spectacular as that accorded the Hungarian revolutionary Louis Kossuth in 1851 and 1852. Yet the event has not been dwelt upon very much by American historians, few of whom have been able to use the massive work in Hungarian by Dénes Jánosy. By providing in English a detailed narrative of Kossuth's tour and an appraisal of its political and diplomatic implications, John Komlos has made a welcome contribution.

Komlos harbors no illusions about Kossuth, finding him largely responsible for the failure of his own American efforts. There was never, of course, any prospect of official help from the American government. But Kossuth himself spoiled whatever chances there were for extensive private aid for his cause. He appears as a poor leader who was hampered by his own vanity, whose ineffective organizing frittered away what meager funds he gained. He lectured in condescending fashion to Americans about the proper interpretation of Washing-

MILDRED THRONE. *Cyrus Clay Carpenter and Iowa Politics, 1854-1898*. Iowa City: State Historical Society of Iowa. 1974. Pp. xi, 302. \$8.00.

For more than a decade historians have awaited the resumption of publication by the State Historical Society of Iowa of those special scholarly studies and biographies for which the society had become justly respected. To signal the return of these publications the society has published an almost forgotten manuscript on the life of Cyrus Clay Carpenter by the late Mildred Throne. Long esteemed by her colleagues as an excellent editor and able scholar, Throne has, in effect, produced her own splendid memorial.

Devoted to the Republican party since its birth, Carpenter was a minion of the Dodge-Clarkson faction of the party that controlled politics in Iowa for more than a generation after the Civil War. Yet, he remained on good terms with such antimachine Republicans as James Grimes, James Harlan, and John A. Kasson. A reliable Radical, indefatigable campaigner, and trusted party work horse, Carpenter's rewards for faithful service included two terms as governor, two terms in Congress, and an appointment as a minor functionary in the Treasury Department. As Throne so ca-

pably points out, Carpenter lacked the necessary character traits essential for attaining the front ranks of political leadership, and therefore he always remained a minor figure in political circles, more respected than admired. Throne's fine study has rescued Carpenter from an obscurity that some may believe he correctly merits. Her balanced presentation shows competent scholarship, judicious restraint, and felicitous style. This volume complements earlier works on Carpenter's political contemporaries, Younger's *John A. Kasson* and Sage's *William Boyd Allison*, both also published by the State Historical Society of Iowa.

A number of weaknesses are apparent in the book, perhaps because Throne did not believe her manuscript was quite ready for publication. We look in vain, for example, for a thorough examination and penetrating analysis of the origins and development of the various splinter parties in Iowa during the last third of the nineteenth century; of the roles of the silver issue and the tariff question in local politics; of the depth and extent of the power of the railroads in the state. These are, at best, only noted superficially. Nevertheless, this biography makes a useful addition to the historical literature of late nineteenth-century Iowa.

MORTON M. ROSENBERG
Ball State University

GEORGE HARWOOD PHILLIPS. *Chiefs and Challengers: Indian Resistance and Cooperation in Southern California*. Berkeley and Los Angeles: University of California Press. 1975. Pp. xi, 225. \$10.95.

This is a well-researched, lucidly written monograph on southern California Indians. George Harwood Phillips revises the writing of American history by according the Indians the attention they deserve in their relationships with the white man. Consequently, Phillips does not dwell on the mistreatment of the Indians by Americans and does not view the whites "as the elites of American history." Instead, he examines how the Indians responded to the Spanish, Mexican, and American intruders "in ways that were logical and valid in light of their own experiences." By doing this he demonstrates that the southern California Indians were important in molding the history of the region to the 1860s.

In this study of Indian resistance and cooperation, Phillips focuses on the ambivalent responses of the three most active Indian groups whose lifestyle was affected by the American conquerors. The Cahuillas, led by Juan Antonio, responded by being cooperative with the Americans and Californians but received the least consideration and material help from them. The Cupeños, headed by Antonio Garra, who sought to organize an Indian

confederacy, rose against the Americans at Warner's Ranch and Kupa. Although the Garra or Cupeño uprising struck fear among the English-speaking population and made Garra a wanted criminal, it was Juan Antonio who turned him over to the authorities. The Luiseños were directed by Manuelito Cota, an American appointee and collaborator. Unlike Juan Antonio, who died in poverty, and Antonio Garra, who was executed, Manuelito Cota prospered and enjoyed old age.

Although I can understand the circumstances that led the chiefs to respond differently, I disagree with the author, who seems to be more appreciative of Cota's and Juan Antonio's leadership than of Garra's. Notwithstanding this difference of opinion, Phillips must be congratulated for producing a model of how United States history should be written and for making an original contribution to a long-neglected facet of California history.

MANUEL P. SERVIN
University of New Mexico

SHELBY FOOTE. *The Civil War: A Narrative*. Volume 3, *Red River to Appomattox*. New York: Random House. 1974. Pp. 1,106. \$20.00.

"Farwel my book and my devocion." With words of Chaucer, Shelby Foote completes his massive three-volume study of the Civil War. Thus ends a twenty-year love affair of an author with words and colorful descriptions of men and events in our great sectional conflict, leaving us richer in new literature and historical narrative of that war. This work will surely rank with the classic studies of Allan Nevins, Bruce Catton, and Carl Sandburg.

It is with "characters and incidents" of the war that Foote is at his best. "I have searched them out," he writes. "I took them as they were." With creative imagery, he sets his characters upon the stage of war—soldiers and civilians, the nameless and renowned, the hero and the knave—and each responds to war's complexity in his own particular way, be it "the scantily clad Confederates and their crowbait nags," blue-coated Yankees dying recklessly before Cold Harbor, or generals and civilians behind battlelines, in endless conferences that determined fates of nations—and of other men.

Foote's portrayal of Robert E. Lee is a masterpiece of understanding and love, capturing the distinctive qualities of a Southerner he never ceases to admire. He also recorded the complexities of other lives—the embattled presidents with their sad eyes, generals in victory and defeat, and the lesser-known figures who also served. There is always the lingering regret for the fallen, the gnawing wonder of what might have been had

the statistics not claimed their Jacksons, Stuarts, Reynolds, or Sedgwick.

On every page is the sound of battle, echoing across the years to the present, making each succeeding generation "a part of" a war that yet defies our imaginations. As in his first volumes, Foote is at home in the field, amidst the surge of armies, the clash of battle, the charging echelons of Blue and Gray fighting at the pinnacle of life to capture another enemy position. There is, however, a growing maturity with each new volume, a style that remains warm and consistently human. The conclusions in the final chapter are refreshing and personal. This account is a mosaic of war—its ferocious charges, the bloody withdrawals, the unrelenting decisions of command, the boredom and loneliness under silent guns, infuriating moments of indecision and disobedience, the exuberance of victory, and the tears of defeat. There is no fatal bias, but as Catton, Nevins, and Sandburg chose Northern perspectives, so Shelby Foote chose a Southern one.

The author depended heavily upon the secondary materials of the war for his research, mining them extensively and thoroughly in his search. He used traditional judgments of other historians to draw conclusions on debatable issues of the war, and he depended too heavily upon controversial interpretations such as Hudson Stroud's characterization of Jefferson Davis and Charles Ramsdell's interpretation of Civil War causation.

Though history written as literature may cease to be history as we should define it, it may still retain validity as an art form and as another source. Foote's monumental work may lose some of the objectivity that goes with more up-to-date research, but these volumes are commendable materials for Civil War study. By following different routes to his conclusions, the author has achieved a work of art, one characterized, as the late Wendell S. Stephenson wrote, "by serious purpose and hard work."

In the midst of the celebration of our nation's Bicentennial, this work strips a bit of the luster from our self-congratulations and reminds us again of the dark star that led us down the trail of tears to sectional war. It also reminds us that history can be presented with beauty and feeling and be written for understanding.

ROBERT HARTJE
Wittenberg University

PETER J. PARISH. *The American Civil War*. New York: Holmes & Meier Publishers. 1975. Pp. 750. Cloth \$25.00, paper \$12.50.

The American Civil War has always had great appeal to Britishers. One need only recall the dis-

tinguished works of such authorities as J. F. C. Fuller, Colin Ballard, Liddell Hart, and William R. Brock to be reminded that some of the most engaging and perceptive writing about America's greatest conflict has come from the pens of British authors. *The American Civil War* by Peter J. Parish, lecturer at the University of Glasgow, is an impressive addition to the shelf of distinguished volumes by foreign authors.

Parish's volume is outstanding for its comprehensiveness. It is a big book, with a text of well over 300,000 words, supplemented by twenty pages of reference notes, a chronology, and a remarkably good bibliographical essay. Narrative, notes, and bibliography evidence a thorough acquaintance with published materials treating the Civil War, including general accounts, source collections, biographies, studies of campaigns, monographs, and scholarly articles. Five of the book's twenty chapters deal with the background and causes of the conflict, and the two final chapters are devoted to Reconstruction.

The author maintains a good balance in attention devoted to the North and South; social, economic, and political aspects; domestic and foreign affairs; and military and naval activities. From beginning to end he manifests a desire to be fair.

Parish examines and evaluates various influences contributing to the outbreak of hostilities between the North and South, but he rates slavery as "the prime cause of the Civil War." In evaluating leadership he gives Northerners a higher rating than Confederates. His treatment of Jefferson Davis is sympathetic, but he concludes that much of the trouble experienced by the Confederate president was of his own making. He finds Davis "fixed in his ways, conventional in his outlook, reluctant to forgive a critic or forget a grievance . . . legalistic and self-righteous . . . never able to reach out to the whole Southern people, to articulate their war aims in words of genuine inspiration." Parish is by no means uncritical of Lincoln, but he praises the Union president for his flexibility, resilience, strategic wisdom, political astuteness, courage, and magnanimity. "Of all democratic leaders of his or any other day," he states, Lincoln "best preserved the common touch. . . . Although no one felt the agony and anguish of war more deeply than Lincoln, he also possessed the iron resolution of a great war leader. He sustained the Union cause in its darkest moments. . . . He became, warts and all, the living embodiment of that cause, whereas Jefferson Davis was more of a marble monument to his."

The superiority of the North's political leadership was not confined to the presidency. According to the author, "there were no Confederate equivalents to Seward, Chase and Stanton at cabi-

net level." He characterizes Alexander Stephens, the vice-president, as "a tiny, sickly, neurotic but extremely gifted man." He concludes that as a group, Northern congressmen were more able than their Richmond counterparts. Among Southern governors he finds no one as capable as Morton of Indiana, Andrew of Massachusetts, or Curtin of Pennsylvania. "The South, setting out to create a new government, proved too inflexible, traditional, negative and sterile."

Parish recognizes the "battlefield brilliance" of Lee, Jackson, Forrest, and other Confederate generals, but he concludes that "in military as in political leadership they [the Northern generals] showed a resilience, a flexibility and a breadth of comprehension which the South could not match." For all their brilliance, Lee, Jackson, and their associates "gradually came to look old-fashioned . . . the last great exponents of a dying school, they won the kind of battles which were not to decide this kind of war."

In the first years of the conflict Grant, Sherman, Meade, and other high-ranking Union leaders were less impressive than their opponents, but they grew with experience until by 1864 they and the command system that they helped develop proved unbeatable. "Lincoln and Grant were right to attach so much importance to elimination of the opposing armies," the author states, "but Sherman's mind, like his army, penetrated more deeply in its insistence that in a war of peoples, the prospect of defeat must be brought home to the people themselves." Parish characterizes Sherman's march through the Carolinas as "one of the truly stupendous feats of the war."

In discussing the outcome of the conflict, the author states that the North's enormous superiority in men and material resources was offset to a large extent by the fact that it had the more difficult task of invading and occupying a vast area and conquering a resolute and resourceful people. He aptly states that "more recent wars have underlined the difficulty of converting an overwhelming preponderance of power into final and total victory."

The North's advantage in men and materiel, according to the author, was greatly strengthened by other assets, among them a more diversified economy, a greater capacity for organization, and a more enduring will to win. He states that "the North had a better cause, the greater ideological strength, the less damaging moral flaws." He concludes that the South's internal weaknesses, including an undue attachment to state rights, a stubborn insistence on individual freedom, and an exaggerated idea of the importance of "King Cotton," contributed immeasurably to Confederate defeat. "Above all," he states, "slavery, the very

cornerstone of the Confederacy, proved to be its severest handicap." Slavery, he adds, "blocked the path to European recognition. Perhaps most insidious of all . . . was its corrosive effect on Southern morale and self-confidence."

The American Civil War is remarkably free of factual error. While I find myself in agreement with nearly all of the author's interpretations, I question his apparent conclusion that Northern morale sank so low in 1864 as to greatly imperil the survival of the Union. Letters of Union soldiers indicate such strong and persistent devotion to the Northern cause as to make unlikely the acceptance of Southern independence by the masses, even if Lincoln had failed to be re-elected. I also doubt that Sheridan "had no superior in the Civil War leadership on the field of battle." I would rate Forrest and Cleburne above "Little Phil" as combat commanders. But these are matters of judgment, and the author's views may have more validity than mine. His book is a model of excellence in every respect. In my opinion it is the best of all one-volume surveys of the American Civil War, and it contains more of substance and soundness than many multivolume treatments of the subject.

BELL I. WILEY

Agnes Scott College

DAVID COE, edited and with an introduction by. *Mine Eyes Have Seen the Glory: Combat Diaries of Union Sergeant Hamlin Alexander Coe*. Rutherford: Fairleigh Dickinson University Press. 1975. Pp. 240. \$10.00.

In August 1862 a twenty-two-year-old wagon maker named Hamlin Alexander Coe joined the Nineteenth Michigan Volunteer Infantry Regiment. After five weeks of training he began his diaries as his regiment, "armed and equipped for Dixie," advanced into Kentucky. In March 1863 Private Coe was captured in Tennessee. "Weary and emaciated," he was sent to Libby Prison in Richmond. A week later he was exchanged, and after seven months of recuperation he returned to limited duty as Union forces gained the upper hand in the Chattanooga area late in 1863.

Corporal Coe was finally healthy enough to return to combat duty as his Nineteenth Michigan Regiment joined Sherman's powerful offensive into northwest Georgia in the spring of 1864. Promoted to sergeant, he saw his Company E cut to pieces in a series of bloody battles before Atlanta fell on September 2. He was again weakened by illness and spent the last months of the war convalescing. Discharged in May 1865, he went home and concluded his last diary, rejoicing to be "once more in God's country and a free man."

Sergeant Coe's diaries and other papers lay for-

gotten in a trunk until his grandson rediscovered them. A combat infantry sergeant in World War II, David Coe was convinced that his grandfather's Civil War diaries merited publication. His experience as a photographer-journalist was helpful, especially with the effective illustrations, but his lack of historical training was a real handicap. The book lacks an index, a bibliography, and footnotes, and the editorial comments are sometimes shallow. Coe appears unaware of the best works of scholarship in recent decades. On the dust jacket the diaries are described as "sometimes nearly illegible," but they are published in flawless English without explanation.

Nevertheless this volume is interesting and informative. Like Bell Irvin Wiley's studies of Johnny Reb and Billy Yank, it shows what the Civil War was like for the ordinary soldier. The historian as well as the general reader can learn from intelligent "little men" like Sergeant Ham Coe.

F. N. BONEY
University of Georgia

BELL IRVIN WILEY. *Confederate Women*. (Contributions in American History, number 38.) Westport, Conn.: Greenwood Press. 1975. Pp. xiv, 204. \$10.95.

Originally presented as a series of lectures, *Confederate Women* consists of four essays: the first three are portraits of Mary Boykin Chestnut, Virginia Tunstall Clay, and Varina Howell Davis; the fourth is a survey of the wartime experiences of Southern women in general. Based on extensive research among letters, diaries, and memoirs of Confederate women, these essays are interesting and occasionally provocative, but they do not significantly enlarge or revise our understanding of Southern women drawn from Anne F. Scott's *The Southern Lady* and Elizabeth Massey's *Bonnet Brigades: American Women and the Civil War*.

Some of the limitations derive from the author's choice of subjects. Although, as Wiley maintains, Chestnut, Clay, and Davis may represent "distinct types of Confederate womanhood: the childless intellectual; the inveterate Southern belle; First Lady, wife and mother," Clay and Davis are notable only because of their husbands' positions, and Wiley's portraits of all three bear little resemblance to the women discussed in the fourth chapter. Moreover, a focus on other types—a woman who managed the family business, or one who provided nursing or other services to the Confederate Army—would more fully illuminate the experiences of elite women during the war. Nor does the author make maximum use of the material on the three primary figures. These chapters are overly anecdotal with a disproportionate emphasis on superficial social relations at the expense

of more profound analysis of how these women perceived and operated within the patriarchal order.

The final essay provides a larger perspective on the war's meaning for women in general. Because Wiley relies on women's letters and diaries and does not employ statistical data, his focus remains on privileged women. Women of the yeoman class are mentioned infrequently, and black women are allotted just five paragraphs. Although his discussion of women's responses to the new demands and hardships brought on by the war is necessarily brief, Wiley mentions a variety of roles—women as factory and government employees, as leaders of bread riots, as petitioners to the government, as wives and mothers of soldiers—which suggest opportunities for further investigation.

SUSAN M. HARTMANN
University of Missouri,
St. Louis

ROBERT P. SWIERENGA, editor. *Beyond the Civil War Synthesis: Political Essays of the Civil War Era*. (Contributions in American History, number 44.) Westport, Conn.: Greenwood Press. 1975. Pp. xx, 348. \$13.50.

This is a useful compilation of the work of a group of the self-styled "new political historians" with their social-scientific orientation. However, not all of the historians in the volume fall into that category. The title is derived from Joel H. Silbey's argument in the opening essay that historians of the middle period of United States history have allowed the Civil War to distort reality by over-emphasizing sectionalism and, in effect, reading backward from the fact of the war to interpret too much of what preceded it. Where others have emphasized rising sectionalism in the 1840s, Silbey's analysis of congressional voting leads him to stress its partisan rather than its sectional nature. In the second essay Eric Foner suggests that some of the new political historians "seem to be in danger of substituting a religious or cultural determinism" to replace an older economic determinism (p. 18). Richard O. Curry's penetrating overview of recent work in the Civil War and Reconstruction period and Stephen B. Oates' survey of the historical literature on John Brown round out part 1, the historiographical portion of the volume.

"Politics at the Grass Roots" is the focus of part 2. David E. Meerse analyzes the Northern Democratic party's loss of congressional seats in 1858 and concludes that the development was "as much the result of power, prestige, and patronage as of unpopular Presidential policies about Kansas" (p. 96). Robert P. Swierenga deals with a group of Dutch immigrants in Iowa who stubbornly voted Democratic in 1860, thus defying not only certain

of their leaders but those generalizations about Lincoln's hold on the foreign-born in the Midwest. Lawrence N. Powell argues that the larger issues of Reconstruction had little bearing on the fact that seventeen Republican incumbents were not renominated in 1866, yet in the subsequent essay Michael Les Benedict advances the thesis that in the state and local elections of 1867 in the North, the congressional plan of reconstruction was on trial and was rejected by the voters, thus setting "the limits on reform in reconstruction" (p. 147). Phyllis F. Field studies referenda held in New York on the question of black suffrage in 1846, 1860, and 1869; she demonstrates that while even the Republicans were badly divided on the matter of black suffrage in 1860, they had moved far closer to agreement in support of it by 1869.

Various aspects of roll call voting in Congress are treated in part 3. Although the editor contends in the introduction that "all five of the articles in Part III suggest that party is the most important determinant" (p. xvi), Gerald W. Wolff, employing Guttman scalogram analysis of voting on the Kansas-Nebraska question, insists that both party and sectional influence played major roles but "sectionalism appears to have been most potent" (p. 179). Richard E. Beringer traces an "unconscious" party spirit that developed in the Confederate Congress, and Allen G. Bogue studies bloc and party voting in the Senate from 1861 to 1863 to conclude that since party, geographic bloc, and faction were all important factors, historians should study the "houses of Congress during the Civil War as political systems in which a variety of determinants of voting behavior were interacting" (pp. 222-23). Edward L. Gambill and Glenn M. Linden provide, respectively, more precise categories for the Republican factions in the Senate in 1866 and a roll call analysis of the Senate from 1873 to 1877 which shows that "the Senate had returned to its normal voting habits in the realm of political and economic measures" (p. 241).

The ethnoreligious dimension of politics is the subject of part 4. Leonard Tabachnik argues that, despite the charges of nativists, foreign-born citizens were significantly underrepresented in the Federal Customhouse Service from 1821 to 1861. Richard Jensen uses Midwestern county directories as a basis for his proposition that, outside the South, the pietistic voters (that is, Congregationalists, Methodists, Baptists, and so on) tended to be Republican and the members of liturgical churches (especially Roman Catholic) tended to be Democrats. August Meier marshals traditional, nonquantitative evidence to support his thesis that most of the Northern people in positions of political power during Reconstruction "were not really interested in the Negroes' welfare" (p. 289).

In the fifth and final group of essays, "Ideology and Politics," Larry Gara suggests that a Northern political party was made possible by a "steadily increasing opposition to the slave power" rather than by "any growth of pure antislavery sentiment or humanitarian consideration for the slave as an oppressed human being" (p. 308). Bertram Wyatt-Brown and James B. Stewart deal with William Lloyd Garrison in the last two essays, the first emphasizing that the fiery abolitionist actually helped keep traditional antislavery action in nonviolent channels and the second arguing that the Garrisonian approach to Northern politics in the 1840s and 1850s was neither as unsophisticated nor as unproductive as many historians have assumed.

While the new political historians obviously have much to contribute toward a more precise formulation of various aspects of the history of the middle period, several of the strongest essays are traditional in method and style. Moreover, some of the quantifiers are so preoccupied with their methodology that their findings tend to become subordinated and lost in lumpy prose that is badly scattered among tables. Certain details about the "Civil War synthesis" have been corrected, but there is surely no alternative synthesis yet in sight.

ROBERT F. DURDEN
Duke University

JERRELL H. SHOFNER. *Nor Is It Over Yet: Florida in the Era of Reconstruction, 1863-1877*. (University of Florida Book.) Gainesville: University Presses of Florida. 1974. Pp. x, 412. \$12.50.

Additional sources and a different set of preconceived opinions allow Jerrell H. Shofner to produce a general history of Florida between 1863 and 1877 that is considerably different from William Watson Davis' 1913 Dunning School offering. The new work fits the framework being so gropingly erected by revisionist scholars, and, while not free of discernible blemish caused by modern biases, it ranks with the better efforts of that school currently in vogue. Veering most importantly from Davis in attitude toward black people during the time of Reconstruction, Shofner offers the usual revisionist evaluations, particularly in assessing levels of corruption and crime. Shofner is perhaps justifiably sympathetic in areas where his predecessor was hostile, but the book's most crucial flaw remains that while the author recognizes a program which was clearly opposed by a majority of the local population and which was forced into effect during Reconstruction, he fails to wrestle adequately with this philosophical and weighty matter.

Shofner, a consensus historian, develops the

theses that the period is identifiable neither by sudden nor by substantive historical elements, that in Florida continuity was much greater than change, that conservative Democrats were not much different from the Republicans except in racial attitudes, and that almost everyone's actions were complex and often reciprocally interrelated in the historical context. Shofner fights an ongoing battle against the persistent myth that, in the end, carpetbaggers packed up and headed North and that the leadership change which followed was among the most complete ever.

The work is soundly researched, though some of Davis' sources unfortunately are no longer available. The literary quality is good. The plethora of detail illustrating and supporting the generalizations is occasionally tedious, but with the anecdotes and other data this gives the book some utility as a reference. Commendable chapter summations and a concluding essay render its messages easily discernible. Of special interest are the occasional linkages to national affairs of matters that appeared on the surface peculiarly Floridian.

HERMAN HATTAWAY
University of Missouri,
Kansas City

RICHARD A. BARTLETT. *Nature's Yellowstone*. Albuquerque: University of New Mexico Press. 1974. Pp. xiii, 250. \$10.00.

The 1972 centennial of Yellowstone National Park, the nation's and the world's first, prompted publication of several volumes concerning the region's character and history. They range from the lavish, coffee-table format of Ann and Myron Sutton's *Yellowstone: A Century of the Wilderness Idea* (1972) to Aubrey L. Haines' *Yellowstone National Park: Its Exploration and Establishment* (1974), an exhaustively detailed, documentary approach. Bartlett's book takes a middle course. His treatment is scholarly and well-documented yet light in tone. The author clearly loves Yellowstone, and the fact that he enjoyed writing about it is to the reader's decided advantage.

Though not a scientist, Bartlett describes the geological and biological history of the Yellowstone country quite impressively. There are few better places than the opening chapters of this book where one can find out why a Yellowstone geyser erupts or a Yellowstone black bear mooches garbage. Turning to the human history of the region, Bartlett brings together the best knowledge available about its Indian occupants and the first white visitors. The remarkable wilderness wanderings of John Colter are featured in the latter discussion. Bartlett sifts the inconclusive evidence about Colter's actual route and, to his credit as a

historian, frankly confesses that unless new material is discovered, no one can be sure that Colter ever saw Yellowstone between 1806 and 1810.

Another topic, disputed for a century, is the question of whom to credit for inventing the national park idea. In his presentation of the formal exploration of Yellowstone and the establishment of a national park in the region in 1872, Bartlett of necessity enters the argument. His account deals clearly and fairly with the history of the national park concept, although George Catlin's journal proposal of a "nation's park" in the American West came in 1832, not 1833 as Bartlett asserts (p. 191). In his discussion of the candidacy of Thomas Francis Meagher as father of Yellowstone National Park, Bartlett turns new sod. An Irish revolutionary who escaped from exile in Tasmania to become governor of the Montana Territory, Meagher received reports of Yellowstone's wonders from trappers, miners, and a Jesuit priest to the Blackfeet, Francis Xavier Kuppens. Meagher's 1865 suggestion—if correctly reported by Father Kuppens—that the Yellowstone region be reserved "for a national park" is not only the first specific suggestion of a park in the area but the first known use of the phrase "national park" in American letters. Had not Meagher drowned in the upper Missouri River in 1867, he might well have implemented his plans to explore Yellowstone and advocate park status.

If the present volume may be taken as evidence, Bartlett's proposed continuation of Yellowstone's history from 1872 through the park's uncertain early years should be eagerly awaited.

RODERICK NASH
University of California,
Santa Barbara.

LEONARD J. ARRINGTON. *Charles C. Rich: Mormon General and Western Frontiersman*. (Studies in Mormon History, volume 1.) Provo, Utah: Brigham Young University Press. 1974. Pp. xvii, 386. \$7.50.

The significant theological and cultural units in Mormonism are the family and the church; individuals matter less. Perhaps that explains why biography, a congenial genre for most American authors, is a difficult form for Mormon scholars. In biographies written by Latter-day Saints the individual is so often overshadowed by his world that most Mormon biography is really family history or, more likely, church history that merely highlights the activities of the biographer's subject. Leonard J. Arrington's biography of Charles C. Rich, an apostle whose importance rests mainly on his military leadership and his colonization efforts in San Bernadino and Bear Lake Valley, is no exception. Rich's story dominates the early chap-

ters, but when the history of his life as Latter-day Saint begins, the emphasis shifts. At times the background obscures the foreground to such an extent that the events in Rich's life seem almost incidental. While information is admittedly sparse, the problem is less a silent record than misplaced focus. Intended as Rich's story, the study intermittently assumes the character of a narrative history of Mormonism. Nevertheless, this is not a run-of-the-mill Mormon biography. Conjecture is identified with scrupulous care; quoted material is reproduced—as a Rich descendant reminded me—with misspellings intact; and pious platitudes are generally absent. Despite its shortcomings, historians will find this a better biography than the 1936 panegyric it replaces.

A scarcity of scholarly studies of the leadership is a serious lacuna in Mormon history. Acceptable biographies of Brigham Young, John Taylor, George Q. Cannon, and other major figures do not exist. Since Arrington's knowledge of the Mormon past is second to none, since his respect for the canons of historical scholarship is manifest, and since his access to the sources is virtually unlimited, historians can only regret that he chose to study a Latter-day Saint leader of the second rank.

JAN SHIPPS
Indiana University—Purdue University,
Indianapolis

JOHN M. CARROLL, compiled, edited, and written by. *Custer in Texas: An Interrupted Narrative*. New York: Sol Lewis and Liveright. 1975. Pp. xx, 288. \$15.00.

ALBAN W. HOOPES. *The Road to the Little Big Horn—And Beyond*. New York: Vantage Press. 1975. Pp. 336. \$9.50.

BRUCE A. ROSENBERG. *Custer and the Epic of Defeat*. University Park: Pennsylvania State University Press. 1974. Pp. xiii, 313. \$13.50.

Here are three of the many books on George Armstrong Custer that are appearing during the centennial year of the Battle of the Little Bighorn. Beyond involving Custer in one way or another, the three have little in common.

The one of broadest appeal is Bruce A. Rosenberg's *Custer and the Epic of Defeat*. It is literary analysis rather than history. Indeed, as history it is deficient, for the author's knowledge of the history of the Battle of the Little Bighorn is superficial and sometimes in error, and it rests on a very small sampling of the extensive bibliography of the subject. As literary criticism the book will be received as a penetrating study of the process of legend-making. Taking Custer and his "last stand" as the model, Rosenberg shows that certain enduring

principles shape legends about heroic figures and that they can be discerned in the literature of such diverse characters as Saul, Roland, Leonidas, and others over the past twenty-five centuries. Although occasionally murky and irrelevant, the comparisons for the most part are illuminating and often fascinating.

Alban W. Hoopes is remembered by students of Indian history chiefly for a slim but useful volume entitled *Indian Affairs and Their Administration, with Special Reference to the Far West, 1849-1860*, originally a doctoral dissertation. With *The Road to the Little Big Horn—And Beyond*, he returns to publish in this field after an absence of more than forty years. This is not simply a history of the Battle of the Little Bighorn but rather of relations with the Indians of the northern Plains from Sand Creek in 1864 to the surrender of Sitting Bull in 1881. It draws on an unusually broad bibliography, including private papers and official records. Even so, it adds little to an oft-told tale and is in places uneven.

The final volume will be of interest mainly to Custer specialists. Handsomely designed and printed and illustrated with original artwork, *Custer in Texas* deals with the eight months following the close of the Civil War during which time the twenty-five-year-old general was commander of a mutinous cavalry division sent to Texas for possible use against Maximilian's forces in Mexico. This is billed as an "interrupted narrative"—it is Elizabeth Custer's account, taken from her *Tenting on the Plains*, interrupted by editor John M. Carroll for his own remarks or for the insertion of material from other sources.

ROBERT M. UTLEY
National Park Service

BINGHAM DUNCAN. *Whitelaw Reid: Journalist, Politician, Diplomat*. Athens: University of Georgia Press. 1975. Pp. 305. \$11.00.

Whitelaw Reid (1837-1912) owned and edited one of the great newspapers of his time, the *New York Tribune*; was an important political figure for more than a quarter of a century, running for vice-president on the Republican ticket in 1892; and served as minister to France, as a delegate to the peace conference after the Spanish-American War, and as ambassador to Great Britain.

Surely a man of this importance deserves to be studied. Yet the only complete biography of Reid previously published was written by Royal Cortissoz, a good friend and ardent admirer, more than fifty years ago. Bingham Duncan amply fills this lacuna with this slender and excellent volume. Using a variety of manuscripts and standard works,

Duncan has written a solid and absorbing new account of Reid's public life.

Duncan does not try "to support or destroy any preconceived theory of Reid's significance nor to make him seem to be a force for progress or a negative influence on developments in his period." Within this framework he achieves his purpose. Though his study will not entirely supplant the Cortissoz volumes, it probably will become a standard work, and it surely makes a significant contribution to American society and politics during the Gilded Age.

Duncan shows that Reid was a supporter of civil service legislation, sound money, and a protective tariff in politics. In foreign affairs he opposed any entanglement in European affairs, supported the Monroe Doctrine, believed in American superiority in the Caribbean, and eventually became an outright imperialist advocating a course of colonialism both in the Philippines and the Caribbean. While he often mentioned duty in discussing colonialism, his arguments were mainly deterministic, putting him with the Social Darwinists.

This is a balanced, readable, and well-researched book, and it should appeal to scholars and general readers alike. But I wish the author and publisher had put the footnotes at the bottom of the pages where they belong.

VINCENT P. DE SANTIS
University of Notre Dame

and significance of immigrant mobility. Thus he surmises that in South Bend "the immigrant might hope to find a community whose social and economic structure was not already fixed"; that such segregation as existed "might have been based" on ethnic preferences; or that a marginal difference in literacy somehow "helps to explain" a group's success.

Some inferences seem contradictory: from the general rate of success Esslinger concludes that South Bend must have attracted immigrants with "more money and ambition" straight from the dock—or conceivably they had gained useful experience during lengthy stays along the way—and yet lack of success among them is held to imply previous failure in the East as well. Some conclusions are tautologies: Germans score higher than Englishmen (11-9) as leaders of the general urban community mainly because Esslinger counts officeholding in ethnic societies (six of them German, none English) among his indexes of public leadership, without ever demonstrating how such societies involved either "daily contact" among ethnic groups or "adjustment to an American way of life." If the remaining two hundred-odd cities of the time are in line for such studies, the analysis will have to be considerably less tenuous than this.

ROWLAND BERTHOFF
Washington University

DEAN R. ESSLINGER. *Immigrants and the City: Ethnicity and Mobility in a Nineteenth-Century Midwestern Community*. (Interdisciplinary Urban Series.) Port Washington, N.Y.: Kennikat Press. 1975. Pp. xii, 156. \$9.95.

To the half-dozen American cities of 1850-80 that have been quantitatively analyzed since 1964 on the lines of Stephan Thernstrom's Newburyport model, Dean R. Esslinger now adds South Bend.

This Midwestern city displayed some differences. Although the Irish, French Canadians, and Poles of South Bend were residentially more concentrated than other ethnic groups, no ward had more than a fraction of any group; this resulted from the tendency of newly arrived immigrants to settle near recently established local industries. Except for the Irish, immigrants who "persisted" in town for more than a decade (as elsewhere, most did not) enjoyed far greater upward mobility to skilled and white-collar jobs than in Newburyport or Boston, and their sons continued the climb.

Such measurements are useful enough. Since the book lacks the qualitative half of the Thernstrom model, however, Esslinger often resorts to speculation—or to deduction from propositions of Thernstrom's and Handlin's—to explain the causes

JAMES M. YOUNGDALE. *Populism: A Psychohistorical Perspective*. Port Washington, N.Y.: Kennikat Press. 1975. Pp. 220. \$15.00.

James Youngdale, a Minnesota farmer and laborite-turned-academic, has two aims in this book. The first is to rescue Populism from historians like Richard Hofstadter who have confused its radical social programs with the hysteria of mainstream "Bryanism." Both movements developed as the nineteenth-century small-producer paradise shattered. William Jennings Bryan, his followers, and his successors, trapped inside a self-made dream, invented monsters to explain the loss of their world. Populism was not free of Bryanist elements, but because it grasped the social character of existence, it could explain social change without demonology and offer concrete programs of community control. Under the influence of labor and socialist elements, which played a larger role in grass-roots Populism than traditional historians have admitted, Populism moved in a "radical neomercantilist" direction.

Youngdale wants psychology, like history, to center on the social world. Thus, his second aim is to replace a Freudian interpretation of mass movements with an Adlerian one. While Freud empha-

sized inner anxiety and unconscious needs, Adler rejected "internalized personality structure" (p. 58) for a psychology in which "the attitude toward others" (p. 61) generates goal-oriented strategies for making sense of the world. Adler makes social movements rational and offers, against Freudian and American individualism, an ideal of human cooperation.

The book's opening chapters are wide ranging, impressive, and unstereotyped. The actual studies of Populism offer valuable vignettes, particularly for twentieth-century Minnesota. If the presentation is episodic, Populism's relation to Bryanism is still a major theme. Adler, however, completely disappears. Although a psychology that does away with interior life flattens out experience, it may have insights to offer. But Adlerian psychology, whatever its qualities, does not inform the actual history written here. Youngdale uses Adler to refute Hofstadter's Freudianism, not to ground a psychological reading of his own. *Populism: A Psychohistorical Perspective* has a strong purchase on Populism. It is not psychohistory.

MICHAEL ROGIN
University of California,
Berkeley

JEFFREY G. WILLIAMSON. *Late Nineteenth-Century American Development: A General Equilibrium History*. New York: Cambridge University Press. 1975. Pp. x, 350. \$19.50.

Economic historians have dealt with nearly every aspect of American development during the years between the Civil War and World War I. Jeffrey Williamson believes, however, that our knowledge and understanding is still lacking because scholars have not used proper research methods to answer the key questions connected with economic development. To deal with what he considers the shortcomings of previous researchers, Williamson uses the techniques of model building, counterfactuals, and general equilibrium analysis. This volume, therefore, is filled with equations, graphs, charts, and statistical tables that will deter all but the most determined reader.

In chapter 3 the author develops a long and detailed model for regional growth, and he argues that the model accurately simulates actual economic development between 1860 and 1910. He then applies this model to six major issues: secular decline after 1870, the national capital market, agricultural discontent, the role of railroads, overseas trade, and immigration. By dealing with these issues in "actual" terms and then applying his counterfactual history to the same events, Williamson comes up with some interesting conclusions.

Some of his positions are new and even startling,

although questionable. Williamson concludes, for example, that the economy performed inefficiently from the late 1860s to the early 1870s because too few resources were devoted to agriculture. He also argues that land availability had very little effect on economic growth after the Civil War, and that the supply of relatively cheap land had no significant effect on immigration to the United States. On the other hand, the author simply substantiates what most scholars have long accepted. Such conclusions as "world market conditions had a profound effect on the agriculture of the Midwest" (p. 213), or "An America without immigrants indeed would have grown very differently from how in fact she did in the late nineteenth century" (p. 249), hardly seem worth the author's complicated methods.

The evidence is not yet all in on the development of the American economy between 1860 and 1910. This volume, however, provides more speculation than understanding. There is little doubt that the book will arouse controversy and debate, but it is more distinctive in its methodology than in what it shows us about the developing economy of the United States between 1860 and 1910.

GILBERT C. FITE
Eastern Illinois University

ALBERT W. NIEMI, JR. *State and Regional Patterns in American Manufacturing, 1860-1900*. (Contributions in Economics and Economic History, number 10.) Westport, Conn.: Greenwood Press. 1974. Pp. xiii, 209. \$11.00.

FREDERICK MOORE BINDER. *Coal Age Empire: Pennsylvania Coal and Its Utilization to 1860*. Harrisburg: Pennsylvania Historical and Museum Commission. 1974. Pp. 184. \$5.50.

Neither of these studies significantly alters traditional views of the nineteenth-century American economy. The more ambitious and successful of the two books is Albert W. Niemi, Jr.'s treatment of changing state and regional patterns in manufacturing. Niemi's study is based on data from the 1860 and 1900 published federal manufacturing censuses. In order to compare more systematically and accurately information from the two censuses, Niemi performed the arduous task of classifying all the data into twenty major manufacturing groups according to the Standard Industrial Classification (SIC) two-digit industry categories. The tables throughout the text and an appendix of nearly one hundred pages present—in SIC categories by state and region—estimates of value added, employment, labor costs, capital invested, output-labor ratios, capital-output ratios, and capital-labor ratios for 1860 and 1900. This presentation of data in

a usable and comparable form is Niemi's major contribution.

In the brief text that accompanies the tables, Niemi compares the structural and locational pattern of manufacturing in 1860 with that of 1900. He finds that manufacturing was concentrated in the Northeast in 1860 and that there was a gradual drift of industry into the Midwest and South by 1900. These changing locational patterns, he concludes, depended less on supplies of capital and labor than on resource conditions, local demand, and external economies. Niemi's conclusions confirm earlier views, although no previous work rested on this revised data base. There is little reason to doubt the author's general conclusions, but questions and opportunities for further study remain. Analysis of the manuscript rather than the published census, which is now underway, may allow for alternative or better answers to these questions, especially in providing causes for the structural shifts in manufacturing. It may also be possible—it is certainly desirable—to replace census data defined by state boundaries with data that reflect more accurately the market realities of economic geography. And, while Niemi briefly sketches manufacturing changes after 1900, more detailed comparisons with early twentieth-century structural and locational changes are needed. As a reference source and as an introduction to late nineteenth-century state and regional patterns of manufacturing Niemi's book is a useful addition to the literature.

Frederick Moore Binder's theme in *Coal Age Empire* is the use of Pennsylvania coal, bituminous and anthracite, before 1860. He concentrates on developments in home heating, urban gas lighting, steamboats, locomotives, and fuel for industrial growth. The book is weakest in consideration of transportation and industrialization—areas where Pennsylvania coal most affected the course of antebellum economic development. Binder does not relate systematically increased use of coal to major changes in technology, markets, or the industrial order. He neglects to analyze fully and explicitly such fundamental factors in the West as the correlation between use of coke, exploitation of the Connellsville coal region, and the market for rails or, in the East, the timing and nature of transportation access to anthracite fields. Knowledge of river, canal, and railroad transportation from the Pennsylvania coal regions is a necessary prelude to understanding the use of coal and its relationship to industrial change, but Binder largely ignores this subject until the final chapter, a shortcoming that illustrates the book's poor organization and inadequate attention to major questions.

Although the author presents interesting and useful facts, scholars studying the significance of

coal in the antebellum economy will also wish to read Peter Temin's *Iron and Steel in Nineteenth-Century America* (1964) and Alfred D. Chandler's essay on anthracite coal in the *Business History Review* (summer, 1972), neither of which is cited in Binder's footnotes or bibliography.

JAMES H. MADISON
Indiana University,
Bloomington

JAMES GILBERT. *Designing the Industrial State: The Intellectual Pursuit of Collectivism in America, 1880-1940*. Chicago: Quadrangle Books. 1972. Pp. ix, 335. \$10.00.

James Gilbert believes that too many American liberal intellectuals have been mesmerized by stereotypical political terms despite the mountain of revisionist writing on progressivism and socialism. He believes there is a language of politics that often evades reality in order to defend clichés. The clichés are, of course, words like liberal, conservative, progressive, socialist—the parlance of both progressive and revisionist history. Gilbert's use of the concept of collectivism as an umbrella for the thought and activities of Americans who span the whole political spectrum is extremely effective and makes intellectual sense out of what was before a series of anomalies.

The book seems to grow naturally out of the W. A. Williams Wisconsin school that called into question much of progressive and liberal-consensus history. But Gilbert is not trying to provide yet another explanation for the failure of socialism or the triumph of liberal-capitalism. Rather, he sets out to demonstrate the almost apolitical impact of post-Civil War corporate reorganization of American social-economic life. In the course of his discussion he encompasses such familiar themes as "response to industrialism," "triumph of conservatism," "search for order"; but his essential thesis is that in the welter of ideas—from socialism to liberal-conservative corporatism—there is a common source. That source is experience with and observation of the developing forms of the American corporation. Thus the intellectuals who struggled to extract, or impose, meaning and purpose had as their connecting link an appreciation of the need to accept or mold collectivism. They might argue about the exact role of the state, but not really about the inevitability of collectivism. The line from an Edward Bellamy or a Laurence Gronlund to a James Burnham or an A. A. Berle becomes clear.

Gilbert shows us why American political categories have been blurred, why Americans have gone back and forth across "political" lines, and why varieties of collectivist thinking have been

much more important than the traditional gloss of liberalism in the shaping of contemporary America. His discussion of inevitability and of the role of intellectuals, if occasionally contentious, is always illuminating. Perhaps Gilbert's most important contribution is increasing our understanding of the indigenous sources of American social-political thought that, ironically, improves considerably the comparative perspective in American history.

KENNETH MCNAUGHT
University of Toronto

THOMAS R. COX. *Mills and Markets: A History of the Pacific Coast Lumber Industry to 1900*. (The Emil and Kathleen Sick Lecture-Book Series in Western History and Biography.) Seattle: University of Washington Press. 1975. Pp. xx, 332. \$17.50.

CHARLES E. TWINING. *Downriver: Orrin H. Ingram and the Empire Lumber Company*. Madison: The State Historical Society of Wisconsin. 1975. Pp. ix, 309. \$17.50.

These two volumes are studies of the late nineteenth-century lumber industry, and both are expansions of dissertations. Thomas R. Cox examines the companies of the littoral areas of the three contiguous West Coast states and British Columbia. Charles E. Twining confines himself to one firm in Eau Claire, Wisconsin, emphasizing its downriver marketing activities. The works are of high quality—Cox received the Emil and Kathleen Sick Award from the University of Washington—but both have shortcomings.

A major fault in Twining's study is the scant attention given to sources other than Empire Lumber Company papers. There are only three references to other manuscript collections. I waited in vain for some comparisons between the Empire Lumber Company and its neighbor, the Daniel Shaw Lumber Company. This narrow focus makes it difficult to evaluate Ingram's policies or to understand the company in its historical context. Only one of Ingram's contemporaries, Frederick Weyerhaeuser, is considered in the study. I have not visited Wisconsin county courthouses, but, judging from experience in other states and from the descriptions in WPA inventories of Wisconsin county resources, many relevant documents were not used.

Cox's purpose is to "provide insights . . . into the forces . . . forging the modern, industrial United States" (p. xii). With this goal, the slight treatment given the subject of land is difficult to defend. If technological changes warrant a chapter, the problems of resource access that forced many of these innovations are worthy of greater consideration. On the same point, the extent and location of lumber company land holdings exerted

an influence on regional development and remain a significant economic and social factor; this is a major omission. I am also dissatisfied with the abbreviated treatment of labor. Cox notes that expanded coverage would have meant writing a different book (p. xii). But an understanding of the industry notorious for labor strife is impossible without an explanation of its attitudes and policies toward labor.

Cox is at his weakest on the details of the industry. For example, the first logging railroad was put into operation in the Puget Sound Basin in 1876, not in 1881 (p. 218), George A. Meigs did not restore the Port Madison mill to solvency in the late 1870s (p. 109), and sawdust was blown into furnaces to increase efficiency, not just to speed disposal (p. 237).

Neither author uses econometric methods, nor does either fully use the available statistical evidence. Twining lists without comment Corps of Engineers figures for log and lumber movements on the Chippewa River. If the figures are incorrect, their inaccuracy should be noted; if they are reliable, they should be used more fully. Cox views available statistics with skepticism but fails to seek alternatives. The House Report on Internal Commerce during the Fifty-first Congress would have been helpful in evaluating these data. Cox apparently did not consider substituting shipping tonnage for board feet as a more accurate indicator of volume.

These complaints aside, both volumes are valuable additions to the literature on the lumber industry. Twining has contributed a rare view of the internal workings of a frontier enterprise. Cox has sketched the broad outlines of important aspects of the West Coast industry in a manner that will help scholars place localized studies in proper context.

THOMAS F. GEDOSCH
The National Archives

CHARLES FIERCE LEWARNE. *Utopias on Puget Sound, 1885-1915*. Seattle: University of Washington Press. 1975. Pp. xiv, 325. \$12.50.

In this book Charles Pierce LeWarne traces the rise and fall of five communitarian colonies located in western Washington during the Populist-Progressive era. These experiments were the Puget Sound Co-operative Colony, Equality, Freeland, Burley, and Home. Although they all exemplified the revival of utopianism at the end of the nineteenth century, each venture had varied initial objectives. Puget Sound Co-operative members desired communal living; Equality's founders wanted to "capture" Washington state for socialism; Freeland residents sought cooperative merchandizing based on semicapitalistic principles;

Burleyites dreamed of the "Co-operative Brotherhood"; and Home colonists hoped to create a land-holding institution where individuals with unorthodox views might live in peace.

Paralleling other contemporary colonies, these Washington state utopias were secular in nature and practiced some form of economic cooperation. And like the others, they proved ephemeral. The reasons for their demise fit a common pattern. Internal strife, which decimated so many experiments throughout the annals of American utopianism, struck the Puget Sound colonies. They also, according to LeWarne, "failed to create the sense of commitment that might have spelled greater success or endurance."

This detailed work is a fascinating study of obscure utopias. The overall scholarship is superb, the style crisp. The book, however, is weakened slightly by LeWarne's scanty treatment of why utopian colonies arose at this particular time. He notes in his conclusion that "the rise of communitarianism in the 1890s was in part a response to the severe depression," yet he never fully explores this apparently valid interpretation. There is another flaw. Although LeWarne attempts to trace the interrelationships between the Puget Sound utopias and other contemporary colonies, he might have examined more closely the reform-utopia press. Papers like the *Altruist*, *Integral Co-operator*, *Progressive Thought* and *Dawn of Equity*, *Co-operative Reform*, and *Co-operation* reveal the often close connection between the various late nineteenth-century utopias. For example, several members of the Freedom Colony in Kansas moved to Burley, while some residents of the Puget Sound Co-operative Colony joined the Colorado Co-operative Company. Nevertheless, LeWarne has produced a major contribution to the understanding of a largely neglected phase in the history of American utopianism.

H. ROGER GRANT
University of Akron

KENNETH M. ROEMER. *The Obsolete Necessity: America in Utopian Writings, 1888-1900*. Kent, Ohio: Kent State University Press. 1976. Pp. xiv; 239. \$10.00.

Since the decline of the myth-symbol school, there has been no widely accepted approach to the problematical relationship of literature and culture within American studies. Kenneth Roemer's examination of utopian writings is part of a recent attempt within the discipline to formulate a new approach to the problem. He acknowledges that high culture does not necessarily reflect popular attitudes. He also recognizes the difficulties of assuming that popular literature mirrors cultural attitudes. Roemer's methodological solution is a

"multilevel" analysis of utopian writings. Examining the average, the popular, and "the best" works, he argues that if they shared some values, these values were reliable indexes of popular attitudes in two main ways. First, what authors kept in their utopias pointed to the values that middle-class Americans most cherished, a homogeneous population and traditional sex roles, to name only two. Second, when utopian writings at all levels showed ambivalence toward change, this indicated ambivalence within the culture. Utopian works typically desired change while valuing stability and order. This ambivalence permeated their treatment of many topics, especially the city.

Such a schematic summary cannot do justice to Roemer's ability to relate this paradox to the social context. Nor can it explain the way in which he links utopian writings to the problems that their middle-class WASP authors were coping with. Neither the larger context nor the writers' intentions are slighted in favor of literary analysis. Roemer does not always clarify, however, whether utopian works mirrored the values of the group to which the writers belonged or those of Americans generally. He seems uncertain whether the culture was unified enough to permit the latter to be true. This leaves open the question of how a culture should be conceived. Many people in American studies have abandoned the holistic view, and one wishes that the author had been less ambiguous about this issue. Aside from this, Roemer presents a method for relating literature and society, ideas and culture, that has wide applicability.

JEAN B. QUANDT
The Institute for Research in History

ROBERT HESSEN. *Steel Titan: The Life of Charles M. Schwab*. New York: Oxford University Press. 1975. Pp. xvi, 350. \$14.95.

Robert Hessen has written a strong, affirmative study of the business career of Charles M. Schwab. He portrays Schwab, a leader in the American steel industry from the 1890s to the 1930s, as an outstanding example of what a man of drive and ingenuity could accomplish in a free enterprise society. The best portions of the biography coincide with the creative phases of the steel magnate's life. The author deftly traces his swift rise to pre-eminence in Carnegie's empire; Schwab's skill in restoring full production at Homestead following the Great Strike of 1892 marked a high point in his career and in the book. Yet Schwab's resourcefulness in helping form U.S. Steel in 1901 was not matched by his ability to seize and hold control. In surrendering the presidency of that firm in 1903 he suffered the signal defeat of his career. The second half of the biography centers on Schwab's restruc-

turing of Bethlehem Steel and elevating it to second place in the industry. Here Hessen makes a convincing case for Schwab's talents as an entrepreneur.

Some of Hessen's interpretations will raise objections. Schwab was repeatedly involved in questionable business ventures—the armor plate scandal, the shipbuilding scandal, and the Bethlehem executives' bonus plan, to name but three. For each Hessen sharply slaps Schwab's wrists. He chides Schwab for lacking candor and for defending too feebly his property rights against meddling officials. In the end, Hessen declares, the cases against Schwab were "not proven." From the author's straightforward presentation of evidence on both sides, readers will be able to reach their own conclusions.

In the area of labor relations Hessen objects to charges that Schwab exploited his workers. "He was the one indispensable man . . . He was the entrepreneur, the *Arbeitgeber*—the work-giver, the man who created jobs" (p. 198). The author apparently sees nothing inconsistent in praising Schwab as a bold innovator when he departs from industry-wide practice in quest of profits, while excusing him for slavishly adhering to the industry's low standards for labor. The author similarly accepts Schwab's arguments—at different times—for paying enormous bonuses to company executives and for cutting tonnage rates for workers as "incentives" to greater production.

These views to one side, Hessen has produced an excellent book based on extensive research in public and private archives, including those of Bethlehem Steel. The biography is informative, insightful, and a pleasure to read.

GERALD G. EGGERT
Pennsylvania State University

LOUIS R. HARLAN *et al.*, editors. *The Booker T. Washington Papers*. Volume 4, 1895–98. Urbana: University of Illinois Press. 1975. Pp. xxx, 593. \$17.50.

This fourth volume of the Booker T. Washington Papers, edited by Louis R. Harlan and his associates, contains 491 items, including correspondence, speeches, newspaper and magazine articles, a diary excerpt, a grocery receipt, and a student petition. The documents range in length from a single-line telegram announcing the death of Washington's stepfather on January 29, 1896, to an eight-page discussion of industrial education by Washington which appeared in the *Independent* two years later. The volume opens with James Creelman's New York *World* account of the Atlanta address of 1895 which catapulted Washington into national prominence and ends with a letter from

Collis P. Huntington in December 1898 promising substantial gifts to Tuskegee Institute. The recognition accorded Washington during these years, including an honorary degree from Harvard and a visit to his school by President William McKinley, signaled his emergence as the pre-eminent black man in America. This volume provides rich insight into the multiple roles he played in his rise to power.

A majority of the documents relate to Tuskegee Institute which Washington still considered the principal base of his operations. With the aid of Northern white millionaires he continued to expand the faculty, student body, and physical facilities of the school. Deference may have characterized his relations with whites, but Washington reigned as a despot on the Tuskegee campus where his "will and policy" were "carried out in the remotest corner of the school" (p. 145). He scrutinized every detail of the institution's operation and promptly disciplined faculty and students who violated his prescribed code. The documents relating to the dismissals of Edward H. Wilson and William Jenkins, two faculty members charged with improper conduct toward female students, underscore both the extent of Washington's authority and the zeal with which he protected the reputation of the school.

Since Washington increasingly became involved with black concerns beyond Tuskegee in the years 1895–98, the papers in this volume represent, as the editor suggests here, "an odyssey of the colorline." With the aid of T. Thomas Fortune, a New York journalist, and Emmett J. Scott, a Texas editor who became his secretary in 1897, he acquired strategic allies in black communities throughout the nation. Always conscious of his "peculiar" position, Washington avoided too close identification with black organizations and utilized conservative language even in protesting the disfranchisement of Louisiana blacks. Only once, in 1898 at the Chicago Peace Jubilee, did he attempt to go beyond the race-relations formula of his Atlanta speech, but the hostile criticism from the South prompted a hasty retreat.

As with previous volumes, this one is a model of imaginative and meticulous editing. The extensive notes not only serve the customary purposes of identification and explanation but also dissect myths such as those surrounding George Washington Carver. Clearly the editor has adhered to his original aim of utilizing the Washington Papers to reveal the complexity of American race relations and the richness of black culture in the late nineteenth and early twentieth centuries.

WILLARD B. GATEWOOD, JR.
*University of Arkansas,
Fayetteville*

GERALD F. LINDERMAN. *The Mirror of War: American Society and the Spanish-American War*. Ann Arbor: University of Michigan Press. 1974. Pp. viii, 227. \$10.00.

The Mirror of War merits more than this short review. It displays a great deal of research, though the author sometimes relies too heavily on such doubtfully representative sources as Carl Sandburg's and Sherwood Anderson's war memoirs to advance his ambitious thesis. Gerald Linderman usually writes gracefully; he also manages at times to produce prose as hard to grasp as shaving cream and phrases such as "pinched in pride and pocketbook by these processes" (p. 4).

But what does his book say about American society during the Spanish-American War? Following up some of Robert Wiebe's perceptive arguments, Linderman proposes that the United States in 1898 was in the throes of fundamental transformation: that "localities" and old-fashioned moral precepts were giving way to a dominant "center" (Washington) and more complex and sophisticated ideas; that Americans generally strove to retain the pre-industrial "old consensus" in the crusade against Spain; but that they ultimately managed only to delay the full ascendancy of the new order.

Through a set of loosely connected essays, Linderman puts forward this thesis in his individual studies of Senator Redfield Proctor's electrifying speech of mid-March 1898, McKinley's decision to go to war, the relationship between small-town communities and their own hometown soldiers, the meaning of the war to individual combatants, American perceptions of their Spanish enemies and Cuban "allies," and the role of the popular press in bringing about and reporting the war.

By no means flawless or without distortions, *The Mirror of War* commands one to look anew at how Americans saw themselves and others three generations ago.

ROBERT L. BEISNER
American University

WILLIAM M. ARMSTRONG, editor. *The Gilded Age Letters of E. L. Godkin*. Albany: State University of New York Press. 1974. Pp. xxiv, 583. \$30.00.

ARTHUR A. EKIRCH, JR. *Progressivism in America: A Study of the Era from Theodore Roosevelt to Woodrow Wilson*. New York: New Viewpoints. 1974. Pp. x, 308. Cloth \$10.00, paper \$4.95.

WILLIAM L. O'NEILL. *The Progressive Years: America Comes of Age*. New York: Dodd, Mead & Company. 1975. Pp. xi, 166.

Edwin L. Godkin, the pungent libertarian and "reform" editor of the *Nation* and the New York

Post, worried from time to time that a democracy too impatient to solve social problems might mortgage its future to politicians, party government, and state power. At the end of his life, indeed, a national enthusiasm for strong government to achieve Progressive reforms had begun to transform the mild liberalism that Godkin had so vigorously defended. William O'Neill and Arthur Ekirch address themselves to this modernizing transformation, and though both historians share the current conventional judgment that Progressives were needlessly cautious and compromising in the use of political power for social ends, they depict the resulting decline of the liberal ideal, paradoxically, in terms that may remind readers in the libertarian 1970s of the dark forebodings of a Gilded Age editor nearly a century before.

Armstrong offers a comprehensive edition of the several hundred surviving, private letters that Godkin wrote between 1864 and his death in 1902. Woven into the text are succinct annotations of biographical comment that make this the first reliable account of the fearless, self-righteous editor's complex relations with his staff, his backers, and his circle of elite friends. With this book and his previous one, Armstrong has established himself as Godkin's only scholarly biographer, though his insights do not match those of Geoffrey Blodgett's recent penetrating essay on Mugwump politics, and the reader who would best understand Godkin's place in American letters should turn also to Morton Keller's 1966 reissue and brief, balanced assessment of Godkin's principal essays on democracy.

Both O'Neill and Ekirch offer a compressed synthesis of established scholarship as a way of fastening a unifying theme on the Progressive era. At first sight O'Neill's appears the more promising. He makes use of more recent findings, particularly on the origins of reform and its actual results in politics, culture, and the economy. His prose is sardonic, witty, and casual, and it enhances a rather simple theme: Progressives aimed sometimes at reform but principally at modernization, and despite a degree of commitment to the general welfare, their programs benefited mainly the middle and upper classes. Those familiar with the major works of Hays, Wiebe, Thelen, Hofstadter, Mowry, Link, and Blum, whose findings O'Neill openly acknowledges and summarizes, will find nothing here to startle them. His observations on foreign policy, however, succumb to clichés; social thought he treats with surprising superficiality; the European context he mentions not at all; and of the many strands of history that he singles out for comment, only regulation, resource management, political reform, and wartime mobilization does he successfully connect to his theme.

Ekirch's version of the story is not always convincing but is more original and provocative. It pictures the fulfillment and decline of a liberal society; by the 1900s, in response to accelerated economic growth and social change, liberals everywhere came to rest their hopes for progress on the national state. Americans were no exception. Many had thought that their palpable edge in abundant resources, their geographical security, and their diffused political structure would permit them to escape centralization of power or at least soften its effects. It was not to be. The forces that were reshaping the liberal societies in Europe engulfed the United States with equal intensity and were unmistakably erasing the distinctions among them. By 1914 national progressivism had triumphed over the older liberal ideal of a decentralized power.

Ekirch is not very clear about the nature of the relationship between the liberal and nationalistic impulses that are critical to his study. He permits one to infer that nationalism was a product of the very forces that gave life to liberalism—a rational drive for social control in an age of profound social disturbance—and that nationalism alone made it possible for liberalism to survive in the modern era. This was a view that Godkin shared with his contemporaries in the Civil War era. But there is also in Ekirch's account a hint of an alternative relationship, and a more fateful one: nationalism gave form to atavistic impulses rooted in historic human behavior; against those impulses the ideals of liberals, including the ideal of international peace, could make no headway. Ekirch unfortunately does not explore either relationship and renders instead a less-complex judgment: the failure of Progressives to fulfill liberal ideals lay in their unwillingness to set firm, precise, and effective limits to national power both at home and in the world. This unwillingness, he would have us believe, made it fatally easy for the United States to accept war as an instrument of national policy. In conclusion, it is possible that the nation could not have done otherwise, but again perhaps it could have; and what a pity it did not try.

Ekirch's judgment is ironic for it invokes the very "exceptionalism" for the American people that he has been at such pains to demonstrate was never within their power to sustain. That was precisely the liberals' dilemma in denouncing American imperialism in the 1890s as a betrayal of democratic ideals. Neither Ekirch nor O'Neill finds much to admire in the political and social prejudices of Godkin's Mugwump readers, and in this they speak not only for their own times but for the Progressive generation itself. It is nevertheless instructive to note in the work of two modern historians echoes of the contradictions in the thinking of

an old liberal reformer concerning America's historical uniqueness and, in consequence, more than a trace of his moral indignation at the road not taken.

OTIS A. PEASE

University of Washington

ROBERT SHERMAN LA FORTE. *Leaders of Reform: Progressive Republicans in Kansas, 1900-1916*. Lawrence: University Press of Kansas. 1974. Pp. vii, 320. \$11.00.

This valuable study of the emergence and performance of Progressive Republican leaders in Kansas adds to the political history of that state and enriches our knowledge of progressivism on the national scene. In the arresting first chapter the author characterizes the nature of the movement in Kansas, compares it with its counterpart in other states, and shows its relationship to the earlier populist impulse. La Forte shows a thorough grasp of his subject and presents it in a restrained, persuasive, direct manner.

La Forte points out that rising economic expectations on the part of the smaller economic interests desirous of a larger share of the growing profit opportunities certainly contributed notably, but not exclusively, to the growth of the Progressive movement in Kansas. He writes that "just as important in creating unrest were party factionalism, ambition for office, and a sincere interest in furthering democratic idealism." He also calls attention to differences in the origins of progressivism. In California and Ohio it began in urban centers and moved upward to the state level, but in Kansas it began with state, rural-oriented leaders and never penetrated far into the cities. He is cautious in his evaluation of contributors to the movement. While referring to editor William Allen White as "one of the finest specimens" of the Progressive Republicans in the state, La Forte adds that "whether White and his associates were responsible for improved conditions in Kansas is, of course, a moot question." There were other groups, he reminds us, such as Democrats, regular Republicans, and a few Socialists, who also contributed to the enactment of reform measures.

Although La Forte's description of Kansas Progressives on the national scene and his observations on national leaders are brief, they enrich our understanding of the politics of the era. He remarks, for example, that Congressman Victor Murdock's intense effort to help unseat Speaker Joe Cannon sprang in part from that Kansan's having "felt uncomfortable as an unnoticed member of the House." The material on William Howard Taft helps sustain his reputation as a truly bumbling politician. Theodore Roosevelt emerges

as a politician who could perform persuasively but not always with wisdom. La Forte says Roosevelt's speech at Osawatomie, Kansas, in 1910 was the "most momentous delivered by a Republican during the Progressive Era," but of the Roosevelt-led party split in 1912, he states that it is fair to say that the "majority of progressive Republicans in Kansas were sore at heart about what had happened."

The only conspicuous flaw in this admirable study is its inadequate index.

HORACE SAMUEL MERRILL
*University of Maryland,
College Park*

JOHN EVANGELIST WALSH. *One Day at Kitty Hawk: The Untold Story of the Wright Brothers and the Airplane*. New York: Thomas Y. Crowell Company. 1975. Pp. x, 305. \$10.00.

This would be a much better book if the author was not so insistent on grinding an axe. He is convinced that history has been unfair to Wilbur Wright, largely because Wilbur died early (1912) and never told his own story. The thesis is that Wilbur was the genius behind the achievement of powered flight, with Orville in a minor role, but that Orville, who outlived his brother by thirty-five years, allowed a version of the story to circulate that exaggerated his role and suppressed the evidence that would have shown the truth.

Although John Evangelist Walsh has put together a great deal of information, none of it original or unknown, he does not really know how to use it. It does not help that there are no footnotes. There is a section of notes at the end of the text, divided by chapters, but it is not related to the text in any way. Thus it is impossible to relate the data and claims in the text to the sources of evidence listed.

The best example of the author's fuzziness is in the elaborate argument in chapter 10, "High Noon at Kitty Hawk." Walsh contends that the fourth and longest flight on December 17, 1903, Wilbur's fifty-nine second effort, was actually man's first powered flight, thereby eliminating Orville, who had made the first and third flights. Yet in the very next chapter Walsh quotes the telegram sent to Katharine Wright: "Success four flights Thursday morning—." Evidently both the brothers believed they had made four successful flights.

The Wrights and their achievement are a fascinating story, which can well stand retelling. But Wilbur Wright does not need this kind of vindication. His place in history is too secure to require denigration of Orville, as well as of Chanute, Langley, Farman, Blériot, and other great pioneers of flight.

JOHN B. RAE
Harvey Mudd College

ROBERT D. SCHULZINGER. *The Making of the Diplomatic Mind: The Training, Outlook, and Style of United States Foreign Service Officers, 1908-1931*. Middletown, Conn.: Wesleyan University Press. 1975. Pp. 237. \$15.00.

In recent years historians have become more aware of the organizational dimensions of twentieth-century American history and have begun to study the great private and public bureaucracies that dominate our lives. The results of this scholarship have already profoundly affected our view of the American past, shifting attention away from the preoccupations of liberal history toward an examination of the underlying structure of American society. This new organizational approach has become a powerful force in the writing of American history.

Historians of American foreign policy have become conscious of the need to understand the role of bureaucratization and of professionalization in the shaping of American diplomacy. This study is an ambitious attempt to analyze the "collective outlook" of the first generation of professional diplomats, those who entered the foreign service in the early years of the century and rose to influential positions in the 1920s. Drawing on a wide range of manuscript collections and contemporary literature, Schulzinger discusses the emergence of a professional mentality, the campaign for foreign service reform, the training of diplomats, and the sources of their views on world politics. He offers some brief biographical sketches, analyzes the ideas of prominent scholars such as Archibald Carey Coolidge and Paul S. Reinsch, describes the infighting within the State Department in the 1920s over foreign service reform, and explores the tensions that professionalism brought. He argues that career diplomats in the 1920s were realistic in their assessment of international affairs and convinced of the importance of their role in the maintenance of international stability. Though these diplomats constantly sought to explain their unique expertise, they could not define the essence of this "professional mystique." They also failed to convince politicians that diplomacy was the exclusive preserve of diplomats, nor could they strike a satisfying balance between the broad perspective they valued so highly and the growing need for specialization and technical skills.

Despite broad research and frequent insights, this is not a successful book. In seeking to define the "diplomatic mind" for a whole generation of diplomats, Schulzinger reaches too far and produces an inconclusive and fragmented study, not a convincing collective portrait. Lacking the methodology to cope with such a large and difficult question, he often strays from his central purpose. This book is premature, for much of the monographic literature and most of the biographies on

which a synthesis of this scope must rest have not been written. Of all the prominent diplomats mentioned, only a few have been the subject of full scholarly biographies; for the rest we must rely on memoirs, dissertations, or personal papers. The task of exploring such scattered and voluminous sources is huge, and Schulzinger misses some dissertations and ignores rich manuscript collections, such as those of William R. Castle and John V. A. MacMurray. Moreover, the dearth of specialized studies causes him to blur important distinctions and to ignore, for example, the formation of groups of regional experts. In the end, he offers a portrait that is not entirely satisfying either in explaining the beliefs and attitudes that united these diplomats or the differences in background and experience that separated them. The task of comprehending the whole process of professionalization and bureaucratization that diplomats experienced is monumental.

CHARLES E. NEU
Brown University

DICKRAN TASHJIAN. *Skyscraper Primitives: Dada and the American Avant-Garde, 1910-1925*. Middletown, Conn.: Wesleyan University Press. 1975. Pp. xiii, 283. \$20.00.

"Anarchic, protean, subversive, international—however accurately characterized—Dada remains illusive," says Dickran Tashjian of this avant-garde rebellion against art and culture. Beginning during World War I in Zurich, Dada flourished in its nihilistic aftermath as such extravagant individualists as Marcel Duchamp, Tristan Tzara, Louis Aragon, and André Breton assaulted European sensibilities with their fervent manifestoes, outrageous antics, consistent controversy, and calculated attacks on what Dadaists regarded as the fundamental bankruptcy of Western civilization. Deliberately irrational, frenetically effervescent, the whole phenomenon was shortlived and, according to conventional wisdom, left the United States virtually untouched.

Challenging this last assumption in a comprehensive study of the impact of Dada on a segment of the New York avant-garde, Tashjian explores in the process the works of visual artists such as Joseph Stella, Charles Sheeler, Man Ray, Stuart Davis, and Charles Demuth; the poetry of E. E. Cummings, Hart Crane, and William Carlos Williams; and the little magazines of the period—*Camera Work*, *Broom*, *The Soil*, *Contact*, and *Session*. Fed up with traditional values of the academy and impressed with the merits of "straight" photography as advocated in Stieglitz's *Camera Work*, some young American modernists had already begun attacking prevailing cultural definitions of art before the arrival of Duchamp and Picabia about the

time of the celebrated Armory Show. What the Europeans accomplished was the further de-emphasis of physical technique and traditional, even meaningful, content. Thus by enhancing American receptivity to the Dadaist antiart stance, they helped to liberate the New York avant-garde.

Catalytic as well as liberating, Dada—more precisely Picabia and Duchamp—also stimulated interest in American technology and popular culture. To be sure, Crane, Sheeler, Demuth, and others would undoubtedly have discovered this new urbanized, industrialized environment without the help of Europeans. Tashjian is too careful a scholar to make exaggerated claims of Dadaist influence. Of the artists, he admits that only Man Ray became a full-fledged Dadaist; by the same token, he acknowledges that Williams was indeed a vocal opponent of Dada, but he also insists that Williams' preoccupation with creating an American art that was new came out of his "abrasive immersion in the contradictions of Dada." Perhaps so, but there were for Williams, as for most other artists and intellectuals at the time, many other forces at work. One wishes the author had dealt with them, thereby placing Dadaist influence in a richer historical context that, in turn, would have illuminated more fully the problems and preoccupations of the American avant-garde.

Nevertheless, within the limitations he has chosen, Tashjian has made an important contribution to our understanding of this particular facet of American cultural history; in the process, he has demonstrated impressive mastery of visual and literary materials too often ignored by the less-versatile historian.

JANE DE HART MATHEWS
University of North Carolina,
Greensboro

W. ELLIOT BROWNLEE, JR. *Progressivism and Economic Growth: The Wisconsin Income Tax, 1911-1929*. (National University Publications.) Port Washington, N.Y.: Kennikat Press. 1974. Pp. 154. \$10.00.

Tax enforcement in America has led to the Boston Tea Party, the Whisky Rebellion, and the conviction of Al Capone. In *Progressivism and Economic Growth*, W. Elliot Brownlee, Jr. statistically analyzes the Progressive-inspired 1911 Wisconsin corporate income tax and its two-decade impact. He states that Wisconsin "contributed the first comprehensive and effectively administered income tax, served as a model for other states and for the federal government in the search for tax systems more appropriate to a maturing industrial order, and, until the Great Depression, provided the only example of a system of state income taxation that relied heavily on the taxation of corporate profits." The author based his exhaustive research primar-

ily upon Wisconsin corporate income tax returns, the Edwin Witte Papers, and the Thomas S. Adams Papers.

Brownlee shows excellent command of Wisconsin's 1900-30 political intricacies. Robert La Follette and the Progressives attempted to create what the author terms an "agricultural service-state" in Wisconsin. Rural Progressive legislators passed the corporate income tax. The Wisconsin Manufacturers Association and the Milwaukee-based Socialist legislators, who sided with their constituent corporations, strenuously objected to the tax. The Progressive Republican governor, Francis McGovern, won re-election in 1912 by charging that corporate resistance proved the validity of the old Populist shibboleth that industry did not want to pay its fair share of taxes. Even the stalwart Republican governor, Emanuell Philipp, a Milwaukee industrialist, doubled the surtax on corporate incomes in 1919. The Progressive Republican governor, John Blaine (ironically a future RFC official), raised the corporate income tax in 1925 to finance increased state aid to economically depressed rural areas. Brownlee concludes that the Progressives and manufacturers must share responsibility for retarding "the course of industrialization and, in turn, economic growth" in Wisconsin. Progressives instituted the high corporate income tax that impeded economic development. Manufacturers resisted the tax so vigorously that it remained a major Wisconsin political issue for two decades and led to strict enforcement by the Wisconsin Tax Commission.

RICHARD C. HANEY
University of Wisconsin,
Whitewater

ALAN REITMAN, editor. *The Pulse of Freedom: American Liberties, 1920-1970s*. Foreword by RAMSEY CLARK. New York: W. W. Norton and Company. 1975. Pp. 352. \$12.50.

Alan Reitman is associate executive director of the American Civil Liberties Union (ACLU), and this book contains essays tracing the history of civil liberties and civil rights in this country since the ACLU's inception in 1920. It is not a substitute for the scholarly history of the ACLU that has long been needed, but it is a fine introduction to the Union's major achievements and a good summary of the often dramatic developments witnessed in the field of human rights over the past half century.

Excluding a silly exercise in platitudes by Ramsey Clark, the essays are of generally high quality. Paul L. Murphy's examination of the 1920s is solid and reliable although a bit uninspired. He is especially effective in depicting conditions leading to the creation of the ACLU.

Jerold S. Auerbach devotes much of his exhilarating essay on the 1930s to a discussion of an internal dispute over the exclusion of Communists from ACLU leadership, a dispute that led to the expulsion of Elizabeth Gurley Flynn from the board of directors in 1940. Auerbach is sharply critical of the anticommunist policy that emerged. This is the only negative commentary on the ACLU in the book, and Reitman felt obliged to include not overly convincing rebuttals by Roger Baldwin and Osmond K. Fraenkel.

The 1940s are discussed by William Preston, Jr. His analysis is heavily polemical and at times emotional, but he covers the ground well and depicts the emerging red scare late in the decade with eloquence and understanding.

John W. Caughey's well-written essay on the 1950s is entitled "McCarthyism Rampant." Although it avoids most of the deeper questions about the meaning of McCarthyism, it depicts the full range of the second Red Scare and provocatively assesses its damage. Liberals would do well to have this piece in mind the next time they are challenged to discuss exactly what harm McCarthy and his associates caused. Curiously, the author's bibliography is uncritical and out of date.

Perhaps the best article in this volume is by Milton R. Konvitz. His lengthy essay on the 1960s vividly recounts the startling changes in American life produced within this decade of turmoil and tragedy. The author may be criticized for his unswerving, almost naive liberalism. Still, this is a superior example of how to write very recent history, and it deserves wide reading.

Alan Reitman's long and at times tedious conclusion reminds us that this book, for all its scholarly contributions, was designed as a polemic. Reitman equates the ACLU's work with eternal truth. The bad guys—and they often include almost all of the American people—are those who oppose this truth. He pleads for "a truly free and open society." One wishes that he would be more specific about the contents of that society, and one wonders about the role of the much-maligned majority within it.

THOMAS C. REEVES
University of Wisconsin,
Parkside

KEITH L. NELSON. *Victors Divided: America and the Allies in Germany, 1918-1923*. Berkeley and Los Angeles: University of California Press. 1975. Pp. xiii, 441. \$20.00.

America's persistent efforts to moderate Allied enforcement of the Versailles peace agreements following the Senate rejection is one of the anomalies in recent American diplomacy and has received

only limited study. This book concerns the continuation of the American Army of Occupation along the Rhine, and it fully documents the conflicting views of House, Allen, Poincaré, Erzberger, and others during the Wilson period. Harding's policies are less thoroughly treated, and some flaws appear. Secretary Hughes, not Army Secretary Weeks, had become the decisive voice. Hughes was most interested in the growing crisis, but having no official representation on the Reparation Commission, and Rhineland High Commission, and other League commissions, his means of action were seriously limited. He therefore allowed French extremism to run its course to prove its futility, which would then permit the wisdom of moderation.

Senators Lodge and New were also not reliable spokesmen for the administration. Too little attention is given the efforts of Harding and Hughes to overcome Senate obstruction to their moderate course—the use of executive agreements rather than Senate-approved treaties and dilution of the Knox peace resolution. The question of the legality of the Army of Occupation under the Berlin Treaty is also avoided as well as the supplementary agreements used to temporize Allied excesses. The author should also have explained that the withdrawal of the Army of Occupation did not mark the end or even the decline of the American effort to temporize Allied excesses, including the French-Belgium occupation of the Ruhr. Withdrawal only marked a shift in policies that finally restrained Allied excesses, as reflected in the Dawes Plan for payment of American reparation claims and the costs of the Army of Occupation. The restraining influence of the United States was also evident in the Wadsworth, Dawes Plan and other agreements, which called for the restoration of German viability and reintegration and for relief from further Allied sanctions (occupation and so forth) that had finally come to be what the Army of Occupation was all about.

MITCHELL W. KERR
Towson State University

MARGARET SZASZ. *Education and the American Indian: The Road to Self-Determination, 1928-1973*. Albuquerque: University of New Mexico Press. 1974. Pp. xviii, 251. \$10.00.

The influential Meriam Report (1928) redefined the function of the Indian Bureau as primarily educational, and it advocated basic changes in Indian schooling. Its recommendations won the support of all the major reform forces, new and old, in Indian affairs. The progressive educational measures proposed included community day schools, active Indian participation, the elimination or reorganization of boarding schools, and a

flexible curriculum suited to varying tribal needs. Under Commissioners Rhoads and Collier and their directors of education, the eminent progressive educators W. Carson Ryan, Jr. and Willard W. Beatty, the Bureau attempted to implement these policies. A study of this significant experiment has long been needed.

Education and the American Indian makes a good, if limited, start. The best sections cover the period up to 1945. Setting her account within the larger context of progressive education and the rise and fall of the Indian New Deal and focusing on the hitherto neglected work of Ryan and Beatty, Margaret Szasz explores sympathetically both the reforms instituted and the reasons for their eventual "failure." Included is a critical evaluation of contracting for Indian children in public schools under the Johnson-O'Malley Act.

No attempt is made to assess the impact of Indian progressive education on the new Indian forces that arose in the '40s and '50s with the emergence of the National Congress of American Indians. Similarly, the influence on Bureau education of the broad array of reform movements, Indian and non-Indian, gets insufficient attention. The changing conceptions of such basic ideas as "self-determination" and "cross-cultural education" are also not examined. Fuller and more integrated treatment is needed of the postwar topics discussed in the book. These include the relationship of the termination policy to education, tribally controlled schools and community colleges of recent date, and Indian organizations specifically concerned with education in the late '60s and early '70s. Szasz's pioneering study will no doubt encourage these further lines of inquiry as well as stimulate other interpretations of the educational consequences of the Meriam Report.

HAZEL WHITMAN HERTZBERG
Columbia University

PETER TEMIN. *Did Monetary Forces Cause the Great Depression?* New York: W. W. Norton and Company. 1976. Pp. xiii, 201. Cloth \$8.95, paper \$3.95.

Readers of Milton Friedman and Anna Jacobson Schwartz's monumental *Monetary History of the United States* cannot fail to have been impressed by the compelling narrative and the resourceful display of evidence to support their monetary interpretation of cyclical disturbances in the U.S. economy since 1867. Nevertheless, two questions continue to haunt the reader: Is the Friedman and Schwartz interpretation historically valid? And are their equally persuasive nonmonetary explanations of inflations and depressions? Regrettably, economists and economic historians inspired by Keynesian and post-Keynesian economic theory

have failed to do their historical homework. There is no comparable study by the Keynesians that matches, either in scope or analytical achievement, the study by Friedman and Schwartz.

Peter Temin's new book attempts to fill this lacuna. He restricts himself, however, to a single episode: America's Great Depression from 1929 to 1933. He conveniently simplifies his task by sharply contrasting two rival hypotheses about the causes of the Great Depression—"the money hypothesis" and "the spending hypothesis." The "money hypothesis" associated with Friedman and Schwartz attributes the depression to a sharp reduction in the stock of money resulting from the collapse of the banking system. The "spending hypothesis" has its roots in Keynesian and post-Keynesian income and expenditure theory (of textbook fame) and assigns causal significance to changes in autonomous spending (investment, consumption, or government). In brief, Temin proposes to discriminate between the two rival hypotheses by deducing relevant theoretical implications and designing appropriate statistical tests to measure the presence or absence of these anticipated effects.

He concludes that the "money hypothesis" must be rejected for lack of evidence. Instead, he suggests that the data are consistent with the hypothesis that the demand for money was falling more rapidly than the supply of money during 1930 and the first three-quarters of 1931. He finds strong support for the view that there was a substantial and unexplained decline in real consumption expenditures of approximately three billion dollars in 1930, more than enough in his judgment to explain the subsequent severe contraction of income and employment. He is unsuccessful, however, in his efforts to explain the sharp fall in consumption in terms either of the stock market collapse or the poor harvest of 1929. Temin admits that it is "somewhat unsatisfactory to say that the Depression was started by an unexplained event, but this alternative is preferable to statements that are inconsistent with the data" (p. 83).

This book is an important contribution that imaginatively and skillfully employs the craft of the economist to illuminate an event which continues to baffle both economists and historians. Incidentally, it is a fine example of how the New Economic History can be exploited to great advantage when the problem and the approach are mutually compatible.

ELMUS WICKER
*Indiana University,
Bloomington*

SIDNEY FINE. *Frank Murphy: The Detroit Years*. Ann Arbor: University of Michigan Press. 1975. Pp. viii, 608. \$20.00.

The first of a projected three-volume life and times of Frank Murphy, this book is concerned with Murphy's youth and early career as a practicing lawyer, his judgeship on the Detroit Recorder's Court from 1924 to 1930, and his many problems as mayor of Detroit from 1930 to 1933. The present volume is painstakingly researched and remarkably thorough, chock full of detailed information that should interest historians of politics, law, and urban affairs.

Murphy, born in 1890 to Irish Catholic second-generation immigrants, grew up in a small town in Michigan's thumb into Lake Huron. His father was a self-made man, a lawyer and small-time Democratic politician who drank excessively. As a student at the University of Michigan (LL.B., 1914), Murphy clearly developed political ambitions and was active in student affairs, but he remained a mediocre scholar. As a young man he seems to have been highly principled, vastly enthusiastic, and vigorously athletic, and he emerged as a budding young politician imbued with reformist and progressive ideals of the day. He became a successful lawyer quickly. When first running for office at age thirty-three, he had a lucrative practice and had established himself as one of the city's ablest young attorneys despite more than two years' service as a junior army officer.

On the court, Murphy demonstrated eminent fairness as a trial judge, compassion in setting sentences, enlightenment about bail and probation, and efficiency as an administrator. While on the Detroit court he also built his political base. Blacks, the foreign-born, and unionists honored him for his lack of bias, and middle-class reformers were favorably impressed with his one-man grand jury investigation into municipal corruption. A natural candidate for the special election in 1930 required by a recall of the mayor, Murphy won the nonpartisan election handily, largely because of the good majorities he polled in black and ethnic neighborhoods.

The depression hit the Detroit area fast and hard, with factory employment dropping over fifty percent from August 1929 to August 1930; in November 1932 more than half the labor force was unemployed, and many were working only part-time. No large city was stricken worse during the Hoover years. Mayor Murphy recognized early the magnitude of the depression and the need for public relief of the unemployed, and his was one of the first public voices calling for federal assistance for welfare. The mayor's task was impossible, but his biographer is surely right in concluding that Detroit's suffering would have been much greater if Murphy had not been mayor.

Sidney Fine devotes the overwhelming proportion of his book to Murphy's public career, but he presents some fascinating and puzzling material

about the politician's personality and private life. Fine quite properly describes Murphy's correspondence to his mother while he was in the army as "love letters." Very attractive to women, he had dozens of wealthy girl friends in Detroit, as well as the actress Ann Harding; yet he never married. Murphy was a complex person. He appears to have been unusually ambivalent and contradictory. He had sympathy and easy relations with the poor, but he enjoyed his friendships with the rich; as a young man he was simultaneously an Irish nationalist and enthusiastic about "Americanism" and A. Mitchell Palmer; filled with progressive ideals, he frequently disagreed with good government people.

This is a good start on a major biography. Fine admires Murphy, but recognizes his defects. This book is a model of balance and judiciousness.

DAVID A. SHANNON
University of Virginia

WILLIAM ANDERSON. *The Wild Man from Sugar Creek: The Political Career of Eugene Talmadge*. Baton Rouge: Louisiana State University Press. 1975. Pp. xviii, 268. \$11.95.

EDWARD F. HAAS. *DeLesseps S. Morrison and the Image of Reform: New Orleans Politics, 1946-1961*. Baton Rouge: Louisiana State University Press. 1975. Pp. xii, 368. \$12.95.

These books represent the initial efforts of two youthful biographers whose subjects have not previously been treated in complete biographies. Since Eugene Talmadge and DeLesseps Morrison were products of the New South, these studies will be of special interest to students of Southern history. The authors apparently did adequate research, although their sources were markedly different. Edward F. Haas found much written material and made meager use of oral history, while William Anderson was compelled to depend heavily upon interviews with acquaintances of "Ole Gene" Talmadge. The spirited style of both authors holds the reader's interest.

In many ways the subjects of these books present a study in contrasts. Although Morrison was born in a rural community of southern Louisiana, he soon migrated to New Orleans and lived the rest of his life in that rapidly expanding urban center. Talmadge was born in a rural Georgia environment and spent all of his life in small towns or on a farm. Morrison was repeatedly elected as mayor of New Orleans and served in that position longer than any other person in modern times; Talmadge was elected and re-elected governor of Georgia. Morrison's political life was devoted to the solution of problems confronting his urban constituency, whereas Talmadge served the interests of the farming classes.

When Morrison twice attempted political advancement by running for governor, he was defeated. Talmadge was twice unsuccessful in his efforts to represent his state in the United States Senate. Both of these Southern politicians were racists, but only Talmadge was rabidly so. Both of them strongly opposed integration. Although Talmadge opposed the New Deal and fought the increasing power of the national government, Morrison was more amenable to Washington's expanding regulation of the economy and its enlarging bureaucracy.

Both Talmadge and Morrison had personality conflicts with the boards of educational institutions. As governor, Talmadge fired several educational officials and caused the loss of accreditation for Georgia's colleges and the university. Morrison had differences at times with the city educators, but never took steps as drastic as those of Talmadge. The Georgia governor was always a staunch conservative, while the New Orleans mayor sought to create for himself the image of a reformer. He may have had the image, but frequently the reality of reform was lacking.

The contrast between the two men also extends to their religious convictions. Talmadge was a Protestant in a state that was predominantly Protestant. Morrison was a devout Catholic in a city that was largely Catholic. Both men were devoted to their families.

In appearances they were particularly dissimilar. Although Talmadge was a university graduate with a Phi Beta Kappa key, he was sometimes uncouth in manner, careless in dress, and vulgar in language. The immaculately dressed Morrison was an accomplished conversationalist, suave and debonair.

Both authors were successful in setting their subjects against the issues with which they were confronted during their political careers in the New South. As usual, the Louisiana State University Press did a splendid job in the publishing of these books.

GEORGE OSBORN
Gainesville, Florida

BERNARD BELLUSH. *The Failure of the NRA*. New York: W. W. Norton and Company. 1975. Pp. xiv, 197. Cloth \$8.95, paper \$2.95.

At the hands of historians the National Recovery Administration of 1933-35 has fared badly. Cursed at the time, it has remained the epitome of political aberration, illustrative of the pitfalls of "planning" and deplored both for hampering recovery and delaying genuine reform. Only a few planning enthusiasts have viewed it as the bungling of a good idea, and of these only a few admirers of French and Japanese institutions have looked

upon its corporatist aspects as an unfortunately suppressed thrust toward modern state-building.

In the work under review, Bernard Bellush offers a short, well-written, and well-informed summation of the conventional wisdom. In eight chapters he examines the background and passage of the legislation, the negative impact of the codes, the emasculation of the labor sections, and the decline and demise of the program. His story differs little from that in standard accounts, and the conclusions are essentially those long ago reached by liberal historians. The great mistake, he believes, was in turning political power over to "highly organized, well-financed trade associations and industrial combines."

'As a convenient account usable in college courses, Bellush's work has value. It reads well, has a good bibliographical essay, and succeeds in pulling together the stories separately developed by business and labor historians. But for those who expect the Norton Essays to be vehicles for new interpretations or for clarifying interpretive issues, the work will be disappointing. For the most part, it ignores recent challenges to liberal premises and recent "defenses of monopoly" that make NRA economics more understandable. And more surprising, given current interests, it makes no real effort to fit the NRA experience into the larger stories of modern American planning, corporatism, and administrative development.

ELLIS W. HAWLEY
University of Iowa

JOHN BRAEMAN *et al.*, editors. *The New Deal*. Volume 1, *The National Level*; volume 2, *The State and Local Levels*. (Modern America, number 4.) Columbus: Ohio State University Press. 1975. Pp. xv, 341; xiv, 434. \$30.00 the set.

For the third time John Braeman and his coeditors have combined with the Ohio State University Press to give us a useful collection of original essays on twentieth-century America—this time on the New Deal. Volume 1 contains essays by Albert Romasco on the contrasts between Hoover and Roosevelt, James Holt on the New Deal's antistatist critics, Ellis Hawley on the New Deal's relation to American business, Richard Kirkendall on agriculture, Milton Derber on labor, Jerold Auerbach on the legal profession, Raymond Wolters on blacks, John Salmond on Aubrey Williams, Richard Polenberg on the years 1937–40, David Brody on liberal reform during World War II, and Eric Solomon on fiction and the New Deal. One can see from this list that the editors opted for established scholars of demonstrated ability and for syntheses on familiar yet still important problems. Little entirely new ground is broken, but almost without

exception the essays constitute comprehensive, sound, and insightful surveys of their subject. Especially useful are Hawley's piece on the conflict between contending strategies for achieving a workable political economy; Kirkendall's review of agricultural policy, which deftly distinguishes the radical from the conservative thrusts of a multifaceted set of federal activities affecting the farm; Wolters' balanced review of how blacks fared under the New Deal; and Auerbach's illuminating study of the role of lawyers, certainly the most interesting and original essay in the book.

The second volume was designed to present some of the new work on state and local politics and policy in the 1930s that James Patterson's *The New Deal and the States* helped to stimulate when it was published a decade ago. Here we have explorations into new terrain, at least geographically. Methodologically, these essays are rigorous, traditional political history. The authors in this volume are younger: Harold Gorvine on Massachusetts, Richard Keller on Pennsylvania, David Maurer on Ohio, Robert Hunter on Virginia, John Moore on Louisiana, Keith Bryant on Oklahoma, F. Alan Coombs on Wyoming, Michael Malone on Montana, James Wickens on Colorado, William Pickens on New Mexico, Robert Burton on Oregon, Bruce Stave on Pittsburgh, and Lyle Dorsett on Kansas City.

Allowing for the complexity of the story, the editors are right to conclude that "the result of this new interest [in state and local history] has been further to downgrade the significance of the New Deal as an instrument of fundamental changes in American life and society" (vol. 2, p. ix). The New Deal's local impact was of course substantial in certain respects, bringing some political realignment, broadening the policy agendas of lower-tier governments, and integrating their welfare and economic policies somewhat into a national design. As a whole, however, these essays indicate that change did not run deep or carry very far. Democratic strength ebbed more quickly at local levels than it did in Washington, and the season of reform was shorter. Local Democratic factions and leaders—one hesitates to call them parties—usually did not offer anything like the programmatic coherence or idealism of the Democrats in Washington, and in any event their resources were even thinner.

Since historians do not often ask exactly the same questions, it is no surprise that these volumes do not produce any decisive realignment of accepted scholarly opinion. Most of the authors tried to indicate the impact of their own research upon the running argument between those who think that the New Deal should have made more changes in American life and those who are im-

pressed by the barriers it faced. No scholar in these volumes places himself unfailingly at either extreme, but on the whole they tend to underscore the deep conservatism of American social structure and values and thereby reinforce what might be called the Leuchtenburg, as opposed to the Bernstein-Zinn, view of matters. Roosevelt appears as a restraining influence in a relatively open situation in the essay by Brody, and to some extent that of Wolters. If one wants to ponder other and more deeply rooted sources of resistance to social reform in the 1930s, there is imposing evidence in the essays—for example, on Massachusetts, Virginia, Oklahoma, and Colorado, or in Polenberg's review of the surge of conservatism and nativism that ended the New Deal in the late 1930s—and at many other places in this two-volume work. If these essays were to be dedicated to one of Roosevelt's most influential contemporaries, it should not be Aubrey Williams but Martin Dies.

OTIS L. GRAHAM, JR.
University of California,
Santa Barbara

DONALD HOLLEY. *Uncle Sam's Farmers: The New Deal Communities in the Lower Mississippi Valley*. Urbana: University of Illinois Press. 1975. Pp. xv, 312. \$14.50.

Contemporary critics considered the New Deal's Resettlement Administration (RA) and its successor, the Farm Security Administration (FSA), as radical. Others dismissed them as Communist-inspired, collectivistic communes. Conversely, some latter-day historians have contended that the New Deal served the interests of the middle class, was too conservative, and ignored lower-strata Americans. In his admirable study of New Deal farm communities in the Lower Mississippi Valley (Region 6, comprising Arkansas, Louisiana, and Mississippi), Donald Holley presents a convincing thesis. Neither the RA nor the FSA, he maintains, was particularly radical, but their efforts to cope with agrarian distress marked a new departure. His final estimate is that the program was rather successful, given its inherent drawbacks. This moderate evaluation is not a mark of vacillation, but rather a careful judgment based on thorough research in primary materials.

National figures, such as Henry A. Wallace and Rexford G. Tugwell, are not ignored, but primary attention is properly directed to such regional administrators as William Reynolds Dyess, Marvin T. Aldrich, and E. B. Whitaker. Most of them were products of Southern land-grant colleges, especially Mississippi State.

The bitter struggles of the Southern Tenant Farmers' Union, led by H. L. Mitchell and Henry

Clay East, are skillfully related. Such conflicts helped secure passage of the Bankhead-Jones Tenant Farmers Act of 1937. The RA was later abolished and replaced by the FSA under Will Alexander.

The programs were intended to deal with rural poverty and land reform. The federal government bought submarginal land, converted it to other uses, and resettled the families on better land. To achieve the goal of owner-operated, family-sized farms, low interest loans were made available on a long repayment schedule.

Holley points out that the program discriminated against blacks and that too few black administrators were hired. He concedes that only a beginning was made in dealing with the great mass of poverty-stricken farmers, black and white. Yet he recognizes the importance of the beginning and demonstrates its benefits.

Holley's style is clear and unadorned. He sifts through a mass of documentary evidence and gives it meaning. What he has to say is illuminating and important.

WILLIAM W. ROGERS
Florida State University

MICHAEL S. HOLMES. *The New Deal in Georgia: An Administrative History*. (Contributions in American History, number 36.) Westport, Conn.: Greenwood Press. 1975. Pp. xi, 364. \$14.95.

In this study Michael S. Holmes assesses the impact of conditions in Georgia on the recovery efforts of eight New Deal agencies: the Federal Emergency Relief Administration, the Civil Works Administration, the Works Progress Administration, the Public Works Administration, the National Recovery Administration, the Agricultural Adjustment Administration, the Resettlement Administration, and the Farm Security Administration. Relying principally on project files in the National Archives, Holmes methodically describes the organizational structures and goals of these agencies and analyzes the manner in which they spent their funds. He concludes that New Deal programs were most effective when their state administrators received sufficient latitude to deal with local problems and conditions.

The New Deal in Georgia provides some interesting anecdotes but little else that is not available in James T. Patterson's *The New Deal and the States*. As a whole, the narrative is excessively detailed, but at certain crucial points it fails to supply enough information. For example, although Holmes feels that Georgia Governor Eugene Talmadge's opposition hindered the recovery programs, he provides no evidence that conditions improved when Eurith D. Rivers, a vocal New Deal supporter, succeeded

Talmadge. Because this is a narrowly focused administrative history, the broad conclusions drawn in the final chapter are both incongruous and unwarranted. Holmes credits the New Deal with expanding the horizons of Georgia's "common men" and making them realize that as voters they could "demand that their leaders build schools, roads, or anything else they wanted" (p. 320). There is little evidence, either in this book or in the political behavior of Georgians in the post-New Deal era, to support such a judgment.

JAMES C. COBB
University of Maryland,
College Park

EVERETT CARLL LADD, JR., with CHARLES D. HADLEY. *Transformations of the American Party System: Political Coalitions from the New Deal to the 1970s*. New York: W. W. Norton. 1975. Pp. xxv, 371. Cloth \$12.50, paper \$4.95.

This is probably the most important book available for understanding the American party system, at least as that system exists in the electorate and in the grand coalitions that constitute each major political party. The principal author is a political scientist at the University of Connecticut who has written extensively on American political history. The associate author, a Connecticut Ph.D., was in charge of data analysis; the book is solidly based on all of the major sources of survey data since the beginnings of the Gallup Poll in 1936.

The book begins with a concise review of five recent alternative theories on where the party system is going: a new Republican majority, a new Democratic majority, a search for the center, an era of partisan decomposition, and a renewed New Deal. The authors find some merit in the arguments underlying most of these alternatives and see their own object as a single integrative statement. They have not found a party realignment and do not think that one is occurring. But they are confident that we have had a transformation of the party system in the years since 1960.

Their summary statements produce the following picture: "There is no majority party in the presidential arena." "There has been an inversion of the old New Deal relationship of social class to the vote. . . . This inversion follows from very basic changes in the structure of conflict in American society, and is likely to be long-term." "The Republican coalition . . . is weaker now than at any time since . . . the Great Depression." "A 'two-tier' party system has emerged, with one set of electoral dynamics operating at the presidential level, and yet another in sub-presidential contests." "A major new cohort of activists has assumed vastly in-

creased importance in the electoral arena" (pp. 26-27).

Part 1 of the book deals with the formation of the New Deal party system and its major elements, what happened to it up to 1960, and the disintegration of the New Deal system in the South beginning in 1948. Part 2 is on the party system since 1960, and it begins with an exceptionally well-done chapter on the nature of postindustrial society. The shifting party coalitions are then traced in detail, followed by thorough analysis of how the opposite and equally "unnatural" land-slides of 1964 and 1972 could have happened. The book concludes with an anticipation that we can expect more of the same.

PAUL T. DAVID
University of Virginia

ROBERT SOBEL. *N.Y.S.E.: A History of the New York Stock Exchange, 1935-1975*. New York: Weybright and Talley. 1975. Pp. xi, 398. \$15.00.

The author notes the relative lack of competent critics for a work of this sort; insiders are not given to meaningful comment, and outsiders, who are accustomed to sensing faults in historical treatments, are not sufficiently familiar with the subject matter. I fall into the second group, but immediately protest that Robert Sobel teaches me much about the operations of the Exchange. The book was written "with the lay reader in mind." This includes more than refraining from technical jargon, for the author does not spare space in spelling out functions, practices, and situations otherwise puzzling. The language of the text is lively throughout, and the tables are invitingly clear. This is not the first, though it is the most sustained, of Sobel's explorations in this field of business history.

The New York Stock Exchange imperfectly registers the country's metabolism. Much of this volume is of necessity given to a running account of Big Board day-to-day and month-to-month performance. But many pages are devoted to economic, political, military, technological, and other developments in America and the world that conditioned actions on the Exchange. Sometimes the author looks at events through the eyes of buyers and sellers of securities, but just as often he describes the passing scene as an objective historian. An attractive example of the latter is his characterization of the Eisenhower presidency, with its combination of confidence and conservatism that was wise enough to continue certain proven New Deal policies. Even here the end was not as happy as the middle period, reminding us that we are always to expect incessant fluctuation.

The behavior of the Exchange was frequently

due more to its own internal contentions or to the operating style of its chief executive, than to governmental investigation or policing. The Securities and Exchange Commission loomed larger in the New Deal organization chart than in the discipline of "the street," even when reputed dangerous activists were appointed to its membership.

The history of the Exchange spans generations. Succeeding ones have short memories. Newer participants, Sobel says, regarded the crash of 1929 as a remote calamity like the Civil War, and in one way or another they compelled experienced seniors to cater to their venturesome demands. This prevented decay, although at a cost.

It is evident throughout these pages that the author did prolonged digging in the archives of the Exchange, at Broad and Wall streets and especially at the warehouse on Hudson Street. The mass of the materials at the latter would repel a less diligent investigator. He has not stopped with written and printed records of many kinds but has sought the opinions and recollections of knowledgeable persons in the financial district. It will be a while before another scholar attempts such an inquiry.

BROADUS MITCHELL
Rutgers University

CABELL PHILLIPS. *The 1940s: Decade of Triumph and Trouble*. (The New York Times Chronicle of American Life.) New York: Macmillan Publishing Company. 1975. Pp. xi, 414. \$12.95.

Although written by a former New York Times Washington correspondent and a reporter knowledgeable about the national political scene, this is nonetheless a disappointing book. It is not surprising that Phillips has not researched primary sources; a more crucial deficiency, however, is his failure to have done extensive reading in the recently published secondary literature. His research consists principally of New York Times files and news stories, selected interviews, and a smattering of books (principally memoirs) published during the 1950s and early 1960s. The result is a superficial and impressionistic analysis of American society and politics during the 1940s, which contains numerous minor errors of fact. His limited research further contributes to the author's often simplistic rendering of complex issues. Moreover, developments that were not recognized in the contemporary press accounts as notable but which proved to be important legacies—for instance, the institutionalization and expansion of the presidency, the creation of the CIA, and the expansion of FBI surveillance authority—are barely discussed. These omissions detract from Phillips' book, for he contends that the 1940s constituted a

transition decade, one marking a division between an old and a new order, particularly as this relates to the nation's role in international affairs, changes in values and priorities, and the emerging conception of the role and responsibility of the federal government. Lastly, the author's frequent resort to sweeping, unsupported generalizations about dominant values and public opinion reduce the value of this book for the serious scholar. This is the kind of book that will send the "new social" historians into apoplexy.

Where this book can be commended is in Phillips' masterful account of the important trends and developments of the decade. In one sense, this is a readable and condensed New York Times almanac, reporting statistics in a readable and insightful form; detailing changes in productivity, consumption, residential patterns, and life styles; and discussing the important international developments, political campaigns, legislative debates, and congressional hearings. The book is extremely well written and easy to read, and Phillips' recounting of interesting anecdotes provides insights into the mood of the period.

ATHAN THEOHARIS
Marquette University

W. AVERELL HARRIMAN and ELIE ABEL. *Special Envoy to Churchill and Stalin, 1941-1946*. New York: Random House. 1975. Pp. xii, 595. \$15.00.

As Roosevelt's lend-lease representative in London from 1941 to 1943 and as ambassador to the Soviet Union from 1943 to 1946, W. Averell Harriman wielded extraordinary influence. A confidant of Roosevelt and Churchill, with unusual access to Stalin, he became an intermediary among the Big Three, a man entrusted to represent the views of one to the other. In the early days of the Truman administration he played a vital role in the formulation of policy toward the Soviet Union.

Now in his mid-eighties, Harriman has at last offered a full account of his service in World War II. The fruit of close collaboration with the journalist Elie Abel, *Special Envoy* is neither memoir nor biography. Rather, drawing heavily on Harriman's personal papers and recollections, Abel has written a history of Big Three diplomacy from the ambassador's perspective and offered his judgments on men and events.

The book makes interesting reading, but it will disappoint those who have kept up with the recent literature on the period. There are some fascinating glimpses at major personalities, and there is information about day-to-day contacts with the British and Russians that does not appear in the documents. But there are no "bombshells" here: much of the ground covered is familiar; many of

the documents included are well known. Moreover, Harriman's comments on his contemporaries are remarkably discreet, even bland. He comes down hard only on Eisenhower, whose naiveté he found "astounding," and on James Byrnes, whose secretiveness he deeply resented. He was very close to Roosevelt and Churchill, but he offers little fresh insight into their personalities and attitudes. He comments at length only on Stalin. The Grotanian moralist in Harriman was obviously appalled by Stalin the "murderous tyrant," but the man of affairs was deeply impressed by the Soviet leader's "high intelligence" and "fantastic" grasp of detail. "I found him better informed than Roosevelt, more realistic than Churchill, in some ways the most effective of the war leaders."

Harriman's observations on the origins of the cold war raise some serious problems. He vigorously defends Roosevelt's and Churchill's attempts to promote Eastern European governments that would be "friendly to, but not the creatures of, Moscow." He concludes that the "fact that we tried and failed left the main responsibility for the Cold War with Stalin, where it belongs." It may be questioned, however, whether such governments were within the realm of possibility and how Harriman's "firm but friendly quid pro quo approach" could have produced them. The ambassador chides Truman for his tongue-lashing of Molotov and for the abrupt cessation of lend-lease in May 1945. Yet it was Harriman who stirred Truman's wrath with talk of a "barbarian invasion of Europe" and who masterminded the change in lend-lease policy. *Special Envoy* obscures Harriman's role in Truman's adoption of a "tough" policy, and the Harriman of this account appears more moderate than the Harriman of 1945, at least as he is revealed in the documents.

In short, the book is nicely crafted, but because it comes so late it contains few surprises. One hopes that Harriman and Abel will not delay in carrying the account through the Truman, Kennedy, and Johnson years.

GEORGE C. HERRING
University of Kentucky

CHARLES P. ROLAND. *The Improbable Era: The South since World War II*. Lexington: University Press of Kentucky. 1975. Pp. 228. \$11.95.

The issue of Southern exceptionalism, like that of American exceptionalism, goes back to the seventeenth century, and *The Improbable Era* is yet another contribution to the literature dealing with that old Puritan theme. This study is a macroappraisal of changes in the South since World War II. It is comprehensive, extending from economics and politics through education and religion to

music and the visual arts, and it is both extensively researched and written in a straightforward style.

If the early chapters of the book are weak, the last two are gems of scholarly precision. In chapters 10 and 11 Roland concentrates on the theme of change amid persisting tradition, a theme long associated with C. Vann Woodward, whom the author frequently cites. He is also concerned, inferentially, with the Veblen-Ogburn "cultural lag" phenomenon. Among other things, Roland suggests that Southern history is forgetful but ultimately unforgiving, for, as he amply demonstrates, the mythic and defensive South is still very much alive.

The strength and variety of the author's arguments make impossible a capsule summary of the book; the very clarity and ease of style help conceal the density of thought and concentrated labor that went into it. As for the shape of the book, that was apparently dictated by the easy availability of censuses and other related data.

Social scientists are likely to feel that the book contains too much narrative description and too little interpretive analysis. The bibliographical note is excellent, but historians will find the documentation for the various chapters woefully inadequate. Still, if it is true that Roland's reach exceeds his grasp, one cannot fault him on his acute sensitivity to historical nuance and complexity or on his keen sense of historical irony.

Marx once wrote that "the tradition of all past generations weighs like a nightmare upon the brain of the living." In this work Roland has reconfirmed that proposition as it relates to the contemporary South.

FRANCIS M. WILHOIT
Drake University

THOMAS M. CAMPBELL and GEORGE C. HERRING, editors. *The Diaries of Edward R. Stettinius, Jr., 1943-1946*. New York: New Viewpoints. 1975. Pp. xxviii, 544. \$12.50.

Like many officials departing from government service, Edward Stettinius, former undersecretary and secretary of state, as well as ambassador to the United Nations, carted off his papers—daily summaries of events, transcripts of phone conversations, memoranda of discussions, correspondence with ambassadors and Washington officials, and reports to the president. Drawing upon this substantial collection at the University of Virginia, Thomas M. Campbell and George C. Herring, who as graduate students organized the materials and used them in dissertations, have edited this useful volume. At many points it supplements the *Foreign Relations* series for 1943-46, and sometimes

even the State Department records at the National Archives.

The Diaries, like the larger collection, reveals a cautious, insecure man, reluctant to offer judgments and unable to push for policies beyond his beloved United Nations. Despite the editors' hopes of raising scholars' estimates of Stettinius, this volume confirms the consensus: he was a man of limited ability who operated far from the center of power on most important issues; policy swirled past him, and he watched it develop and change.

Analysts of the cold war will find considerable detailed information on the creation and operation of the United Nations, especially on the San Francisco Conference in 1945 and the Iranian dispute in 1946. But they will be disappointed that *The Diaries*, like the papers themselves, contains little rich information about the formulation of American policy toward the Soviet Union, especially on the key problems of Eastern Europe, Germany, and the Far East. Among the more interesting, previously unpublished documents are those that reveal that on the eve of Yalta Roosevelt told senators that the Russians "had the power" in Eastern Europe, that a break in relations was undesirable, and that America's economic position (primarily lend-lease) was not "a bargaining weapon of any strength"; he informed Stettinius a week later that he preferred dealing in person with Stalin on the Russian loan; W. Averell Harriman, ambassador to the Soviet Union, reported shortly after Yalta that the conference proved that Soviet policy was directed at maintaining "intimacy between the three Great Powers"; Secretary of State James Byrnes admitted in September 1945 that he had thought of telling Molotov at the London Conference that the United States would use the atomic bomb only to halt aggression and maintain peace; and Truman concluded by October that the United States "had perhaps four to ten years" to work out international control of atomic energy.

BARTON J. BERNSTEIN
Stanford University

GEIR LUNDESTAD. *The American Non-Policy towards Eastern Europe, 1943-1947: Universalism in an Area Not of Essential Interest to the United States*. Tromsø: Universitetsforlaget; distrib. by Humanities Press, New York. 1975. Pp. 653. \$22.50.

"The main point of this study," concludes Geir Lundestad, a young Norwegian historian, "is that Washington was never able to develop any consistent policy towards Eastern Europe" (p. 429). In demonstrating that proposition, the author explores the basic elements and motivating factors of the United States' wartime policy; gives detailed accounts of American interests and actions regard-

ing Hungary, Czechoslovakia, Poland, Romania, and Bulgaria, as well as brief notes for comparative purposes on policy toward Finland and the Baltic states; considers the eventual peace treaties with the Axis satellites; and assesses the military and economic leverage available to the United States in support of its policies. A notable inclusion is the useful survey of American attitudes regarding the various schemes for Eastern European federation.

The picture that emerges is clear if unsurprising. Washington's approach was dominated by "universalism," which subsumed the pursuit of a world organization, democratic governments, and economic multilateralism. These principles, publicly enunciated in the Atlantic Charter and in the Yalta Declaration on Liberated Europe, were eventually nullified by the immutable determination of Stalin to secure a subservient sphere of influence on the Soviet Union's western approaches. The marginal strategic and economic importance of Eastern Europe for the United States, the irreconcilability of the twin Rooseveltian goals of an Eastern Europe democratically governed as well as genuinely friendly to the Soviet Union, the American inclination to neatly distinguish between military strategy and political objectives, and Anglo-American and United States unilateralism in dealing with defeated Italy and Japan, all conspired against application of the universalist model to Eastern Europe. What was left was a declaratory policy whose diplomatic pursuit in 1945-46 proved a bitter exercise in futility and marked the irrevocable disintegration of the Grand Alliance.

The author claims that in stressing the "constant contest between universalism and its modifiers" he surpasses existing "traditionalist" and "revisionist" accounts that allegedly fail to discern the complexity of United States policies toward Eastern Europe. He finds little evidence to support the revisionists' economic determinism, notes that Washington was generally most sympathetic to the political center in Eastern Europe (where the indigenous communists were in a distinct minority), and concludes that "even if universalism with its democratic-multilateralist elements may be seen as an expansionist ideology, this did not necessarily mean that it conflicted with the wishes of the Eastern European peoples. Usually it did not" (p. 420). Washington's policy of "idealistic self-interest" proved impotent in the face of the "one crucial factor"—the presence of the Red Army in a region that in the American world view was of secondary priority.

Lundestad's style is clear and his research impressive. He has explored all the relevant American documentary collections. The unnecessarily

fragmented structure of the work suggests a dissertation in need of streamlining, but the study provides a thorough account of a critical and often contentious sphere and phase of United States foreign policy. The promise of novelty through a synthesis of traditionalist and revisionist approaches is belied by the author's conclusions. As with John Lewis Gaddis' outstanding analysis of the origins of the cold war, the outcome of dispassionate scholarship is only a marginally amended elaboration of orthodox historiography.

BENNETT KOVRIG
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JOSEPH P. LASH, biographical essay and notes by. *From the Diaries of Felix Frankfurter*. Assisted by JONATHAN LASH. New York: W. W. Norton and Company. 1975. Pp. xiii, 366. \$12.00.

Joseph Lash's fine introductory biographical sketch of Felix Frankfurter (the "Brahmin of the Law"), which is ninety pages long and well annotated, endows this book with lasting value. It is written with the meticulous care of Frankfurter's own work and is a thought-provoking estimate of the judge's character and contributions. There is a curious dualism between Frankfurter's two prime roles as the behind-the-scenes political adviser and as the Supreme Court justice holding forth for judicial self-restraint in the realm of personal liberties as well as economic matters. Lash sees Frankfurter as the brilliant immigrant youth who became a profound American patriot, worshipping a panoply of heroes: Holmes foremost, but also Brandeis and Stimson, and beginning in the 1930s, Franklin D. Roosevelt. Frankfurter gained a reputation for radicalism through his early defense of the liberties of such social rebels as Sacco and Vanzetti, and he reluctantly abetted Roosevelt in his struggle against the Supreme Court. On the Court, Frankfurter was being consistent in insisting that the judiciary should not interfere in economic matters. He proved himself more a wartime patriot than a radical in matters of civil liberties. Lash speculates fruitfully on the reasons for these attitudes; the diaries illustrate some of his points.

Beginning with his arrival in Washington in 1911 as a young assistant to Stimson through his years on the Supreme Court, Frankfurter kept, at least intermittently, a diary full of detail and feelings. Parts he withheld, others he destroyed, and some pages were stolen from the Library of Congress. The sections Lash has located and reproduced are an interesting sampler, valuable for their glimpses of Frankfurter and his renowned contemporaries. There are only a score of pages on the Taft administration, and even fewer on two squabbles with President Lowell of Harvard in 1929 and 1933. At

the heart of the book are well over a hundred pages on 1943, the year in which Frankfurter records his emphatic differences with Justices Douglas and Black and his considerable involvement in the issues and intrigues of wartime Washington. A span of less than a hundred pages covers the period from the autumn of 1946 to the spring of 1948, with emphasis more on the Court than the Truman administration. These sections on the 1940s are primarily of value to the legal historian interested in tracing the discussions leading to several significant decisions and in the interplay among the justices. The vehemence of some of Frankfurter's comments may well indicate, as Lash suggests, that he used the diary as a safety valve.

As a mirror of Washington, the diary is useful but by no means extraordinary. In 1943 Frankfurter was seldom involved directly with President Roosevelt, and like Justice Brandeis earlier, he was most significant as a sage removed from the center of action, feeding suggestions to his devoted and influential coterie. In the Truman years his chief access to inside knowledge was Dean Acheson, with whom he regularly walked to work. The diary is primarily useful, therefore, as a supplement to Frankfurter's legal writings, his reminiscences, and Max Freedman's edition of his enormous, adulatory correspondence with Roosevelt.

FRANK FREIDEL
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ALEXANDER L. GEORGE and RICHARD SMOKE. *Deterrence in American Foreign Policy: Theory and Practice*. New York: Columbia University Press. 1974. Pp. xii, 666. Cloth \$17.50, paper \$6.95.

This book provides a comprehensive assessment of both the theory and practice of deterrence in American foreign policy from 1948 to 1963 through the perspective of eleven limited-war case studies. Some of the more important cases examined include the Berlin Blockade, the outbreak of the Korean War, the intervention of the Chinese in that war, the Hungarian revolution, and the Cuban missile crisis. The authors conclude that deterrence theory has been seriously deficient as a guide to effective foreign policy in that period because it was insufficiently rooted in real historical data, oversimplified, deductive rather than inductive in method, and more prescriptive than analytical. Arguing that deterrence theory is not a self-contained strategy, the authors call for greater reliance on historical studies by decision makers, more willingness to use a multifaceted process of give and take, and especially the application of pressure for a more positive and long-term world view of where America should be going.

In the first part of the book the authors discuss the nature of contemporary deterrence theory,

suggesting that it is well developed at the strategic level of all-out nuclear war but comparatively underdeveloped at lower levels of policy. At no level have the advocates of this theory given much attention to historical cases of deterrence. Methodologically, deterrence theory remains untested and therefore ought to be used sparingly, if at all, and decreasingly as multipolarism increases in the world.

The second part of the book applies deterrence analysis together with historical explanations to evaluate the deterrence process and explain what happened in the eleven instances. The authors conclude that the Berlin Blockade is not a good example of successful deterrence but a failure from the standpoint of the Western powers because the Soviets achieved policy modifications in Western Europe and West Germany they would not have achieved otherwise. On the Chinese intervention in the Korean War, the writers conclude that neither side wanted an escalation and that both the Chinese and the Americans miscalculated the motives and the intensity of the other power's commitments because of wishful thinking, shifting goals, and Washington's desire to eliminate the North Korean regime. The Cuban missile crisis was caused by the Soviets' misperception of John Kennedy's character, their tendency to discount serious warnings as politically motivated, and American lack of receptivity to early clues of a missile deployment owing to a high priority the White House gave to relaxations of tensions with the Soviet Union. Kennedy did correctly perceive Khrushchev's willingness to act rationally when under extreme pressure, however, and the result of the 1962 missile crisis was a major turning point in the cold war toward greater détente. The other cases are examined in much the same fashion.

The final part of the book attempts a reformulation of deterrence theory toward a broader, multifaceted process that would include greater emphasis on classical diplomacy, more discriminating use of a variety of other means of influencing adversary behavior, and the acceptance of a policy based less on threats and more on the recognition of a limited role for appeasement and negotiations.

George and Smoke have written a thoughtful, instructive, and interdisciplinary account that should be useful to policy makers for some years to come. Their call for greater use of history in foreign policy making should be widely applauded by historians generally, but their conclusions will probably not fully satisfy revisionists. There are few new data here, but the analysis is exceptionally well done and should be useful to anyone working on matters where deterrence theory is of some importance.

JAMES L. CLAYTON
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LAWRENCE S. WITTNER. *Cold War America: From Hiroshima to Watergate*. New York: Praeger Publishers. 1974. Pp. x, 403. Cloth \$12.50, paper \$6.95.

In *Cold War America* Lawrence Wittner writes an appealing, but often aggravating, omnibus of America's political history from the end of the Second World War to the fall of Nixon. Aiming at the nonspecialist and being self-consciously critical of "consensus" history, Wittner shows the strengths and weaknesses of the survey approach. To his credit, the author succeeds in discussing a great number of complex issues in an entertaining manner. This stylistic asset becomes a liability, however, where Wittner avoids or oversimplifies a complex historical problem for the apparent sake of the narrative. Unfortunately, this tendency occurs at numerous times in the text. Wittner distorts the "non-consensus" view of American foreign policy by accepting it so uncritically. Few serious revisionist historians would agree with Wittner's portrayal of cold-war origins; in particular, few would accept his premise that the accession of Truman in 1945 marked an abrupt shift from the policies of the Roosevelt administration.

The section on the beginning of the cold war is especially disappointing for its disregard of the many new sources on that subject. This neglect leads the author to rely heavily upon Truman's own notoriously self-serving memoirs—ironically so since the former president is himself clearly one of the villains of Wittner's book. There are other significant omissions or distortions: American policy toward the Middle East receives surprisingly little attention, whereas America's interest in Vietnam is dubiously portrayed as being based upon economic motives. The condition of blacks in America gets superficial treatment, and feminism receives barely a paragraph. In his defense, however, Wittner's predominant concern is with foreign and domestic policies, and his forays into social and cultural history (at one point he suggests a connection between "McHale's Navy" and escalation in the Vietnam War) appear more in the way of interesting sidelights.

It is perhaps because of Wittner's skill as a reductionist that the best parts of his book deal with the recent domestic events. The domestic consequences of foreign policy are too often ignored in survey histories, though not here. Wittner's brief section on McCarthyism and the McCarran Act is a useful corrective to the myths that continue concerning the anticommunist hysteria of the 1950s. His treatment of the 1968 and 1972 elections, based largely upon journalistic accounts, is also reflective and convincing. Finally, the author's bibliographic essay is a compendium of major secondary literature.

Wittner's book hardly breaks new ground with

regard to either sources or interpretations of the cold war; nor, in fairness, was that its intent. It is a worthwhile synthesis of the modern period with a new and welcome emphasis upon the domestic scene.

GREGG F. HERKEN
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DAVID EDWIN HARRELL, JR. *All Things Are Possible: The Healing & Charismatic Revivals in Modern America*. Bloomington: Indiana University Press. 1976. Pp. xi, 304. \$10.95.

In the decade following World War II all kinds of religion received unusually warm and often unexpected acceptance. One group of revivalists who benefited from this outpouring of enthusiasm consisted of more than twenty men and women from pentecostal backgrounds who preached a gospel stressing bodily healing. In the tradition of revivalism they developed idiosyncratic styles, often controlled their finances badly, were charged with self-serving and corruption, but retorted with fierce individualism and competitiveness. They broke free from their denominations, founded magazines, and attempted to create radio and television networks. In the style of the 'fifties, they often traveled abroad, bringing healing to Argentina or Java. People who attended their campaigns threw away their crutches, saw for the first time, or felt cured of cancer. But, when physicians examined these people, the doctors failed to support the revivalists' fantastic claims. After a period of decline in the 1960s, these charismatic revivalists entered another era of prosperity and success in which their message was not so much healing but the gift of tongues and an emphasis on prosperity as a gift of God. These revivalists are virtually unknown outside the circle of their admirers, but as usually has happened in revivals of this sort, one managed to stamp his name and personality on the movement as a whole and to exemplify what the movement stood for. In this case the man was Oral Roberts.

DAVID EDWIN HARRELL, JR. *All Things Are Possible*: collection of fugitive sources to bring together this account of a movement that has not previously received scholarly treatment. As a biographical dictionary or a reference work, it will serve historians well, but Harrell has failed to set the movement into a context in which these revivalists can be understood by the student of either social or religious history. Furthermore, he has been so timid in judging the careers of the revivalists that the reader cannot ascertain what the author means to say. He writes, for instance, that "few popular writers understood the vast differences between the ministries of Jimmy Swaggart and David Ter-

rell"; but I felt no more enlightened about that difference than the maligned "popular writers" after having read Harrell's descriptions. One certainly does not want a simplistic score card of the good and bad guys, but if a historian fails to establish a point of view, he produces, as I fear Harrell has, only a catalog.

ERNEST R. SANDEEN
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HAROLD JOYCE NOBLE. *Embassy at War*. Edited with an introduction by FRANK BALDWIN. (The Studies of the East Asian Institute.) Seattle: University of Washington Press. 1975. Pp. xxiv, 328. \$12.50.

Harold Joyce Noble was first secretary of the United States embassy in South Korea from August 1949 to January 1951. *Embassy at War* is his personal account of the first three difficult months of the Korean War. Noble vividly portrays the frustrations of an embassy on the move and its relations with the Korean government and army, the United States military, and, to a much lesser extent, the State Department. There are few startling revelations, but by providing for the first time a perspective from the embassy, Noble makes several interesting observations. Contrary to the impression given in Douglas MacArthur's *Reminiscences*, the South Korean army was not a disorganized rabble following the North Korean capture of Seoul. For the most part, the ROK army fought bravely and had a number of able generals. With that sense of omniscience that has characterized American military leaders since the Second World War, the American generals thought they knew exactly how to fight a war in Korea and adopted an arrogant attitude toward their Korean counterparts. It was the embassy staff, especially Ambassador John Muccio and Noble himself, who convinced the American command to adopt the successful program of integrating Korean soldiers into the army's undermanned units and using Korean police battalions to detect and fight infiltrators. Besides serving as liaison with the ROK government and army, the embassy profoundly affected the course of the war by maintaining the morale of Koreans and Americans shocked by the invasion and initial defeats—its chief role in these crucial summer months—and by preventing President Rhee and the Korean politicians from interfering with the prosecution of the war. Noble, who knew Rhee since 1938, was assigned the delicate task of persuading the president to leave the front. Noble stresses that South Korea was unprepared for war and did not provoke it; Rhee always remained dubious of American support.

Noble was raised in Korea by his Presbyterian

missionary parents. He taught Far Eastern history at the University of Oregon and at colleges in Korea and Japan before turning to journalism and government service. Throughout these years, Noble was devoted to the cause of Korean independence, unity, and freedom, to MacArthur's occupation policies in Japan, and to hard-line anti-communism and containment of Soviet Russia. *Embassy at War*, however, is generally a well-balanced and lively account based on Noble's notes and correspondence. Noble blasts the State Department for even thinking of dissolving the South Korean government following elections in liberated North Korea, but he fails to appreciate the international complications that confronted America's Korean policies. The chief weakness of this account is Noble's plastic image of Rhee, who is presented as the patriotic leader throwing temper tantrums whenever he was overruled by Washington and his own cabinet.

Frank Baldwin has provided an excellent sketch of Noble and has admirably edited the long manuscript that the diplomat left at the time of his death in 1953. Baldwin's explanatory notes, which contain correspondence from members of the embassy staff, are particularly valuable.

Embassy at War is a partial but suggestive account of the Korean War. It serves as a reminder that a comprehensive diplomatic history of the conflict has yet to be written.

NOEL PUGACH
University of New Mexico

GARY W. REICHARD. *The Reaffirmation of Republicanism: Eisenhower and the Eighty-Third Congress*. (Twentieth-Century America Series.) Knoxville: University of Tennessee Press. 1975. Pp. xv, 303. \$14.95.

In this well-organized and clearly written study Gary W. Reichard attempts "to define . . . Eisenhower's political views, expertise, and overall impact" by analyzing his relationship with the Eighty-third Congress. The choice of the Eighty-third Congress for a case study is logical because it was Eisenhower's initial, and only Republican, Congress. Reichard limits his investigation to four major areas: "foreign policy, fiscal and economic policy, welfare policy, and policy related to power and resource development," which, he correctly believes, reveal the range of Republican ideology. He devotes the first fifth of the book to a political profile of the Republican bloc in Congress, including both incumbents and members of the class of 1952. To determine the degree of support these Republicans gave to Eisenhower's programs, Reichard applies quantitative techniques to 135 roll call votes on "Eisenhower issues." He also relies

heavily upon manuscript collections, especially those at the Eisenhower Library and the Dulles Oral History Project at Princeton.

The book's conclusions offer no major surprises. Eisenhower and the Eighty-third Congress did not usher in a new Republicanism; they returned to and reaffirmed "traditional Republicanism." Reichard demonstrates that the Eisenhower administration proved "unerringly conservative" regarding public power, resource development, and fiscal policies. Its objectives included a balanced budget, lower taxes, diminished responsibilities for the federal government, and encouragement of private activity. Eisenhower's programs toward social security, labor and farm legislation, health, and housing likewise "adhered closely to orthodox Republicanism."

Foreign policy, Reichard admits, is the exception to the pattern of traditional Republicanism that Eisenhower championed. Focusing on the Yalta resolution, the confirmation of Charles E. Bohlen as ambassador to the Soviet Union, and the proposed Bricker amendment, the author concludes that Eisenhower "internationalized" his party. But, he argues, "aside from foreign policy, there was no 'Eisenhower revolution'"; he could have insisted just as easily that aside from domestic policies, Eisenhower reshaped his party's ideology.

Reichard's assessment of Eisenhower as a party leader is solid, although the author admits that "incontrovertible evidence . . . frequently is missing." The president desired to lead his party and did so successfully, using "a combination of conciliation, persuasion, and political maneuver," while keeping in mind he needed Democratic votes in Congress.

Within the guidelines he set, Reichard has produced a careful study that deserves a respected place on the short shelf of Eisenhower historiography.

KEITH W. OLSON
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College Park*

LEWIS J. PAPER. *The Promise and the Performance: The Leadership of John F. Kennedy*. Foreword by JAMES MACGREGOR BURNS. New York: Crown Publishers. 1975. Pp. xi, 408. \$9.95.

This is a curious study, one that is useful and better balanced than some of the products of passionate journalistic partisanship that have already appeared, but one that is nevertheless deeply flawed. Some will see it as a more judicious appraisal than Henry Fairlie's analysis. Like that book, it also concludes that Kennedy's performance did not fulfill expectations. The most impor-

tant difference is that Paper stresses Kennedy's confrontation with the realities of the presidency rather than the cynical manipulation of expectations.

Paper's Kennedy comes remarkably close to the earlier view presented by James MacGregor Burns, at least to the more critical parts of that earlier work. Here, too, Kennedy emerges as a capable, well-intentioned politician, one with a taste for power and with confidence in his own talents. Unlike Burns, however, Paper concentrates on the presidency and his use of power. In the process he confirms many of the criticisms made in the past. More than any president before Nixon, he was concerned with personal image. His obsession with the art of the possible precluded more effective leadership, especially during the civil rights struggles of 1962 and early 1963. Paper comes to the reasonable conclusion that too often "his ideals were compromised by his understanding of the American and international political systems" (p. 346).

For all its value, the book is disturbing. Composed of five massive chapters, it largely ignores essential chronological relationships and concentrates on problems as though they occurred in a vacuum. So Kennedy's support of the communications space satellite program, acknowledged as an attempt to ingratiate himself with business, is viewed apart from the showdown over steel prices, a confrontation that occurred just a few months earlier.

But most jarring are the qualities that will tempt the specialist to question Paper's mastery over his subject. The entire work is laced with naïveté, including the statement that Kennedy was concerned with his television appearances *even after* entering the White House and the observation that "the president should always want to do the right thing" (p. 7). His discussion of Kennedy's 1946 victory ignores the importance of the family background in the Boston area and his appeal to war veterans, let alone the recent publicity over PT-109. It will be unfortunate if such weaknesses obscure the contributions that Paper makes to an analysis of Kennedy's conduct of the presidency.

HERBERT S. PARMET
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RICHARD MAXWELL BROWN. *Strain of Violence: Historical Studies of American Violence and Vigilantism*. New York: Oxford University Press. 1975. Pp. xiii, 397. \$13.95.

LEE KENNETT and JAMES LAVERNE ANDERSON. *The Gun in America: The Origins of a National Dilemma*. (Contributions in American History, number 37.) Westport, Conn.: Greenwood Press. 1975. Pp. x, 339. \$12.95.

Violence in American life is an ordeal, a dilemma, a curse, and most of all a historic fact. Until President Johnson set up the National Commission on the Causes and Prevention of Violence (1968), however, it had been little studied. The Commission's report helped put violence into some historical perspective, and now Richard M. Brown, a member of the Commission who is currently teaching at William and Mary, adds to the literature.

"Our history," Brown declares in his introduction, "has produced and reinforced a strain of violence, which has strongly tinted our national experience." Indeed, "repeated episodes of violence, going far back into our colonial past, have imprinted upon our citizenry a propensity to violence" (p. vii). Brown attempts to prove his point with a detailed examination of violence in the American Revolution, putting special emphasis on South Carolina extremism. He devotes the middle third of the book to vigilantism and the last third to black-white violence and "The Violent Region of Central Texas: Land of Lyndon B. Johnson." The appendix contains lists of colonial riots, revolutionary riots, vigilante movements, slave insurgencies, black-white riots, and lynchings.

Altogether, it is a dreary story. Brown does little in the way of analysis; he is more concerned with recounting violent acts than understanding them. There is a great deal of information in this book, which is characterized by diligent scholarship buttressed by extensive notes. But horrifying as each individual event may be, they lose in the text their ability to shock when murder follows murder on page after page.

There is no concluding chapter, but Brown makes some generalizations within the body of his text. First and foremost, we are all guilty. "The patriot, the humanitarian, the nationalist, the pioneer, the landholder, the farmer, and the laborer (and the capitalist) have used violence as the means to a higher end" (p. 36). But some are more guilty than others. Vigilante violence "has been socially conservative. . . . It has been used to support the cohesive, three-tiered structure of the American community with its upper, middle, and lower classes" (p. 4), and "established groups have been quick to resort to violence in defense of the *status quo* they dominate" (p. 5). Brown contends that "outlaws and the hostile lower people . . . constituted a 'contraculture.' They . . . wished to upset the social structure" (p. 105). The conclusion seems too pat, and in any case it would appear that outlaws on the American frontier were hardly a group out to upset capitalism; more than anyone else they wanted the stagecoaches and the trains to run on time, carrying lots of money.

Brown contends that Lyndon Johnson's Viet-

nam policy "was a throwback to the central Texas ethic of violent self-defense" (p. 238), and he devotes nearly seventy pages to an account of violence in the region. To the objection that Walt Rostow, McGeorge Bundy, and others pushed the Vietnam policy and that they did not come from a region with a violent tradition, Brown replies that they were responding to a totally different tradition, "a tradition of paternalistic American intervention in the world that flourished among the Anglophile elite of the eastern U. S." (p. 288). Brown insists that Johnson personalized the war, a result of his upbringing. But what then of Nixon, who also personalized the war and who outbombed Johnson, despite his California Quaker background?

The major difficulty with *Strain of Violence* is the absence of any comparative work. Brown proves the obvious—that there has been a great deal of violence in American life—but he does not explain it, nor does he compare it to other cultures. It has been said that we are no more violent than any other people; what is different about Americans is that we are more likely to have a gun handy when we get angry and thus more likely to commit murderous violence. Certainly this is the conclusion of Lee Kennett and James L. Anderson in their study, *The Gun in America*.

The Kennett and Anderson book is a model historical study, well written, deeply researched, objective. They begin with an extended discussion of the European experience with privately owned guns, and throughout their examination of gun laws in the United States they continue to refer to contemporary European practices. This is also a refreshing study in that the authors are not advocates; they do a first-rate job of presenting the arguments of both the National Rifle Association, which they find less influential than the popular press believes, and the antigun groups.

Are Americans more violent than other people? Kennett and Anderson take pains to point out that sheer numbers of guns at hand does not always lead to lethal violence: "Israel, which has survived by becoming an armed camp, is a case in point. Switzerland, whose military constitution requires that able-bodied men keep weapons in their homes, is another" (p. 249). The entire subject of violence in America, in short, needs more study. These books will help push that study along.

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CANADA

IVAN AVAKUMOVIC. *The Communist Party in Canada: A History*. Toronto: McClelland and Stewart Limited. 1975. Pp. x, 309. \$5.95.

Ivan Avakumovic recounts in detail the history of the Communist party of Canada (CPC) from its origins after World War I up to the early 1970s. He describes the twists and turns of party policy through the Popular Front period of the 1930s, the Second World War and after, the crisis of de-Stalinization in the 1950s, the relationship between the CPC and various ethnic groups, particularly the Finnish, Ukrainian, and Jewish immigrants in the 1920s, and the infiltration of youth groups, tenants' associations, cultural societies, and other front organizations. Special attention is devoted to the attempts of the CPC to gain control of the trade unions, an effort that was temporarily successful.

Throughout most of its history the party was under the leadership of Tim Buck who retired in 1962. Under his guidance the CPC adhered rigidly to the Stalinist line and vigorously opposed Trotskyite and Maoist deviations. In spite of all its efforts, however, the party never won more than one to two percent of the popular vote in national elections. The reasons for the failure of the CPC to make headway in Canada were varied: intermittent harassment by governments; the constant factionalism within the party; the difficulty of implanting Stalinist policies in a society different from that of Eastern Europe; and, above all, the existence of a strongly entrenched democratic party in the CCF-NDP, which attracted most of the left-wing intellectuals and political activists in the trade unions. In Canada, as in other countries, Khrushchev's denunciation of Stalinism in 1956 created a crisis in the party from which it was slow in recovering. The future of the party today is uncertain.

Although this book is a good factual presentation of the party's history, it is repetitious in places. A major criticism is that it is poorly structured and reads more like a chronology of events rather than an analytical study.

HERBERT F. QUINN
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DAVID JAY BERCUSON. *Confrontation at Winnipeg: Labour, Industrial Relations, and the General Strike*. Montreal: McGill-Queen's University Press. 1974. Pp. x, 227. Cloth \$10.00, paper \$5.00.

David Jay Bercuson provides a new perspective on the Winnipeg general strike of 1919 in *Confrontation at Winnipeg*. Earlier scholars, including D. C. Masters in *The Winnipeg General Strike* (Toronto, 1950), viewed the strike's causes as largely political, but Bercuson stresses the importance of the "industrial background" and the "additional strains" placed on labor by World War I.

Bercuson traces the roots of the general strike

back to 1906, when a strike against the contract machine shops failed because of an injunction. Drawing upon a wide variety of published and manuscript sources, he then proceeds skillfully to explain the growing alienation of labor in Winnipeg from government, employers, and even the Trades and Labor Congress of Canada, supposedly controlled by AFL "choreboys" from the east. By 1918 "radicalism was firmly in the saddle." This radicalism contributed to western Canada's support of the One Big Union.

Although interest in the OBU confirmed that many workers viewed society through "polarized glasses," Bercuson convincingly argues that they remained confused both as to their objectives and their resources. Despite their oratory, the strike leaders were neither revolutionary nor foreign directed. Speaking in the "round tones of north England or the thick burr of the Clydeside," they believed in 1919 that a general strike was the best means of achieving a traditional objective, the right to bargain collectively. The lack of revolutionary fervor doomed the general strike, usually seen as a political weapon, to failure. The strikers were unwilling to permit society to collapse and themselves became "the chief strikebreakers."

Since *Confrontation at Winnipeg* is presented as a reinterpretation of the general strike, it is somewhat disappointing that Bercuson did not explicitly discuss the historiography of labor relations in western Canada and the importance or uniqueness of his own approach. Especially surprising is his failure to analyze, or even to recognize, Masters' book on the general strike. The inclusion of this work would have been appropriate if for no other reason than to illustrate the need for another book on the topic.

BRIAN L. BLAKELEY
Texas Tech University

LATIN AMERICA

RICHARD GRAHAM and PETER H. SMITH, editors. *New Approaches to Latin American History*. Austin: University of Texas Press. 1974. Pp. xiv, 275. \$8.75.

This is among the most important and useful books on Latin American history to appear in the past decade. The nine essays are not only good historiographies of a significant range of Latin American topics, but they are also "think pieces" on how historians might proceed in their search to understand these topics. Suggestive approaches to the study of colonial bureaucrats, nation-state formation in Spain and Latin America, nineteenth-century landowners, political party alignments, regionalism, slavery, immigration, psychohistory, and political legitimacy are presented.

The editors write that the book was "designed to reflect and to promote the conceptual and methodological changes that have already started to affect the study of Latin American history." They and their colleagues have admirably fulfilled this purpose. Each one of these essays makes a solid contribution, a rarity in a collective work. All skillfully make use of a specific research interest to focus on broad conceptual and methodological issues, and all utilize to some extent disciplines other than history. Two-thirds of the essays deal in a broad sense with some aspect of politics. The authors are interested in the socioeconomic context of politics, the linkage between socioeconomic groups, and the exercise of political power.

As impressive as this book is in most respects, it nevertheless has several shortcomings. Many of the authors, for example, refer to the central problem of the underdeveloped status of research and writing on Latin America, by which they mean that compared to the United States and Western Europe there are poor data, a lack of basic monographs, and few conceptual works to guide research. The book makes an important contribution to overcoming the conceptual lag, but none of the authors dwell sufficiently on the implications of this underdevelopment and what might be done about it.

A second and related problem is that seven of the nine authors focus their attention on elites. Some of them do so in part because of the underdeveloped state of scholarship in Latin America, but also on philosophical grounds. As Richard Graham sums up, "From the point of view of the historian, then, there is a marked economy of effort in tracing the course of change by studying the behavior of those in the upper levels of the political structure. This is especially so for areas that, like Latin America, have not been thoroughly studied even by old-fashioned political historians." This, it seems to me, is a false economy and a distortion of history. Elites are important, but so are nonelites, and the one group is not intelligible without the other.

One approach that is of critical importance to an understanding of nonelites in Latin America is quantitative history, yet quantitative history receives little attention in this book. Michael Hall laments the lack of manuscript census data, directories, and tax records for São Paulo and suggests that "it seems unlikely that research of the sort associated with Stephân Thernstrom and other 'new urban historians' in the United States can be duplicated for São Paulo." This is only partially true. There is no lack of available aggregate data on Latin America in published censuses, government reports, and records of private groups. And these can be exploited successfully by quantitative methods.

The shortcomings, as well as all of the issues of this fine book, are subjects for debate. It is a must for professors and students of Latin American history.

SAMUEL L. BAILY
Rutgers University

C. R. BOXER. *Women in Iberian Expansion Overseas, 1415-1815: Some Facts, Fancies and Personalities*. New York: Oxford University Press. 1975. Pp. 142. \$12.75.

Four lectures delivered at Bryn Mawr in late 1972 are here published in somewhat expanded form. The work is the first general survey of the history of women in the Spanish and Portuguese empires, and though uneven in its coverage, it provides a large amount of information and documentation. As is well known, an older interpretation made much of the contrast between the predominantly male colonization of Hispanic America and the family life of the English colonies, but the simplistic versions of this contrast have been progressively revised in recent decades. We now recognize that the situation in the Spanish and Portuguese empires was one of far greater complexity and variety than was once supposed. Boxer first offers a series of vignettes from the Portuguese colonies in Africa, including stories on white women, Moorish wives and concubines, mulatto servant girls, captives, and occasionally whole companies of female soldiers. For Spanish America, where recent studies by James Lockhart, David Brading, and others have added so much to our knowledge, we have a great range of situations and types. Boxer emphasizes the strong position of colonial Spanish and Creole ladies in *encomienda* and property inheritance. Seclusion of women occurred in both Brazil and Spanish America (the former more than the latter), but the practice is not to be equated absolutely with subordination, nor did it eliminate women from a functioning social or economic role. More Portuguese women went to Brazil than to either Africa or Goa, and it seems to be in Goa especially that the more celebrated excesses in colonial social relations prevailed. A final chapter, more analytic and less episodic than the others, considers the Iberian version of the idea of inherent female inferiority, particularly in relation to the cult of Mary.

CHARLES GIBSON
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Ann Arbor

MENDEL PETERSON. *The Funnel of Gold*. Boston: Little, Brown and Company. 1975. Pp. xi, 481. \$15.00.

This is a popular history of Spain in the Caribbean from the time of Columbus to the mid-eighteenth

century, and Mendel Peterson tells the story with imagination and enthusiasm. The book describes the many obstacles—pirates, storms, shipwrecks, and virulent disease—that hampered Spanish monopolization of the New World. It is an adventure story filled with accounts of heroism and villainy played out against the background of the struggle between Spain and its enemies, England, Holland, and France, to control the wealth of the New World. In addition to the heroic, the mundane aspects have not been neglected. There are detailed descriptions of the ships that plied the seas between Spain and its American empire, their routes and their cargoes of treasure. Of particular interest are the chapters dealing with recent underwater exploration of sixteenth-century shipwrecks. Many interesting illustrations, including photographs of objects uncovered by the underwater explorations, complete this volume.

For his sources the author depends almost entirely on printed primary accounts (collections of documents, chronicles, contemporary writings) with only scattered references to archival documents, although he indicates that he undertook investigations in the Spanish and British archives. The quotations from the printed materials are unnecessarily long, since they have been drawn from readily available and known sources (Bernal Díaz del Castillo, Vázquez de Espinosa, Thomas Gage, and others). The bibliography contains references to most of the standard older accounts and chronicles, but it includes few recent monographic studies.

The book is more or less dependable as to facts and events but laden with personal generalizations on the Spanish, the English, and the Dutch. There are opinionated comments on historical personages. The old clichés and stereotypes are present; for example, the Spanish soldier is described as being "convinced of his own skill, haughty and full of self-righteousness, greedy and cruel for the love of it" (p. 9). Is this not the villainous Spaniard of the "Black Legend"? Does the author really expect us to believe that "the incurable dilatoriness of the Spaniard also contributed to confuse the schedules of the American fleets" (p. 69)? Individuals fare no better—the royal judge Francisco de Bobadilla was, according to the author, a "jealous small-minded petty tyrant able to disgrace one of the truly great men of history [Columbus]" (p. 7). Here is a world peopled by good guys and bad guys with the stereotyped Spaniards cast as foils for the English and Dutch interlopers.

Certainly there is more to the history of Spain in the Caribbean than its struggle against natural phenomena and foreign enemies. Viceroy, governors, settlers, pirates, and interlopers move across these pages in a colorful procession, but Peterson has not been concerned with the broad and deep

circumstances—social, political, and economic—of the world in which they lived, or for that matter the institutional structure of the Spanish empire. The book is pure and simple narration.

There is no doubt that "sword and swagger" volumes of this kind will continue to sell books for large commercial publishing companies because they seemingly provide entertainment for the general reading public. Moreover, it is quite clear that the author wrote this volume because of his fascination with the period and region, and that is an admirable reason. But it must be recognized that such books contribute little to the advancement of serious historical research.

RUTH PIKE
Hunter College,
City University of New York

PHILIP WAYNE POWELL. *Soldiers, Indians and Silver; North America's First Frontier War*. Reprint. Tempe, Ariz.: Center for Latin American Studies, Arizona State University. 1975. Pp. xv, 317.

MAX L. MOORHEAD. *The Presidio: Bastion of the Spanish Borderlands*. Norman: University of Oklahoma Press. 1975. Pp. xiii, 288. \$9.95.

Arizona State's paperback edition of Philip Powell's *Soldiers, Indians and Silver* is simply a reissue of the book originally published by the University of California Press in 1952. With the exception of a new four-page foreword, the texts are identical. For a review of this work, the reader may consult C. G. Motten's laudatory evaluation in the *American Historical Review* (58 [Jan. 1953]:404-05). It is one with which I fully concur.

What Max Moorhead has undertaken is to take Powell's work as a starting point—it covers the story of the advance of the Spanish frontier northward in Mexico only in the sixteenth century—and carry the account to the end of the eighteenth century. The first 110 of the 271 pages of his work are devoted to this task. An equal number of pages were then allotted to a more detailed examination of the presidio itself in terms of its physical construction, its personnel and administration, and its relationships with other frontier Spanish settlements, towns, ranchos, and Indian reservations.

The role of the presidio as a military fortress in the advancing Spanish frontier in North America was crucial, and it has long called for a thorough investigation. Moorhead's work goes a long way toward satisfying this need, at least so far as the central *meseta* of Mexico is concerned. His account is particularly full and valuable in its treatment of the extreme northern presidial line, stretching from Sonora to Texas, which formed the culminating development of the system. Moorhead points out that the line of presidios along the Rio Grande

and spaced westward across Chihuahua and Sonora is virtually identical with the present Mexican and United States border. It marked the northern edge of effective Mexican settlement, with detached salients in Texas, New Mexico, and California, and was so recognized in the Treaty of Guadalupe Hidalgo. Moorhead is also to be commended for his description and analysis of the problems of graft and maladministration these distant frontier posts presented to the central authorities of New Spain. The efforts made in the *reglamentos* of 1729 and 1772, and by the establishment of the *Provincias Internas* as an administrative frontier agency to cope with these problems, are also well covered.

Moorhead deliberately excluded the two Californias from his survey because he felt the problems they presented were dissimilar. For the same reason, presumably, he did not attempt to cover the Mexican mainland west of the Sierra Madre Occidental: the present states of Sinaloa and southern Sonora. This is unfortunate because the interesting collaborative function of the presidio and mission as paired frontier agencies is best illustrated in those areas. This aspect of the presidio's contribution to the frontier system is only lightly touched upon in this book.

It is also unfortunate, in view of the general excellence of this work, grounded as it is on primary documents, that the author chose to rely on secondary works entirely for his treatment of the Yuma area. The authors of these secondary works had not gone to the sources either, except in a most perfunctory way. As a result, the treatment of the Yuma story the author presents is not only worthless, it is almost entirely erroneous. Readers who wish to get at the truth should consult *legajo* 517 of the Guadalajara *ramo* in the Archive of the Indies.

For the most part, though, Moorhead's work is thoroughly researched. The footnote citations are copious and rewarding, and the bibliography is a full one, though there are some curious omissions, Ocaranza and Priestley, for example. The book is also beautifully bound and well printed. There are excellent maps and illustrations; the most fascinating of the latter are twenty-one contemporary sketches of frontier presidios that Moorhead found in the British Museum.

EDWIN A. BEILHARZ
University of Santa Clara

WARREN G. KNEER. *Great Britain and the Caribbean, 1901-1913: A Study in Anglo-American Relations*. East Lansing: Michigan State University Press. 1975. Pp. xvii, 242. \$12.50.

This Creighton University professor's work has appeared almost simultaneously with Lester D.

Langley's study of Caribbean diplomacy, which perhaps suggests a renewed interest in this particular area. The two books complement each other, rather than compete. Langley's work, written with an American focus, covers the whole period from 1776 to 1904, whereas Warren Kneer's study is confined to the years 1901-13 and has a British perspective.

Kneer shows that the Anglo-German cooperation in the Venezuelan intervention of 1902-03 was prefaced by the cooperation of five European powers in the Guatemalan dispute of 1901, staged largely at Lord Lansdowne's initiative. He emphasizes that the Admiralty, nervous about the growth of the German navy, became increasingly reluctant to disperse its strength to meet the requirements of gunboat diplomacy and virtually abandoned the West Indian station in 1904 in favor of concentration in home waters. Thus Britain therefore was in no position to contest the Caribbean area with the United States even if it had desired to do so, which it did not. Lansdowne and Grey readily accepted the political thrust of the Monroe Doctrine, but they continued to fear its possible economic consequences.

The author examines in some detail Anglo-American relations vis-à-vis Cuba, Santo Domingo, Panama, and Colombia. He then turns to the problems in Central America created by the Dollar Diplomacy of Taft and Knox. Finally, he traces the decline of Anglo-American cooperation, which resulted in part from the quarrel between the United States and England over the Panama Canal tolls. He concludes that the United States never quite lived up to the implications of the Roosevelt Corollary; it proved to be only a part-time policeman, and an unreliable bill collector of European debts. But both British investments and commerce greatly expanded in the Caribbean area during these years.

Kneer's work, though short, is based on solid sources. Interesting and informative, it illuminates how and why Britain acted as it did in this relatively minor, though complex, corner of the world.

WILBUR DEVEREUX JONES
University of Georgia

Evaldo Cabral de Mello. *Olinda restaurada: Guerra e açúcar no nordeste, 1630-1654*. São Paulo: Editora da Universidade de São Paulo. 1975. Pp. 390.

At the Newberry Conference on Colonial Brazil in 1969, C. R. Boxer called for a moratorium on writing about Dutch Brazil in that so much of value had already been produced by H. J. Wätjen, F. A. Varnhagen, José Honório Rodrigues, Gonçalves de Mello, and Boxer himself. We are in some ways fortunate that the author of this book

did not heed this advice. Evaldo Cabral de Mello, a Pernambucan and an amateur historian only in the sense that he is not an academician, has produced a solid and exciting book that not only presents new materials, but also offers an interesting set of revisionary hypotheses concerning the motives of the Luso-Dutch War in Brazil and the means by which it was carried out. The author's diplomatic career has allowed him to visit the major archives of Spain and Portugal as well as those of Pernambuco and to make use of little-exploited collections such as *Guerra Antiga* at the Archivo Nacional de Simancas. This new material, coupled with a broad reading of the major social and economic studies of the European and Brazilian seventeenth century, makes this an especially valuable and well-rounded monograph.

The central thesis of the work is that the Luso-Dutch War was a sugar war not only in the commonly held sense that it was a war for sugar, but rather that it was also a war made possible by sugar. The sugar sector had to mobilize its human and economic resources to win the war, but when victory was accomplished in 1654 the means of winning the war—taxes, forced migration, confiscation, commercial disruption—and the subsequent general period of stagnation in the Atlantic economies created a crisis for the northeastern sugar economy from which it did not recover until the nineteenth century. Although others have noted the Brazilian contribution to the war, Cabral de Mello makes it painfully clear that the almost totally Brazilian financing and manning of the War of Restoration (1645-54) was more responsible for the decline of the sugar industry in the northeast than the destruction of the military campaigns themselves. After 1654 poor international prices for sugar and a lack of capital for rebuilding in the northeast created a closed circle in which the crown was forced to continue its heavy taxation because of low revenues, and the fiscal burden prevented capital accumulation and reinvestment in new or expanded mills. Thus, while the book is primarily about the Luso-Dutch War, its implications are much broader, and these are largely but not always recognized by the author.

Forgoing a chronological recounting of the events, the author has organized *Olinda restaurada* into eight topical chapters that deal with aspects of the central theme described above. Most interesting and original are three chapters concerning the financing of the war, the recruitment of troops, and the provisioning of the armies. These chapters use detail, rather than quaint descriptions of minutiae, to support the major theses. Cabral de Mello demonstrates, for example, how the desire to save oxen needed by the sugar mills led to a

slaughter of cows to feed the troops and that this in turn later caused a shortage of oxen. Such details are set in a broad perspective that includes comparisons of the Luso-Brazilian armies to those described by Geoffrey Parker in Spanish Flanders and correlations between the timing of the revolt and the Atlantic crisis as analyzed by Ferdinand Braudel, Pierre Chaunu, and Ruggerio Romano. The falling price of sugar in Amsterdam, more than "nativism" or religious enmity, caused the "War of Divine Liberation."

Olinda restaurada is proof that seemingly well-worn topics can yield important new insights to serious research and historical imagination. Cabral de Mello must now have a place alongside those authors mentioned at the outset of this review.

STUART SCHWARTZ
University of Minnesota,
Twin Cities

ABRAHAM F. LOWENTHAL, editor. *The Peruvian Experiment: Continuity and Change under Military Rule*. (Written under the auspices of the Center for Inter-American Relations.) Princeton: Princeton University Press. 1976. Pp. xvi, 479. Cloth \$22.50, paper \$6.95.

The only way to hint at the range of this book on the endeavors of the Peruvian military, beginning in October 1968, to forge a new Peru and a new Peruvian is to indicate some of the conclusions of the contributors. First, though, I shall hazard a general evaluation. The essays with two exceptions sustain an unusually high level of excellence and are based on exhaustive research. Lowenthal's opening chapter disappoints, being far less perceptive and probing than the introductory essay David Chaplin provides to his edited book *Peruvian Nationalism: A Corporatist Revolution* (1976), a more helpful book than the one under review for the general reader interested in an interpretive framework and willing to dispense with massive detail. Notwithstanding its challenging insights, Julio Cotler's analysis of the new mode of political domination is marred by ideological rigidity and repetitious Marxian jargon. This will commend the essay to many readers whose judgment may be sounder than mine.

Richard Webb finds the military government has accomplished a little, but no more than that, toward stepping up progressive income redistribution. David Collier demonstrates that in its policies toward squatter settlements the officers have drawn on policies of previous governments. The difference is that the new regime is moving, with some success, to incorporate the urban poor into corporative control systems aimed at undermining traditional parties.

Dealing with the transformation of the rural sec-

tor, Susan C. Bourque and David Scott Palmer stress the diversity that has impeded accomplishments and resulted in vastly differing levels of social mobilization, and they conclude that by increasing the number and level of demands placed upon it, the regime is jeopardizing its ability to carry out objectives. On a related topic, Colin Harding writes that once the political decision was made to carry out serious agrarian reform, "the conflicting interests and claims upon the land released by the process itself made either increasing radicalization or tougher repression inevitable" (p. 221).

Education, as described by Robert S. Drysdale and Robert G. Myers, has become a tool of cultural pluralism with the objective of maintaining the Quechua language and culture alongside the Spanish. At the university level, government attempts to mobilize the academic community have been a notable failure. Shane Hunt skillfully shows that rather than throwing out foreign investors the government has sought to tame multinational capital and to make it work better for national interests. Foreign investors now view the regime as a sometimes capricious and always formidable adversary at the bargaining table.

Peter T. Knight describes the attempt to introduce worker-managed firms within a pluralistic economy, including important private and state sectors. Noting widespread objections to the experiment, he foresees trouble, as do virtually all the authors. In the concluding essay Jane S. Jaquette finds that the government bureaucracy has not been able to adjust to the role of entrepreneur, that the officers are trying to build a new society on the basis of an unprecedented and perhaps fatally weak political combination (Lowenthal properly drew attention to the lack of widespread and enthusiastic support for the government), and that it is not yet clear whether they "will be able to create a sound institutional base to give substance to the Revolution."

FREDRICK B. PIKE
University of Notre Dame

MARTIN WEINSTEIN. *Uruguay: The Politics of Failure*. Westport, Conn.: Greenwood Press. 1975. Pp. xvii, 190. \$13.50.

Focusing on twentieth-century Uruguay, the "proverbial Switzerland of South America," Martin Weinstein sets out to explain why a nation regarded as vastly different from and superior to its neighbors so quickly fell and came to resemble them. After establishing the theoretical framework, the author describes and analyzes Uruguay's development during the first half of the twentieth century. He discusses the impact of José Batlle y Ordóñez and his egalitarian political phi-

losophy, *Batllismo*, as well as that of his most notable political opponents: the famous *Blanco* leader Luis Alberto Herrera, the *Federación Rural*, and Benito Nardone's *Liga Federal de Acción Ruralista*. The latter group promoted a conservative vision of Uruguayan society based on religion, land, free trade, and idealization of rural life. Weinstein argues that this *ruralista* philosophy ultimately had a greater impact on shaping post-1930 Uruguayan politics than *Batllismo*. The author explains that these conservative groups rose to power, in part, through the clever use of the politics of co-participation and the failure of the *Batllista* state-directed economic programs.

Examining the history of co-participation reveals its ability to contain conflict as well as its tendency to divide the nation into two competing political parties each representing the "patria" for its constituents. The parties garnered support by controlling political patronage in the vast state bureaucracy and government enterprises; Weinstein stresses that this particularistic and clientelistic brand of politics did not offer Uruguay either the leadership or mechanisms necessary to exercise power in an effective national manner. To reinforce his theory of the corporatist and divided nature of Uruguayan society, the author utilizes demographic, economic, and educational data to demonstrate the lack of factors promoting geographic and economic integration and social mobility. In the final chapter, Weinstein argues that the failure of both *Batllismo* and the politics of co-participation opened the way for today's military-dominated authoritarian regime, which emerged in a climate of economic chaos, political stasis among traditional parties, and mounting urban guerrilla warfare. As in so many other nations, Uruguay's leaders chose class and privilege over the development of an egalitarian national community.

This compact and informative book provides an incisive analysis of Uruguay's "politics of failure" and subsequent "fall" in the 1960s and '70s. It is the best book in English on twentieth-century Uruguayan political history. Included is an excellent bibliography of primary and secondary sources in both English and Spanish. Even though *Uruguay: The Politics of Failure* will have most appeal for Latin Americanists, other area specialists will find it useful for comparative purposes.

CYNTHIA LITTLE
Philadelphia, Pennsylvania

MANUEL GARCÍA SORIANO. *El periodismo tucumano (1817-1900): Ensayo de investigación sobre un aspecto de la cultura de Tucumán durante el siglo XIX*. (Cuadernos de Humanitas, number 38.) Tucumán: Universidad Nacional de Tucumán. 1972. Pp. 113.

This work is the product of love and dedication. For over fifty years Manuel García Soriano patiently labored to locate and identify all the newspapers published in the Argentine province of Tucumán between 1817 and 1900. In recent times he also had the help of his students at the Universidad Nacional de Tucumán. His undertaking was an important one, not only for the history of journalism but also for the political, cultural, and economic history of the province. He was able to identify 148 newspapers, and he has classified them as follows: political, literary, educational, scientific and technical, official, student (elementary and secondary), Catholic, Masonic, humorous, satiric, occasional, union, and immigrant. He also identifies women's publications and publications appearing outside the provincial capital. Over half of the political newspapers cataloged appeared after 1885, and over half of the others after 1890. Between 1888 and 1892 political fervor led to the founding of nineteen newspapers, the largest number for any four-year period. These had an ephemeral existence, for all but two of them are known only because they are cited in other newspapers. Newspaper references rather than extant copies actually account for slightly over half of the political newspapers listed. The description of the others is based on isolated numbers, facsimiles, or incomplete sets at the Biblioteca Alberdi, Sociedad Sarmiento, Instituto Lillo, or in private libraries.

The author points out in his valuable introduction that content analysis reveals that some political newspapers failed for lack of funds or because of persecution by the local authorities, while others furthered the political ambitions of their founders and editors, many of whom became national senators and congressmen. Others reflected the struggle between and within the leading provincial families. A popular press does not appear until after 1890. However, even at that late date, provincial administrations were establishing their own journals and selecting editors from the provincial or municipal bureaucracy.

In sum, García Soriano has compiled the definitive list of pre-1900 newspapers in Tucumán. We need a similar work for each of the Argentine provinces.

JOSEPH T. CRISCENTI
Boston College

PETER H. SMITH. *Argentina and the Failure of Democracy: Conflict among Political Elites, 1904-1955*. Madison: University of Wisconsin Press. 1974. Pp. xx, 215. \$12.50.

Peter H. Smith's book is a thought-provoking examination of the failure of democratic institutions to take root in Argentina. He argues effectively

that the "pathological approach to Argentine politics, which has dominated so much of the literature . . . , is mis-placed and ill-conceived" (p. 111). That Argentina should have turned to authoritarian government "need not come as a surprise or a betrayal" (p. 112). Democratic institutions failed because of recurrent crises, logical though inadequate responses, and ultimate systemic collapse. Profound legitimacy crises in 1930, 1943, and 1955 helped trigger military intervention. Also important to the failure of democracy is the "dialectical relationship between the patterns of socioeconomic development and the patterns of political transition" (p. 113). When the socioeconomic elite began to lose control over the political order between 1916 and 1930 some sectors sought a restoration of government by *acuerdo*.

Such conclusions are highly speculative and demand further study and refinement. Virtually all of Smith's hypotheses rest on a sophisticated statistical analysis of the roll call votes in the Argentine Chamber of Deputies between 1904 and 1955. The substantive results are fascinating and richly suggestive. The methodology used, while impressive, should give pause for reflection, particularly the use of factor analysis, which has come under increasing attack by social scientists. Ambiguity in the categorization of factors is the most serious shortcoming of this complex methodology. Smith is well aware of the difficulties, and most of his findings are properly qualified.

With the aid of the computer Smith is able to survey Argentina's political transition from the closed system of the oligarchy, through the more open politics of middle-class Radicals, to the authoritarian populism of Perón as a "coherent and unified process" (p. xvii). The Chamber of Deputies, an unparalleled "arena of conflict and contention," is his explicative vehicle. Not all scholars will agree with Smith's assessment of the importance of the Chamber of Deputies as a mirror of society, or facets of society. Nor does voting analysis, as Smith realizes, necessarily reveal motivation. And what was the nature of debate and cleavage in committee—behind closed doors and beyond public scrutiny? Complaints aside, Smith's book is a significant, challenging, and important study that should serve as a springboard to further research.

PAUL B. GOODWIN, JR.
University of Connecticut

ALBERTO CIRIA. *Parties and Power in Modern Argentina (1930-1946)*. Translated by CARLOS A. ASTIZ, with MARY F. MCCARTHY. Albany: State University of New York Press. 1974. Pp. xv, 357. \$15.00.

More and more scholars interested in modern Argentina are focusing their attention on the critical,

and often neglected, period from 1930 to 1946. Among the most important additions to the growing literature in English on these years is this translation of a work by one of Argentina's most knowledgeable and perceptive writers. When Albert Ciria's study first appeared in Spanish in 1964 it was widely acclaimed for its thorough documentation, its detailed analysis of a complex and confusing period, and its stimulating hypotheses. The second Spanish edition (1968) updated bibliographical references and expanded the final chapter to apply the patterns of the pre-Perón period to the post-Perón era. The current English edition is based on the second Spanish edition.

The book consists of two parts. The first presents a chronological description of Argentine political history from 1930 to 1946 organized around the four presidential administrations of Uriburu, Justo, Ortiz-Castillo, and Perón. The second part is an examination of important power groups: the political parties, the Church, the armed forces, the economic groups, and the labor movement. The analysis in both parts is supported by extensive reference to primary and secondary sources, in particular to the frequently overlooked *Diario de sesiones* of the Argentine Congress, and by nearly two dozen tables.

The importance of the book lies essentially in its explanation of the parliamentary crisis that has afflicted Argentina since the 1930s. Ciria's thesis is that the crisis is the result of the basic inapplicability to twentieth-century Argentina of the liberal political system established by the Constitution of 1853. Under external as well as internal pressure, the liberal system disintegrated during the 1930s. Political parties ceased to be the centers of political power and were replaced by the armed forces, by various economic groups, and, to a lesser extent, by the labor movement.

One may disagree with this thesis or with some of the subtheses of the author, but this is an important book on a critical period in Argentine history that is now available to the English-reading audience.

SAMUEL L. BAILY
Rutgers University

HAROLD BLAKEMORE. *British Nitrates and Chilean Politics, 1886-1896: Balmaceda and North*. (University of London Institute of Latin American Studies Monographs, number 4.) London: Athlone Press, and University of London, for the Institute; distrib. by Humanities Press, Atlantic Highlands, N.J. 1974. Pp. viii, 260. \$13.50.

Harold Blakemore demonstrates that Marxian interpreters, both national and foreign, have woven a web of mythology around the events of 1886-91 in Chile. The foremost hero to these interpreters has been José Manuel Balmaceda, the president of Chile during these troubled years that ended in

civil war. He has been depicted as an economic nationalist intent upon challenging the control over his country's economy wielded by British investors in league with an emerging Chilean bourgeoisie avid for personal profit and unconcerned with national well-being. The villain is John Thomas North who, born into reasonably favorable middle-class circumstances near Leeds in Yorkshire, became the most notable and visible foreign investor in Chilean nitrates and acquired the title of "the Nitrate King." Anxious to retain more nitrate income in Chile for use in his public works projects and other programs aimed at national betterment, Balmaceda is supposed to have clashed with North and his Chilean henchmen who in turn combined to precipitate the civil war that overthrew the president, setting the stage for his suicide in 1891. As a consequence of these events, Chile allegedly fell into the hands of international and national capitalists whose direction ushered in a prolonged period of exploitation resulting in a consistent economic drain that exacerbated and perpetuated underdevelopment.

Basing his study on a thorough use of published accounts and manuscript sources, Blakemore challenges this interpretation and effectively undermines its credibility, but he fails, in my judgment, to demolish it altogether. It would appear that there simply is not enough documentation available to resolve some of the points of the controversy, and the author is frank in conceding this.

In the brief concluding section of his valuable and dispassionate study Blakemore argues persuasively that Balmaceda was not antagonistic to foreign capital in itself. His ire was aroused only when foreign capital did not provide his regime desired amounts of income. Balmaceda was not seeking to end Chilean dependence on foreign capital but rather to increase that dependence under conditions permitting his government to derive a larger share of benefits to be used for immediate political advantage as much as for long-range national development. In this interpretation, Balmaceda emerges as a precursor of ever so many twentieth-century Latin American leaders whose economic nationalism must be viewed with skepticism. Balmaceda's relations with foreign capital, regardless of what motives shaped them, were not nearly so important in causing his downfall as purely internal political issues. Throughout his book Blakemore is effective in arguing this last point.

FREDRICK B. PIKE
University of Notre Dame

THEODORE H. MORAN. *Multinational Corporations and the Politics of Dependence: Copper in Chile*. (Written under the auspices of the Center for International Affairs, Harvard University.) Princeton: Princeton University Press. 1975. Pp. xiv, 286. \$12.50.

Theodore Moran's analysis of the relationship between multinational corporations, particularly Anaconda and Kennecott, and the Chilean economy and political system is a case study in the best sense of that term. It provides a test of prevailing dependency theory, while at the same time serving to generate a suggestive new model of the relationship between multinational corporations and their host countries.

By means of an analytical history of the formation of copper policy in Chile from the end of World War II through the end of the Allende regime, Moran shows that a bargaining, or balance of power, model better fits the Chilean experience than does the standard dependency model. The essence of the argument is that the multinational corporations had the bargaining advantage at the outset when the host country needed the capital and technology that only the businesses could provide. However, once large capital outlays had been committed, Chile tended to gain in technical and negotiating skills, gradually acquiring the upper hand until nationalization became almost inevitable. Concomitantly, the original coalition between foreign corporations and domestic elites shifted over time as well, until by the late 1960s Chilean industrial and agrarian elites themselves backed nationalization. Moran ends this book by recommending to Chile that its nationalized copper industry would best maximize its own economic interests by, in effect, joining the international copper oligopoly and playing by its rules.

There is little in this book concerning Chilean political history in the usual sense—Frei's victory in 1964 over Allende is even mistakenly called "narrow" (p. 127)—and political scientists and economists will probably welcome the book more than will historians. It is curious that there is only brief mention of those important Chilean political actors, the copper workers' unions. This is, however, a very impressive and innovative study, one that, while not rejecting dependency theory in its entirety, nonetheless brings the discussion of that theory down from the plane of rhetoric and ideology to the more fruitful level of empirically based analysis.

ROBERT DIX
Rice University

Recent Deaths

When on February 26, 1976, ARTHUR C. COLE died, the historical profession lost one of its most distinguished members. Born on April 22, 1886, at Ann Arbor, Cole was graduated from the University of Michigan, received his doctorate from the University of Pennsylvania, and in 1912 won the AHA's Justin Winsor Prize for *The Whig Party in the South*. From 1912 to 1920, he taught at the University of Illinois. In 1920 he became a professor of history at Ohio State and in 1930 moved to Western Reserve. From 1944 until 1956, he was a member of the Department of History at Brooklyn College and served as its chairman after 1950.

Cole was a leading scholar in his specialty. Managing Editor of the *Mississippi Valley Historical Review* from 1930 to 1940 and president of the Mississippi Valley Historical Association in 1940-41, he was one of the few authors of his era whose books on the sectional conflict have not been completely revised by modern scholars. Unlike most of his contemporaries, he never abandoned his belief in the justice of the antislavery cause. In *The Irrepressible Conflict* (1934) he forcefully argued the emergence of two civilizations as a cause of the Civil War.

Cole's contributions to scholarship were widely recognized. In addition to *The Irrepressible Conflict* and *The Whig Party in the South*—the latter a brilliant exposition of the coincidence of Whig voting strength and large-scale slave holding—he wrote *The Era of the Civil War* (the third volume of the *Centennial History of Illinois*) and *A Hundred Years of Mount Holyoke College*, and edited the *Constitutional Debates of 1847* in the *Collections* of the Illinois State Library. His exchange with Joseph G. de Roulhac Hamilton in the 1931 and 1932 *AHR* on "Lincoln's Election an Immediate Menace to Slavery in the States?" became famous (he took the negative view), and his many articles in learned and popular periodicals made him one of the best-known figures in his field.

Cole's commitment to libertarianism was not confined to his writing and teaching. Opposed to American involvement in World War I, he was

investigated by a committee of trustees at the University of Illinois. Subsequently, he played a prominent role in launching a teachers' union at the university. While at Ohio State, he challenged the program of compulsory military training, and as editor of the *Mississippi Valley Historical Review*, reviewed fairly W. E. B. DuBois' *Black Reconstruction*, which at that time was not even considered by other publications. He was an active member of the AAUP and in 1950 became chairman of the ACLU's Committee on Academic Freedom. During World War II, he served on a special Air Force committee to study the feasibility of reducing Germany by strategic bombing alone, a proposition which he correctly held to be erroneous.

Above all, Cole was a great human being. His interventions on behalf of colleagues and his skill as a teacher endeared him to faculty and students alike. The *Festschrift* intended for his ninetieth birthday will now have to be issued *in memoriam*, but it is certain that whenever American history is taught, the name of Arthur C. Cole will not be forgotten.

HANS L. TREFOUSSE
*Brooklyn College and Graduate Center,
City University of New York*

DANIEL COSÍO VILLEGAS, dean of historians of modern Mexico, died suddenly in his nation's capital on March 10, 1976, at the age of 77. He was a man of indefatigable energy, active until the end. In addition to his prolific historical writing, begun in middle life, Cosío had an extraordinary public career as a builder of institutions. From 1957 to 1968 he served as Mexico's special representative on the United Nations Economic and Social Council. In 1960 he presided over the Council.

Born in Mexico City on July 23, 1898, Cosío Villegas emerged in the early twenties as a brilliant student with a deep commitment to the regeneration of a nation racked by violent revolution. He studied abroad from 1925 to 1928, receiving an M.A. in economics from Cornell University. In 1934 he established the journal *El Trimestre Eco-*

nómico and the major publishing house, Fondo de Cultura Económica. As a diplomat in Lisbon in 1937, he initiated the project of inviting to Mexico a select group of Spanish intellectuals, refugees of the Civil War. A year later he became a cofounder of La Casa de España en México, later El Colegio de México, which is now one of the major graduate and research institutions in Latin America. He also founded in 1951 the principal national journal in the field, *Historia Mexicana*.

In 1948 Cosío Villegas began work on his magnum opus and greatest enterprise, the 9000-page *Historia moderna de México* (1955–1972). The collaborative *Historia* was a product of his famous “seminar,” which also served as a training school for young historians. Besides directing the project, Cosío wrote five of the nine volumes, those devoted to international and domestic politics between 1867 and 1910. Because of its massive documentation and its objectivity in a politicized but unstudied field, the *Historia moderna* has become the point of departure for all serious studies of the period. Soon after completing the final volume of the *Historia moderna*, Cosío started and codirected a new Seminario de la Historia de la Revolución Mexicana, the fruits of which are soon to appear.

The *Historia moderna* emerged from Cosío’s concern that the country after 1940 was drifting away from the ideals of constitutional liberalism that had moved the mid-nineteenth-century reformers, the leaders of the Restored Republic (1867–1876), and the early revolutionaries after 1910. For Cosío history must help guide the nation in the search for priorities. In his histories as in his much-read essays he spoke as a liberal, always lamenting the withering of democratic institutions, the authoritarianism, and the pervasive servility in government that made his era reminiscent of that of Porfirio Díaz (1876–1911). His Friday column in the newspaper *Excelsior* became an institution, unique for its penetrating and fearless criticism of the political system.

Though a commanding and dominant figure among his colleagues, Daniel Cosío Villegas was always open to new ideas. He maintained close contact with younger scholars and treated foreign investigators with generosity and respect. In short, he did much to give new life to the professional study of modern and contemporary Mexico, both within the country and abroad.

CHARLES A. HALE
University of Iowa

For various reasons, personal or circumstantial, it is not given to every historian to incarnate the period he has made his own. LEWIS PERRY CURTIS, who died at his home in New Haven on April 8, 1976, so steeped himself in eighteenth-century Eng-

lish politics and society that, with little make-up, he could have played Lord Chesterfield redivivus. The man who knew him best has affectionately recalled the eighteenth-century atmosphere of his household wherein, not altogether paradoxically, Georgian pronouncements assayed twentieth-century vagaries. However deeply committed to a particular past one may be, it is too late to inhabit it exclusively: the present will inspire comparisons. Among the many mirrors of his favorite world in which Curtis was reflected, two closely set together, the epistolary and instructive, may be seized upon as revealing his character and interests.

It is not surprising that a man whose major scholarly book was the meticulously edited *Letters of Laurence Sterne* (Oxford, 1935, 1965) would indulge in similar expression. Since Sterne’s wit and wisdom frequently turned in directions other than instruction, however, Curtis in his own letters to his son followed the Chesterfieldian line—learned, allusive, magisterial, and witty (in the eighteenth-century sense)—heightened by the insights of Burke and Gibbon. The flavor as well as the substance of the eighteenth century permeate all his writing, *The Politics of Sterne* (1929), *Anglican Moods* (1966), and especially the more personal idiosyncratic *Chichester Towers* (1966). Whatever he wrote, whether of Sterne or Gibbon or Archdeacon Ball, he never reduced his subjects to a statistic. That he was a brilliant as well as dedicated teacher comes as no surprise. An unregenerate humanist, he sought always to portray the whole culture by fusing its several parts, never better illustrated than by his understanding of the pervasive importance of religion, even at its most formal, in contemporary life.

Born at Southport, Connecticut, November 30, 1900, he began his professional career in 1927, after graduating Yale B.A. (1923), Ph.D. (1926), as instructor in English Literature at his alma mater, where he was to remain throughout his academic life. In 1933 he moved to the Department of History, without in any sense shifting his intellectual allegiance, retiring in 1969 as Colgate Professor of History. He held ACLS and Guggenheim fellowships in 1931–32 and 1941–42 respectively. During his forty-two years of service he sought constantly, by precept and practice alike, to promote the teaching of history. For him as for David Hume, the historian was a man who had lived from the beginning, and if men would exemplify the highest values they must know from whence they had come.

CHARLES F. MULLETT
University of Missouri

DOROTHY BURNE GOEBEL, professor emeritus of history at Hunter College of the City University of

New York, died on March 13, 1976, in Huntington, Long Island after a long illness. Mrs. Goebel was born in Huntington on August 24, 1898. She received the A.B. in 1920 from Barnard College, where she was elected to Phi Beta Kappa, and the M.A. in 1922 and the Ph.D. in 1926 from Columbia University. She was an assistant and lecturer in the history department at Barnard from 1920 to 1926 when she joined the history department at Hunter as instructor. Subsequently she became professor and served as chairman of the department, 1942-48 and 1961-62. She retired as professor emeritus in 1963.

As chairman and as a member of many important committees at Hunter, Dorothy Goebel made significant contributions to academic, educational, and scholarly achievements at the college. Within the department, in addition to giving distinguished administrative leadership, she played a leading role in originating and directing the required course in American history, which with its emphasis upon the interrelationship of the United States and other countries of the Western community was long a unique offering at Hunter.

A specialist in American history, she wrote *William Henry Harrison* (1926) and *American Foreign Policy: the Documentary Record, 1776-1960* (1961). She contributed to the *Dictionary of American Biography* and the *Dictionary of American History*. Articles appeared in the *American Historical Review*, *Journal of Economic History*, and *Annals of the American Academy of Political and Social Science*.

A striking part of her scholarly record was her cooperation with her husband, the late Julius Goebel, Jr., a distinguished legal historian and professor on the Law Faculty of Columbia University. Shortly after their marriage in 1925 she assisted in the survey of colonial court records in the state of New York that became the basis for Julius Goebel's first major contribution to the history of law, *Some Legal and Political Aspects of the Manors of New York* (1928). In 1945 the Goebels wrote *Generals in the White House*, a "lively study" of the military and political careers of nine generals who became president. From 1963 Mrs. Goebel was associate editor of the *Law Practice of Alexander Hamilton: Documents and Commentary*, edited by Julius Goebel. The first two of four projected volumes appeared before his death in 1973. The full extent of this partnership has been well expressed in the memorial volume for Julius Goebel prepared by the *Columbia Law Review*, which describes the extraordinary extent to which Dorothy and Julius Goebel shared professional, social, and cultural interests—and "sparkling wit."

Mrs. Goebel was a member of the American Historical Association from 1933, serving on the Albert J. Beveridge Award Committee, 1947-53, as chairman, 1951-53. She received a Guggenheim

Fellowship in 1947 to study the free ports of the West Indies. The results of this research appeared in the *William and Mary Quarterly* (July 1963) under the title "The New England Trade and the French West Indies, 1763-1774: a Study of Trade Policies."

Perhaps a final word is permissible for one who was a student in Dorothy Goebel's first history class at Barnard College fifty-four years ago and her colleague in the history department of Hunter College for many years. We respect the memory of a brilliant historian and gifted scholar, but we also cherish with affectionate admiration the personal integrity, the sensitivity of spirit, the exquisite taste, and the practical good sense so evident in her private as well as professional life.

MADELEINE HOOKE RICE
Hunter College,
City University of New York

ALFRED H. KELLY, professor of history at Wayne State University, died in Detroit on February 14, 1976. Born in Pekin, Illinois, on June 23, 1907, he completed his undergraduate and graduate work at the University of Chicago. He came to Wayne State University in 1935 to begin his career as a professor in the field of U.S. constitutional history and to be an administrator, an active and important faculty member, and a guide to thousands of students who knew how deeply interested he was in their hopes and achievements.

For twenty-two years Alfred Kelly was chairman of the Department of History. In 1953 he was appointed Franklin Lecturer. He served on numerous university committees and councils. He taught at the University of Tübingen and was decorated by the government of Italy. In 1954 he assisted Thurgood Marshall and others in preparing the brief filed in behalf of the NAACP in the case of *Brown v. Board of Education of Topeka* that resulted in a Supreme Court decision that soon came to symbolize the Court's determination to destroy segregation. In 1961 he was codirector for research and drafting for the Michigan Constitutional Convention. He was later appointed by President Nixon to the Permanent Committee for the Oliver Wendell Holmes Devise, responsible for preparing a history of the Supreme Court.

For several years Alfred Kelly was parliamentarian of the American Historical Association and the Organization of American Historians. At the time of his death he was Acting Dean of the Graduate School and a member of the Bicentennial Commission of Detroit.

Kelly lectured widely in the United States and Canada. He was a prolific writer of reviews, articles, and books, all of them marked by his usual logic and patterned precision. Among his best-known works are *The American Constitution: Its Origins and Development* (with W. A. Harbison), now in

its fifth edition, *American Foreign Policy and American Democracy*, and *Foundations of Freedom*. One of his last and finest papers was published by the Library of Congress in 1974 in *Leadership and the American Revolution*.

Those who knew Alfred Kelly well will remember him as a man of tremendous energy, much knowledge, eloquence, compassion, courage, integrity, insight, and humor. He was a distinguished professor. He was also a success as a person, and that is perhaps the highest praise that any man may have.

GOLDWIN SMITH
Wayne State University

GEORGE SIDNEY ROBERTS KITSON CLARK, a major contributor to the study of nineteenth-century British history, was born at Leeds, June 14, 1900. His life was intimately associated with Trinity College, Cambridge, where he came up as an undergraduate in 1919, and was a Fellow from 1922. He was Reader in Constitutional History at Cambridge from 1954 until his retirement in 1967. He had appointments as visiting lecturer at the University of Pennsylvania in 1953-54 and the University of Melbourne in 1964. He served as Ford's Lecturer at Oxford in 1959-60, as Maurice Lecturer at King's College, London, in 1960, and as Birkbeck Lecturer at Cambridge in 1967.

After his early book, *Peel and the Conservative Party* (1929), Kitson Clark published almost nothing for many years. He was heavily engaged in college and university teaching and in administrative tasks at Trinity College, where he served as tutor from 1933 to 1945. His most interesting work appeared in the last third of his life when he was over fifty, a period for him of substantial production and also of rapid intellectual growth. He attracted wide attention in the early 1950s by three excellent articles dealing with different phases of the politics of the 1840s. Some of the themes of these papers were picked up and further developed in books that dealt with the mid-nineteenth century in more general terms, notably *The Making of Victorian England* (1962), based on his Ford lectures, and *An Expanding Society: Britain 1830-1900* (1967). His interest in the history of religion in Britain and in its relation to other matters was reflected in *The English Inheritance* (1950) and *The Kingdom of Free Men* (1957), as well as in the last book he published during his lifetime, *Churchmen and the Condition of England, 1832-1855* (1973). He continued also in this last and most fruitful period to publish at intervals articles and long book reviews—really essays for which the book at hand furnished a starting point—that were substantial contributions to the discussion of limited subjects.

In dealing with the general questions that most interested him, Kitson Clark was discerning in

summarizing the existing state of knowledge and imaginative in bringing out the implications of his topic. Though conservative in some of his intellectual habits, he was also open-minded and welcomed new ideas and new approaches. He came, after some initial resistance, to have an informed and judicious understanding of what could be learned from statistics and systematic analysis and, though he did not much follow these lines himself, was cordial to those who did.

Kitson Clark was concerned not only with his own projects but also with the general advance of knowledge of his field, and his friendships with other historians and his contributions to the furthering of their work constituted an important part of his career. He was a famous research supervisor, and many of his students at Cambridge have now become distinguished scholars. His efforts, however, reached a wider audience. His continued interest in the problems faced by younger men resulted in his publication of several small tracts for their benefit on research methods, the location of research facilities, and the art of lecturing. He gave generously of his time and encouragement not only to his own students but also to those who came from elsewhere: I have seen something of this in his great kindness to my graduate students when I have given them introductions to him. Younger scholars, both in England and America, sent him their manuscripts, sometimes of book length, and, although he must have been overtaxed, he went through them painstakingly, offering suggestions that, in some cases, resulted in major recastings. He lived in a suite of big rooms lined with books over the Great Gate at Trinity College and, in these quarters, was wonderfully hospitable.

Kitson Clark never received a chair—many at Cambridge felt strongly that he should have—but his accomplishments were recognized in other ways, by honorary degrees from four universities, by the volume of essays in his honor edited by R. Robson in 1967, by his election as a Foreign Hon. Member of the American Academy of Arts and Sciences in 1975, and by the respect and affection of scholars in his field all over the world. He died suddenly and peacefully in his rooms at Trinity the night of December 8, 1975, while lying in bed reading *The Woman in White*. He was active and in good form up to the end and, in his last weeks, was working on a piece about the medical profession and social reform in the nineteenth century. His annotated edition of G. M. Young's *Victorian England: Portrait of an Age* was completed before his death.

WILLIAM O. AYDELOTTE
University of Iowa

LOUIS MORTON, American historian and the Daniel Webster Professor at Dartmouth College, died

in Burlington, Vermont, on February 15, 1976. Pre-eminent in the field of military history, Morton was also active as a commentator on contemporary national-security affairs and the teaching of history. He was always willing to lend his tireless energies and formidable talents to the work of the AHA and other scholarly organizations. He served Dartmouth with unstinting dedication; he joined its faculty in 1960, was chairman of its history department for six years, and in 1971-72 was acting provost.

Born in New York in 1913, Louis Morton received his B.S. and M.A. degrees from New York University, his doctorate from Duke University in 1938. His dissertation, "Robert Carter of Nomini Hall: A Virginia Tobacco Planter of the Eighteenth Century," was revised and published in 1941. Still in print, it stands as a small classic in the literature of colonial social history. War interrupted Morton's career as a colonialist after he had taught at the College of the City of New York and served as a researcher at Colonial Williamsburg. He entered the Army in 1942 and saw duty in the Pacific theater.

Then Kent Roberts Greenfield recruited him as one of that remarkable group of young historians who staffed the Office of the Chief of Military History to work on the official history of the Army in World War II. Morton remained in that post from 1946 to 1959, charged with supervising the series on the War in the Pacific. Its first volume was his magisterial study, *The Fall of the Philippines* (1953). It immediately won him recognition as a major figure in the field of military history.

Louis Morton never made half-way commitments. For him, the Army project was a venture that could give new meaning to "official" history—to make it professionally written history of the highest quality, free of apologetics, exploiting a unique opportunity to gain access to archival records and major historical actors. The task of bringing modern military history into the mainstream of historical scholarship absorbed him. "Here indeed," he wrote in a 1962 article on the historian and the study of war (*MVHR*, vol. 48), "is a task to challenge the historian, an opportunity to use his knowledge and skill to enlarge our understanding of war so that ultimately, if we cannot abolish it entirely, we may learn how to limit and control it so that it does not destroy us altogether."

His crowning scholarly achievement was his book *Strategy and Command: The First Two Years* (1962), which appeared in the Army series and is acknowledged to be one of the definitive works in his field. It reflects well Morton's view that history done properly embodies "scrupulous objectivity, imaginative reconstruction of past events, orderly

presentation, a broad knowledge of society, and an understanding of and appreciation for the dynamic forces of change." On this same premise, moreover, he contended that the historian's expertise ought to be brought to bear on the consideration of public policy, and his faith in the instrumental value of our discipline also reinforced his dedication to undergraduate teaching.

Program chairman for the AHA annual meeting in 1967, consulting editor to numerous journals, member of the history advisory groups of both the Army and the Air Force, president of the New England Historical Association, he was a professional's professional. His colleagues in these organizations doubtless remember him for his powerful intellect, his forceful personality, and his graciousness. Those of us who were Lou's colleagues at Dartmouth also felt the effects of his other gifts of wisdom and generosity. He was a superb teacher and a highly successful chairman, always lavishing time and energies on support of his younger colleagues' work. He was gladly recognized at Dartmouth as *primus inter pares* by his fellow historians and other faculty.

Many of his activities at Dartmouth reached out to affect the historical profession. Notable in this respect was his development of a model course in military history—one of only a few in the country—that not only was offered to undergraduates generally, but also served in lieu of the conventional ROTC military history sequence. He played a key role in organizing at Dartmouth the Daniel Webster Papers project, which became a major scholarly undertaking under the direction of his close friend Charles Wiltse. Typical of many of Morton's efforts over the years was his quiet (indeed anonymous) effort to see that a senior colleague was honored recently with a *Festschrift* of high scholarly quality. He also led in broadening the range of fields taught in the Dartmouth department and required of majors, to include those outside of European and United States history, far beyond what was conventionally done elsewhere.

Although skeptical of the hectic expansion of history graduate programs in the 1960s (he declined appointments at two major universities), Morton was frequently consulted at Dartmouth by graduate students from other institutions, and his services as lecturer were always in demand.

All the while, he continued to write journal articles in military history; he contributed a brace of brilliant essays to *Command Decisions* (ed., Kent Robert Greenfield, 1960); and he wrote on the historiography of his field in the *AHR*, in *World Politics*, and in other reviews. He was also coauthor of *Schools for Strategy: Education and Research in National Security Affairs* (1965). A major undertaking of the last years of his life, when uncomplainingly he

often worked in great physical pain as a heart ailment grew worse, was the splendid series that he conceived and edited, *The Macmillan Wars of the United States*.

Always a person of quiet strength and enormous integrity, Louis Morton leaves a wide circle of

friends whose association with him and with his family was a privileged opportunity. He also leaves the historical profession much in his debt.

HARRY N. SCHEIBER
*University of California,
San Diego*

Other Books Received

Books listed were received by the *AHR* between March 1 and July 1, 1976. Books that will be reviewed are not listed, but listing does not preclude subsequent review.

GENERAL

- Actes et documents du Saint Siège relatifs à la seconde guerre mondiale.* Volume 9, *Le Saint Siège et les victimes de la guerre, janvier-décembre 1943.* (Secrétairerie d'État de Sa Sainteté.) Vatican City: Libreria Editrice Vaticana. 1975. Pp. xxviii, 686.
- AITKEN, HUGH G. J. *Syntony and Spark—The Origins of Radio.* (Science, Culture, and Society: A Wiley-Interscience Series.) New York: John Wiley and Sons. 1976. Pp. xvi, 347.
- AMUNDSEN, ROALD. *The South Pole: An Account of the Norwegian Antarctic Expedition in the "Fram," 1910-1912,* in two volumes. Translated from the Norwegian by A. G. CHATER. Reprint. New York: Barnes and Noble. 1976. Pp. xxxv, 392; x, 449.
- BAECHLER, JEAN. *Revolution.* Translated by JOAN VICKERS. (Key Concepts in the Social Sciences.) New York: Harper and Row. 1976. Pp. xxiv, 208. \$15.50.
- BAUMAN, ZYGMUNT. *Socialism: The Active Utopia.* (Controversies in Sociology, volume 3.) New York: Holmes and Meier. 1976. Pp. 150. \$13.50.
- BENTINCK-SMITH, WILLIAM. *Building a Great Library: The Coolidge Years at Harvard.* Cambridge, Mass.: Harvard University Press. 1976. Pp. xiii, 218. \$17.50.
- BEST, GEOFFREY, and WHEATCROFT, ANDREW, editors. *War, Economy and the Military Mind.* Totowa, N.J.: Rowman and Littlefield. 1976. Pp. 136. \$10.00.
- BLACK, CYRIL E., edited with an introduction by. *Comparative Modernization: A Reader.* New York: Free Press. 1976. Pp. vi, 441. \$12.95.
- BLACKEV, ROBERT. *Modern Revolutions and Revolutionists: A Bibliography.* Santa Barbara, Calif.: Clio Books. 1976. Pp. xxvii, 257. \$15.75.
- BRERETON, J. M. *The Horse in War.* New York: Arco Publishing. 1976. Pp. 160. \$12.95.
- BRINTON, CRANE, et al. *A History of Civilization.* Volume 1, *Prehistory to 1715;* volume 2, *1715 to the Present.* 5th ed. Englewood Cliffs, N.J.: Prentice-Hall. 1976. Pp. x, 432, xxxii; xvi, 436-928. \$10.95 each.
- BRISSENDEN, R. F., and EADE, J. C., editors. *Studies in the Eighteenth Century.* Volume 3, *Papers presented at the Third David Nichol Smith Memorial Seminar, Canberrra 1973.* Buffalo: University of Toronto Press. 1976. Pp. xi, 262. \$16.50.
- CARRIER, FRED J. *The Third World Revolution.* (Philosophical Currents, volume 13.) Amsterdam: B. R. Grüner. 1976. Pp. vii, 355.
- CARTTER, ALLAN M. *Ph.D.'s and the Academic Labor Market.* (Carnegie Commission on Higher Education Sponsored Research Studies.) New York: McGraw-Hill Book Company. 1976. Pp. xviii, 260. \$12.50.
- CHAUSSINAND-NOGARET, GUY, editor. *Une histoire des élites, 1700-1848: Recueil de textes présentés et commentés.* (École des Hautes Études en Sciences Sociales, Le savoir historique, number 6.) Paris: Mouton. 1975. Pp. 376. 64 fr.
- CIPOLLA, CARLO M., editor. *The Fontana Economic History of Europe.* Volume 3, *The Industrial Revolution, 1700-1914.* New York: Barnes and Noble. 1976. Pp. 624. \$19.50.
- CIPOLLA, CARLO M., editor. *The Fontana Economic History of Europe.* Volume 4, *The Emergence of Industrial Societies, Part 1.* New York: Barnes and Noble. 1976. Pp. 968. \$19.50.
- CORDIER, ANDREW W., and HARRELSON, MAX, editors. *Public Papers of the Secretaries-General of the United Nations.* Volume 6, *U Thant, 1961-1964.* New York: Columbia University Press. 1976. Pp. xviii, 708. \$27.50.
- COX, OLIVER C. *Race Relations: Elements and Social Dynamics.* Detroit: Wayne State University Press. 1976. Pp. xv, 337. \$17.50.
- DAVIES, W. D. *The Gospel and the Land: Early Christianity and Jewish Territorial Doctrine.* Berkeley and Los Angeles: University of California Press. 1974. Pp. xiv, 521. \$15.00.
- DEMAUSE, LLOYD, editor. *The New Psychohistory.* New York: Psychohistory Press. 1975. Pp. 313. \$12.95.
- DEVEREUX, GEORGE. *A Study of Abortion in Primitive Societies: A Typological, Distributional, and Dynamic Analysis of the Prevention of Birth in 400 Preindustrial Societies.* Rev. ed. New York: International Universities Press. 1976. Pp. xiv, 414. \$15.00.
- DICKINSON, ROBERT E. *Regional Concept: The Anglo-American Leaders.* London: Routledge and Kegan Paul. 1976. Pp. xxi, 408. £8.95.
- DODD, LAWRENCE C. *Coalitions in Parliamentary Government.* Princeton: Princeton University Press. 1976. Pp. xx, 283. \$14.50.
- ELIADE, MIRCEA. *Occultism, Witchcraft, and Cultural Fashions: Essays in Comparative Religions.* Chicago: University of Chicago Press. 1976. Pp. 148. \$8.95.
- ELTON, G. R., editor. *Renaissance and Reformation, 1500-1648.* (Ideas and Institutions in Western Civilization.) 3rd. ed. New York: Macmillan Company. 1976. Pp. xii, 362.
- EULER, HEINRICH, et al., editors. *Konferenzen und Verträge, Vertrags-Ploetz: Ein Handbuch geschichtlich bedeutsamer Zusammenkünfte und Vereinbarungen.* Volume 5, 1963-1970. Würzburg: Ploetz. 1975. Pp. xxviii, 484.
- FARQUHARSON, JOHN E., and HOLT, STEPHEN C. *Europe from Below: An Assessment of Franco-German Popular Contacts.* New York: St. Martin's Press. 1975. Pp. x, 218. \$17.95.
- FARR, WILLIAM. *Vital Statistics: A Memorial Volume of Selections from the Reports and Writings of William Farr.* With an introduction by MERVYN SUSSEX and ABRAHAM ADLSTEIN. (The History of Medicine Series Issued Under

- the Auspices of the Library of the New York Academy of Medicine, number 46.) Reprint. Metuchen, N.J.: Scarecrow Press. 1975. Pp. xxiv, 563. \$20.00.
- GALL, LOTHAR, editor. *Liberalismus*. (Neue Wissenschaftliche Bibliothek, number 85, Geschichte.) Cologne: Kiepenhauer and Witsch. 1976. Pp. 352. DM 26.
- HARDER, KELSIE B., editor. *Illustrated Dictionary of Place Names: United States and Canada*. New York: Van Nostrand Reinhold Company. 1976. Pp. xiv, 631. \$18.95.
- HAUSEN, KARIN, and RÜRUP, REINHARD, editors. *Moderne Technikgeschichte*. (Neue Wissenschaftliche Bibliothek, number 81, Geschichte.) Cologne: Kiepenhauer and Witsch. 1975. Pp. 431. DM 28.
- HAVELOCK, ERIC A. *Origins of Western Literacy: Four Lectures Delivered at the Ontario Institute for Studies in Education, Toronto, March 25, 26, 27, 28, 1974*. (Monograph Series, number 14.) Toronto: The Ontario Institute for Studies in Education. 1976. Pp. vii, 88. \$3.50.
- HEN-TOV, JACOB. *Communism and Zionism in Palestine: The Comintern and the Political Unrest in the 1920's*. Cambridge, Mass.: Schenkman Publishing Company. 1974. Pp. viii, 184.
- HINNELLS, JOHN R., editor. *Mithraic Studies: Proceedings of the First Congress of Mithraic Studies*, in two vols. Totowa, N.J.: Rowman and Littlefield. 1975. Pp. xx, 248; xii, 249-560. \$49.50, the set.
- HOLLINGER, DAVID A. *Morris R. Cohen and the Scientific Ideal*. Cambridge, Mass.: MIT Press. 1975. Pp. xvi, 262. \$14.95.
- HOLMES, J. DEREK, selected and edited by, with the assistance of HUGO M. DE ACHAVAL, S. J. *The Theological Papers of John Henry Newman on Faith and Certainty*. With an introduction by CHARLES STEPHEN DESSAIN. New York: Oxford University Press. 1976. Pp. xv, 170. \$15.95.
- HOWE, IRVING, editor. *Essential Works of Socialism*. Reprint. New Haven, Conn.: Yale University Press. 1976. Pp. 850. Cloth \$20.00, paper \$8.95.
- HOYT, WILLIAM GRAVES. *Lowell and Mars*. Tucson: University of Arizona Press. 1976. Pp. xv, 376. Cloth \$13.95, paper \$8.50.
- HUGHES, H. STUART. *Contemporary Europe: A History*. 4th ed. Englewood Cliffs, N.J.: Prentice-Hall. 1976. Pp. xv, 622. \$12.95.
- IGLITZIN, LYNNE B., and ROSS, RUTH, editors. *Women in the World: A Comparative Study*. (Studies in Comparative Politics, number 6.) Santa Barbara, Calif.: Clio Books. 1976. Pp. xviii, 427. Cloth \$19.75, paper \$6.50.
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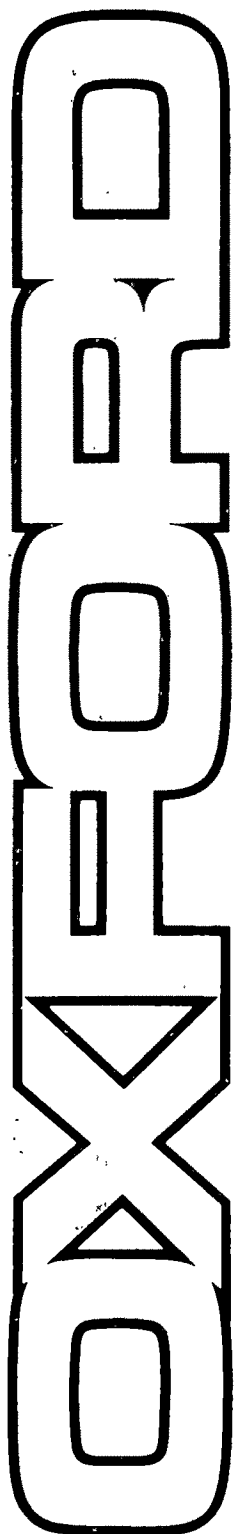
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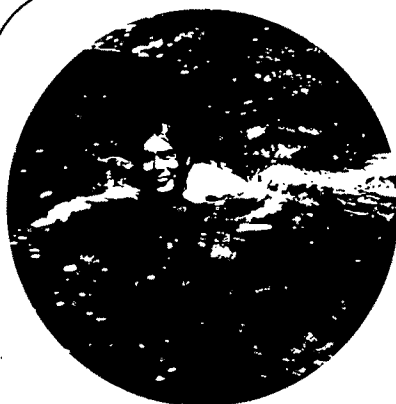
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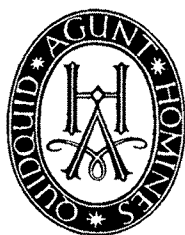
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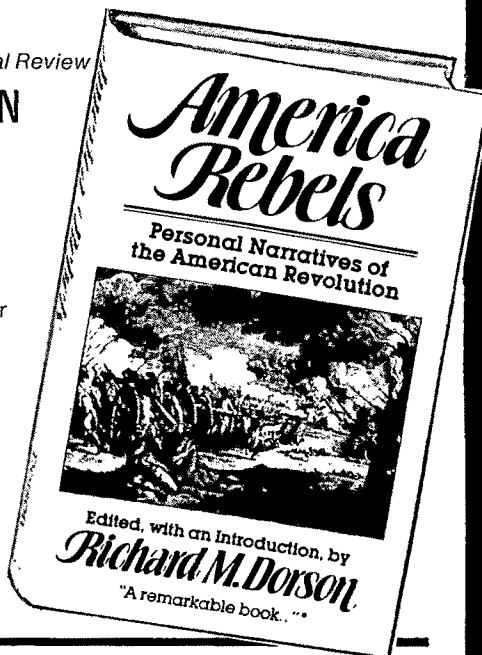
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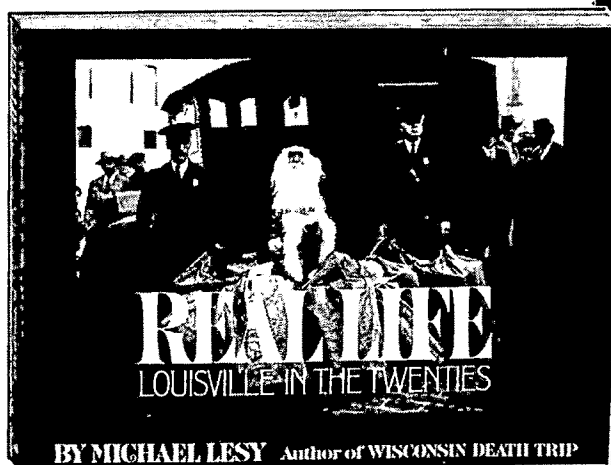


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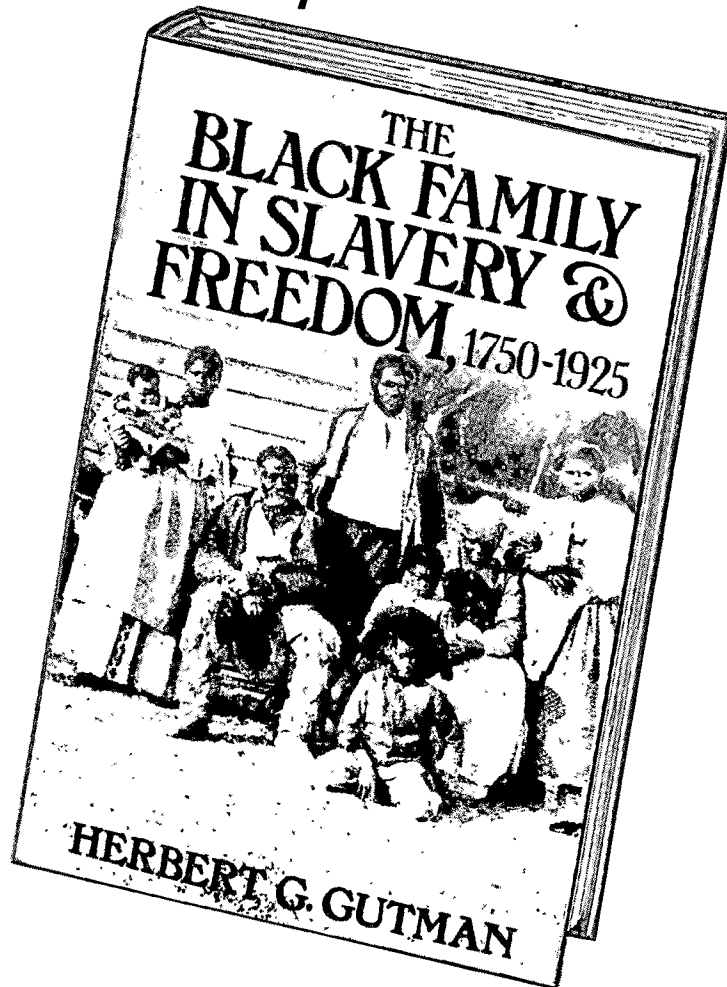
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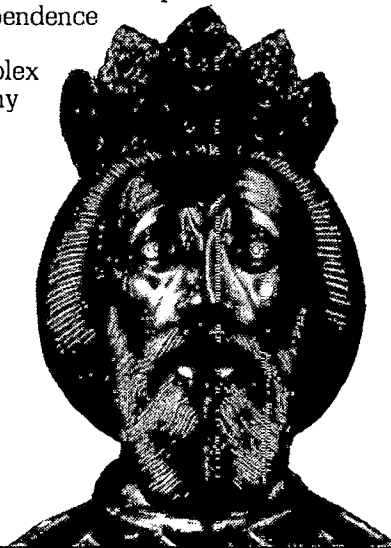
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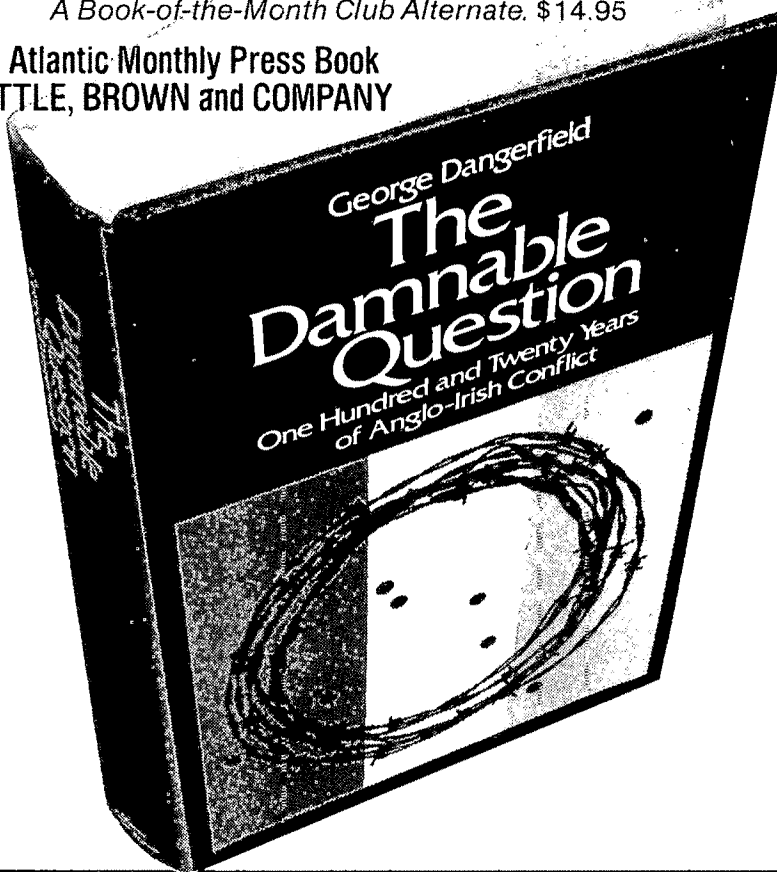
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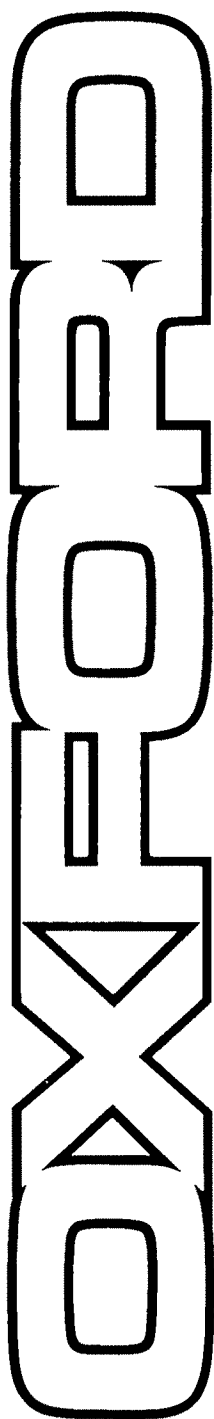
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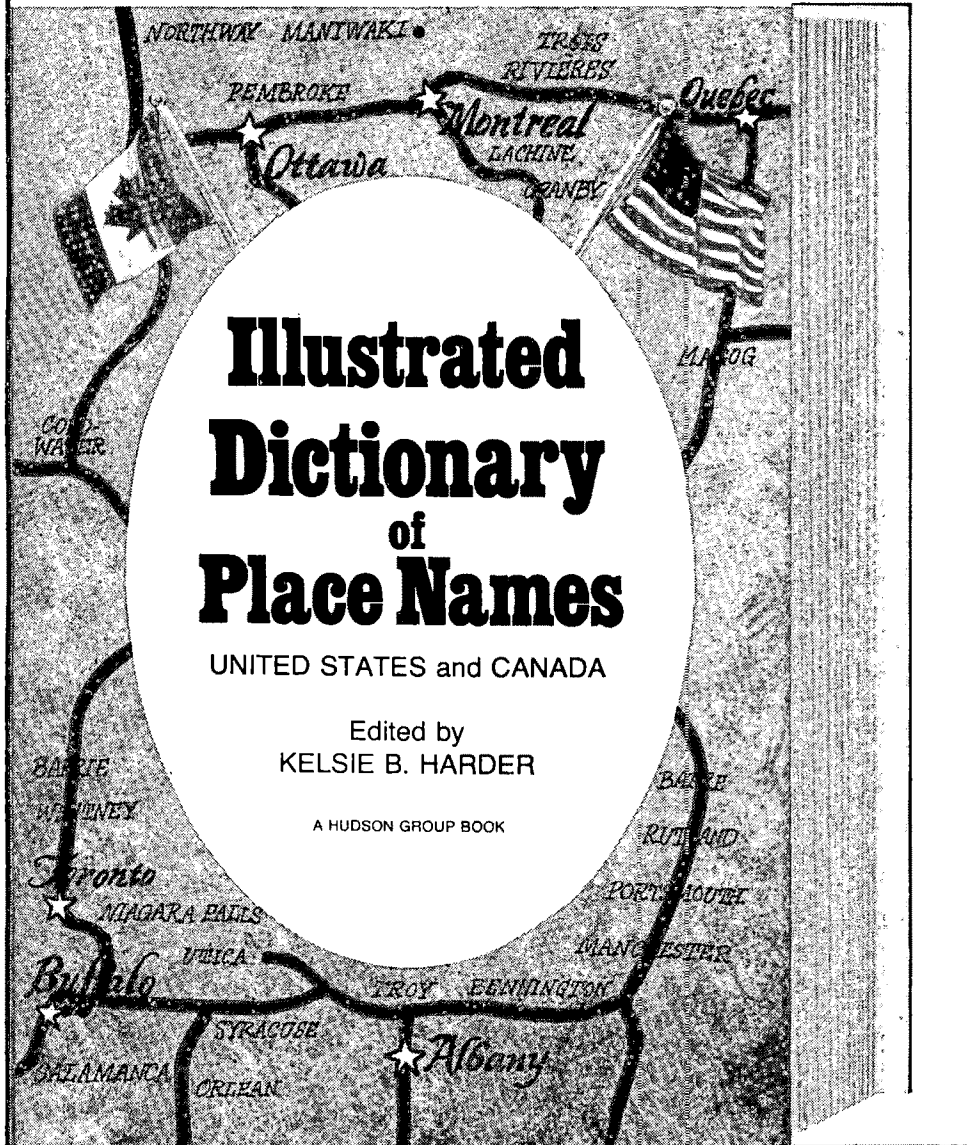
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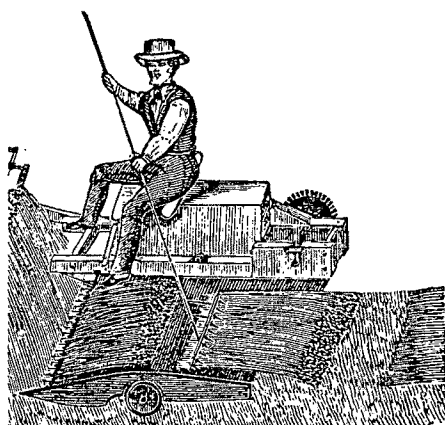
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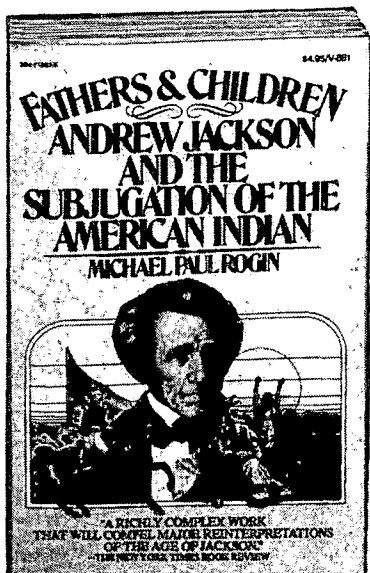
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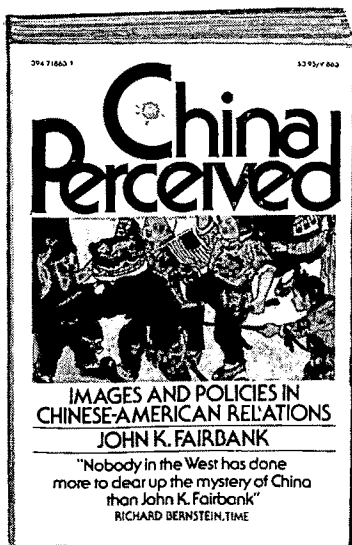
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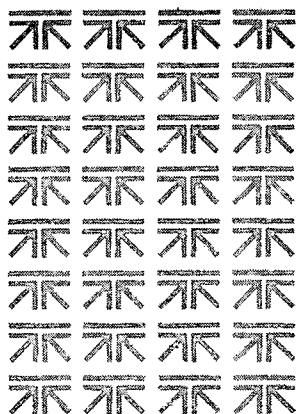
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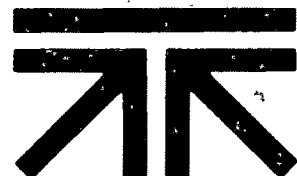
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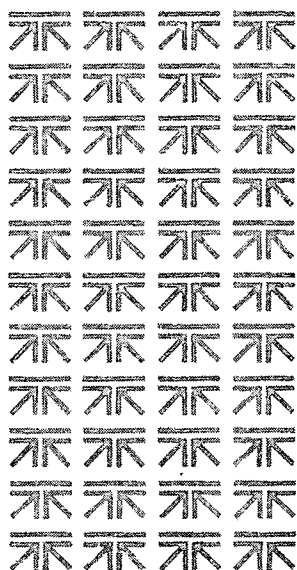


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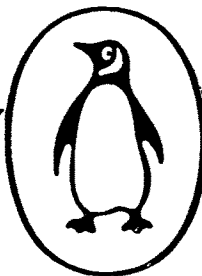
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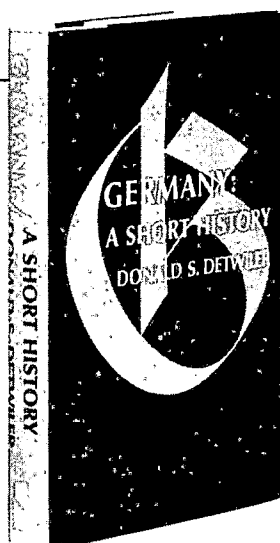
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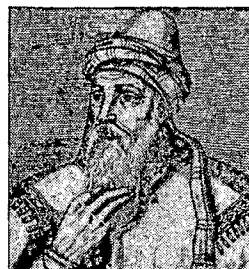
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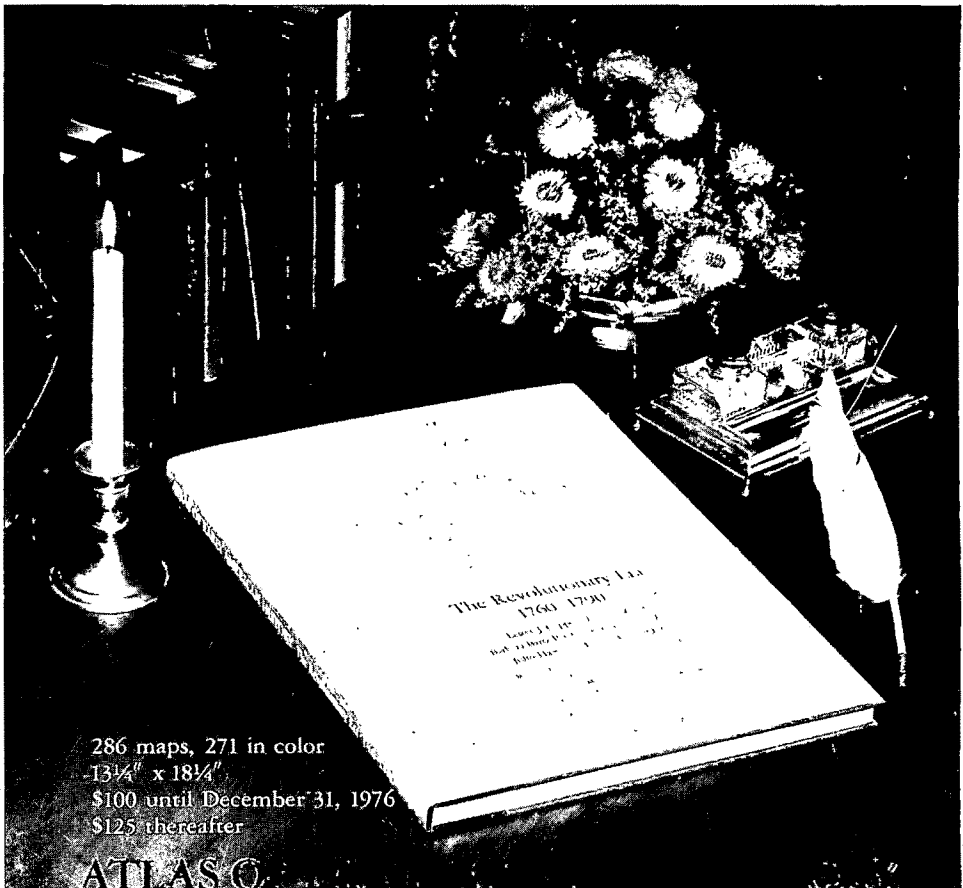
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